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TRANSACTIONS

OF THE

AMERICAN INSTITUTE OF MINING ENGINEERS.

VOL. XLVII.

CONTAINING PAPERS AND DISCUSSIONS OF THE BUTTE MEETING, AUGUST, 1913, AND THE NEW YORK MEETING, OCTOBER, 1913.

NEW YORK, N. Y.:
PUBLISHED BY THE INSTITUTE,
AT THE OFFICE OF THE SECRETARY.
1914.

Charles Rand

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PREFACE.

THIS volume is the third of the three volumes containing the papers and discussions presented at meetings of the Institute during the year 1913. Besides the papers of the New York meeting, October, 1913, its contents include those papers of the Butte meeting, August, 1913, which were omitted from Vol. XLVI on account of lack of space.

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Proceedings of the One Hundred and Sixth Meeting, New York, N. Y., October, 1913.

COMMITTEES.

Iron and Steel.

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Henry D. Hibbard, E. Gybbon Spilsbury.

Reception Committee.—J. E. Johnson, Jr., *Chairman.*

Felix A. Vogel, William R. Walker.

Dinner Committee.—E. Gybbon Spilsbury, *Chairman.*

The opening session was held on Thursday morning, Oct. 16, 1913, at the Institute Headquarters in the Engineering Societies Building, 29 West 39th Street, New York, N. Y., and was called to order by President Rand, who addressed the meeting briefly before presenting Albert Sauveur, who responded to the President's address, and presided at the meeting.

The following papers were then presented in oral abstract by their authors or authors' representatives:

The Cleaning of Blast-Furnace Gas, by W. A. Forbes. Discussed by S. K. Varnes, H. N. Diehl, F. H. Wagner, Joseph Hartshorne, S. B. Marshall, J. W. Richards, J. E. Johnson, Jr., W. A. Forbes, and others.

The Generation of Steam by Waste Heat from Furnaces, by F. Peter (Leoben, Austria).

Note on the Utilization of the Waste Heat of Regenerative Furnaces, by George C. Stone.

These two papers were discussed jointly by D. S. Jacobus, E. B. Carter, T. T. Read, J. W. Richards, J. Hartshorne, and G. C. Stone.

Over-Oxidation of Steel, by W. R. Shimer and F. O. Kichline. Discussed by Allerton S. Cushman, J. E. Johnson, Jr., H. D. Hibbard, H. M. Howe, P. H. Griffin, A. Sauveur, and J. W. Richards.

The Slagging Gas Producer, by W. H. Blauvelt, was presented by title and discussed by J. W. Richards orally.

The second session was called to order at 2 p.m, with Joseph W. Richards acting as Chairman.

The following papers were presented in oral abstract by their authors, or authors' representatives:

The Use of Pulverized Coal as a Fuel for Metallurgical Furnaces, by H. R. Barnhurst. Discussed by W. A. Evans, J. W. Richards, H. A. Prosser, T. T. Read, P. H. Griffin, A. Stansfield, K. Nibecker, and H. R. Barnhurst.

The Scoria Process for the Manufacture of Fine-Ore Briquettes, Flue-Dust Briquettes, and Slag Brick for Building Purposes, by E. Stütz.

The Briquetting of Flue Dust in the United States by the Schumacher Process, by F. A. Vogel and A. M. Tweedy.

These two papers were discussed jointly by H. O. Hofman, J. W. Richards, F. A. Vogel, and E. Stütz.

The Use of Nodulized Ore in the Blast Furnace, by R. H. Lee. Discussed by J. E. Johnson, Jr., H. M. Howe, and J. W. Richards.

The third session was called to order on the morning of Friday, Oct. 17, 1913, at 10 a.m. Albert Sauveur was Chairman.

The following papers were presented in oral abstract by their authors, or authors' representatives:

Determination of the Position of Ae3 in Carbon-Iron Alloys, by H. M. Howe and A. G. Levy.

Thermal and Microscopical Examination of Professor Howe's Standard Commercial Steels, by G. K. Burgess, J. J. Crowe, and H. S. Rawdon.

Discussion of the Existing Data as to the Position of Ae3, by H. M. Howe.

These three papers were discussed jointly by Alfred Stansfield, G. K. Burgess, K. W. Zimmerschied, R. H. Sweetser, Albert Sauveur, H. LeChatelier, and H. M. Howe.

The Critical Ranges A2 and A3 of Pure Iron, by G. K. Burgess and J. J. Crowe. Discussed by H. M. Howe, Alfred Stansfield, A. A. Stevenson, Bradley Stoughton, Albert Sauveur, P. H. Griffin, R. W. Hunt, and G. K. Burgess.

The following papers were presented by title:

Ae 1, The Equilibrium Temperature of A 1 in Carbon Steel, by H. M. Howe.

A Chart for Use in Connection with Wet and Dry Bulb Thermometers in Making Psychrometric Determinations, by C. P. Linville.

The fourth and last session was called to order by R. W. Raymond at 2 p.m. on Friday afternoon, Oct. 17, 1913.

The following papers were presented in oral abstract by their authors, or authors' representatives:

The Influence of Various Elements on the Absorption of Carbon by Steel, by R. R. Abbott. Discussed by J. A. Mathews, Albert Sauveur, and M. A. Ammon.

Shock Tests of Cast Steel, by J. H. Hall. Discussed by H. M. Howe, W. C. Chancellor, W. R. Webster, Leonard Waldo, J. E. Johnson, Jr., Bradley Stoughton, and William Campbell.

The Influence of Copper upon the Physical Properties of Steel, by G. H. Clevenger and Bhupendranath Ray. Discussed by D. A. Lyon, A. S. Cushman, and R. W. Hunt.

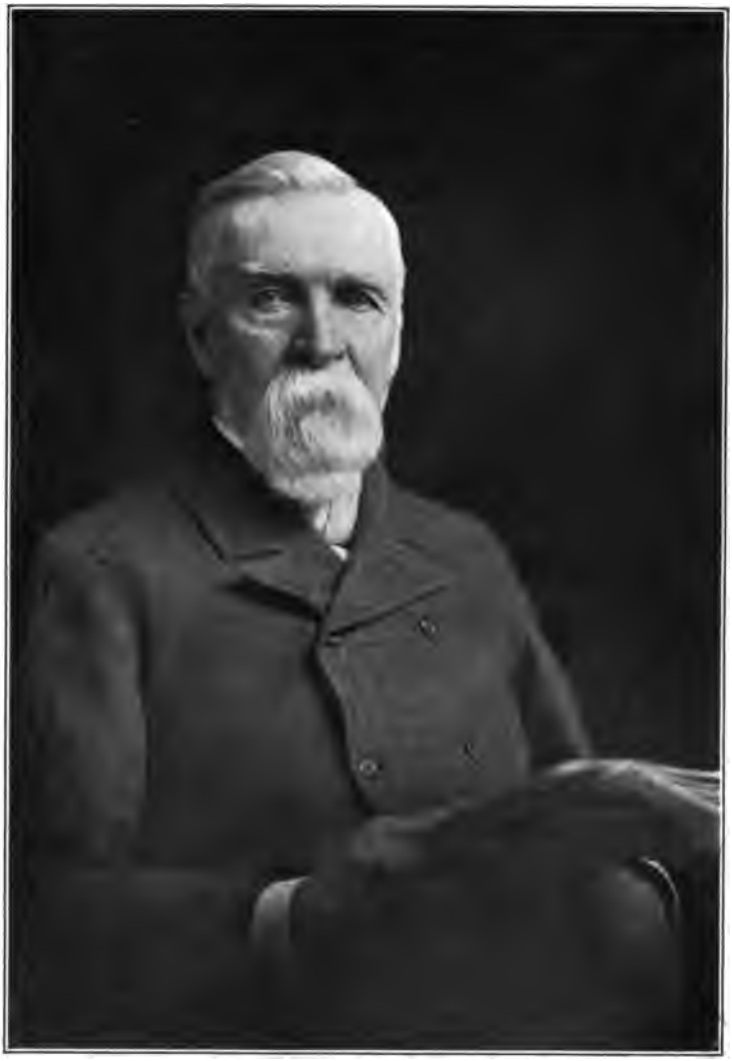
The Life of Crucible Steel Furnaces, by J. H. Hall.

Grain Growth in Silicon Steel, by W. E. Ruder. Discussed by H. O. Hofman, P. H. Griffin, A. S. Cushman, H. M. Howe, and W. E. Ruder.

Robert W. Hunt then presented a resolution urging upon the proper national government committees the supreme importance of proper financial and other support to the Bureau of Standards and also requesting allied kindred American scientific and commercial organizations to act in this matter.

The meeting closed with an informal dinner at the Engineers' Club, at which Mr. Rand presided. Informal toasts were responded to impromptu by about 20 of the persons present. The attendance at the meeting was 151 members and 40 guests.

PAPERS.



J. Fritz

Biographical Notice of John Fritz.

BY ROSSITER W. RAYMOND, NEW YORK, N. Y., AND HENRY STURGIS DRINKER, SOUTH BETHLEHEM, PA.

(Butte Meeting, August, 1913.)

ON Mar. 28, 1913, the Board of Directors of the American Institute of Mining Engineers unanimously adopted the following Minute:

JOHN FRITZ, one of the most distinguished of American mechanical and metallurgical engineers, won that position by the force of innate genius, indomitable industry, unstained integrity and unfailing sympathy, and generosity towards his fellow-men.

Self-educated in the hard school of practice, he appreciated nevertheless the advantages of technical instruction and discussion, and evinced this appreciation both by his membership and lively interest in this and other similar societies, and by his munificent gifts to engineering education at Lehigh University, and his long and faithful service as a Trustee of that institution.

As one of the foremost of those American engineers who, through their brilliant inventions and practical skill, developed here the modern iron blast-furnace and rolling-mill, and introduced and perfected the Bessemer process and other improvements in the manufacture of steel, Mr. Fritz contributed mightily to the chief departments of that industrial progress which characterized the Nineteenth Century.

Proud of his great achievements, we cannot but rejoice over his long and fruitful life, crowned with a peaceful death; but our praise and thanks are mingled with sorrow, as we recall the kindly face which we shall see no more on earth, and the loyal friendship and spontaneous good-will which led the love of his generation, and the reverence of the generation which followed, to regard him universally as "Uncle John Fritz."

That summary, though only an outline, is a good portrait; and in the present sketch, we shall do no more than supply some details of the likeness, and some elements of the background.

John Fritz was born Aug. 21, 1822, in Londonderry, Chester county, Pa. His father, George Fritz, a native of Hesse Cassel, was brought to this country by his parents in 1802, with three brothers and a sister, to whom were subsequently added three daughters born in America. The family settled in Pennsylvania, and "grew up with the country." George Fritz married in 1821 the native-born daughter of a Scotch-Irish Presbyterian immigrant of 1787, and they had seven children—four girls and three boys, of whom John was the first. He was named after his grandfather, the foreign form, Johannes Fritzius, being modernized and Americanized into John Fritz. Thus he was descended from stanch and sturdy stock on both sides. His ancestors came here at a period when faith in the new Republic and the future

development of its domain under free institutions, brought to its shores the bravest and most enterprising of pioneers. It was the era of dauntless, independent individualism, and it produced among us a generation of strong men, whose personal gifts and ambitions could be developed freely in the stimulating atmosphere of liberty and opportunity.

The *Autobiography of John Fritz*, published in the autumn of 1911, bears unconscious testimony to the effect of this environment upon innate genius. His father, a mill-wright and mechanic, could not be content with farming, but repeatedly followed the call of the trade which he loved better; and the three sons, inheriting his talent and his predilection, after dutifully following the plough in their youth, abandoned it for the pursuit of mechanical engineering, in which, educating themselves without the aid of technical schooling, they all achieved high position.

Another influence, not to be overlooked, was that of the large family, with its necessary development of mutual affection and happiness. It was a sad thing for John Fritz, brought up in such an atmosphere, that to him and his beloved wife, during their long life together, only one child was given—a daughter, who died at the age of seven; but it may be fairly imagined that this experience had something to do with the fatherly and brotherly affection which he lavished upon the sons of others. If he had had, like his father, many children of his own, perhaps there would not now be so many to call him gratefully "Uncle John Fritz!"

It should be added that both his ancestry and his early life endowed him with splendid health and strength, so that his body was at all times able to respond to the demands of his spirit.

Finally, we cannot omit to mention (what John Fritz was wont, on all occasions, to emphasize) the moral influence of his God-fearing father and mother upon his whole life. Under that influence, added to all the rest, he became the strong, gentle, simple-hearted, high-souled man we knew and loved, combining with his own inborn genius the warm Irish heart, the steady German head, and the American courage and elasticity of endeavor.

Like other American boys, he had the benefit of some schooling; but his own epigrammatic summary, "Five days in the week, for three months in the year, is too short a time for the study of Bennett's Arithmetic," tells the whole story. The schooling of those days could only show the door, and give the key to those who would enter. Perhaps, after all, our modern systems accomplish little more!

In 1838, at the age of sixteen, he became an apprentice in the

trades of blacksmith and machinist—the latter comprising repairs of agricultural and manufacturing machinery, including the simple blast-furnaces of that day. The old apprentice-system, applied to such miscellaneous work, has largely passed away; but some of us veterans can testify that it turned out the best all-round mechanics we have ever known. Above all, it gave to the apprentice himself the opportunity to find and follow the line for which he was best adapted. That is what it did for John Fritz, who, at the end of his apprenticeship, returned to work for a time on the paternal farm, with his mind made up to engage somehow in the manufacture of iron, with special relation to its use on railroads. This early decision was illustriously justified by his subsequent career.

It was not until 1844 that he succeeded in making an entrance upon this career, by getting employment in a rolling-mill at Norristown, Pa., then in process of erection. After it started, he was put in charge of all the machinery, and soon discovered by annoying experience many weak spots in design and construction which he long afterwards remedied either by his own inventions or by those which he adopted and introduced. Among these were the two-high rolls and their cog-gearing, which he determined to abolish, if he ever got a chance. But meanwhile, not wasting his time in visions of future achievements, he seized the present opportunity to master thoroughly the thing nearest to him, outside of his immediate task. This happened to be the puddling-furnace. Many ambitious young mechanics have attended night-school after a day of hard work; but few have done so when the curriculum meant another shift of manual labor. John Fritz worked through a long day at his job as superintendent and repairer of machinery, and then spent the evening in the exhausting work of a common puddler, studying, while he rabbled or drew the glowing charge, the apparatus and the process. Months of such toil and thought made him at last not only a master-puddler, but also an expert, qualified to improve the old construction and practice. This accomplishment, however, he merely stored for the time when he should be able to use it, and, meanwhile, turned his attention to the heating, rolling, and finishing departments of the mill, with each of which, by the same method of actual practice at night, he acquired a similarly thorough familiarity. It is needless to say that, before long, John Fritz was the superintendent of the whole works, with the hearty support, not only of the proprietors, who knew his value, but also of the workmen, whose comrade and friend he had been—as, to the end of his life, he continued to be.

But, having learned what was to be learned in that particular busi-

ness, he accepted in 1849, with the sympathetic approval of Messrs. Moore and Hooven, his employers at Norristown, a position in connection with a new rail-mill and blast-furnace, erected at Safe Harbor, Pa., by Messrs. Reeves, Abbott & Co. The salary was smaller ($650 a year, instead of $1,000!); but he wanted to learn all about blast-furnace practice and the manufacture of rails. His strenuous and successful work at Safe Harbor was cut short after a few months by an attack of fever and ague, which forced him to give up, for a time, all active labor. During this interval, he made a trip to Lake Superior, and saw with wondering eyes the great Cleveland and Jackson iron-ore deposits in the Marquette district. After his return, he tried in vain to interest Pennsylvania capitalists in Lake Superior iron-mines, as a source of supply even for Pennsylvania. He was told that he might as well dream of bringing iron-ore from Kamschatka as from Marquette—to which he replied that, within ten years (this was in 1852), iron-ore from Lake Superior would be sold in Philadelphia. One-half the Jackson mine could have been bought then for $25,000!

But if his friends and former employers could not trust him as a prophet, they appreciated his character and experience as a mechanical engineer; and he was engaged in 1852 to superintend the rebuilding and improvement of the Kunzie blast-furnace, on the Schuylkill, about 12 miles from Philadelphia. This involved the new method of manufacturing pig-iron with anthracite, instead of charcoal or coke, as fuel —a scheme which had just been proved practicable by David Thomas and William Firmstone[1] in the Lehigh Valley. Mr. Fritz, though not the designer of the new furnace, was soon called upon to remedy defects in the original design, and managed to do so to the satisfaction of the proprietors, and without losing the friendship of the engineer whose opinion he had contradicted. After the furnace had been put in blast, and was running successfully, his desire to learn all about operation, as well as construction, led him to pursue his old habit of prowling about at odd times, day and night; and in this way he discovered, "by accident," as he says—but such accidents do not happen to everybody!—one of the most important principles of modern blast-furnace practice, namely, that of the "closed front," replacing the old fore-hearth and those frequent interruptions of the blast for cleaning out the crucible, known as "working" the furnace. This was, at the time, a revolutionary change of practice. The principle was afterwards embodied and made more effective by the water-cooled cinder-notch patented by Lürmann, and now universally employed. But,

[1] See *Trans.*, iii., 152, and xxix., 901.

while Mr. Fritz cannot be said to have anticipated that invention, as a means of facilitating the purpose in view, he was apparently the first, in this country at least, to recognize the importance of that purpose, and to carry it out in another way. When Lürmann's agent was trying to introduce his improvement in this country, the favorable opinion of John Fritz was one of the strongest arguments at his command.

In 1853, having got the Kunzie furnace machinery into good running order, and learned what was to be learned about the operation of the furnace itself, Mr. Fritz joined with his brother George and others in building at Catasauqua a foundry and machine-shop to supply blast-furnaces and rolling-mills. But in the following year he was invited, through David Reeves, who, as a proprietor both at Safe Harbor and at Kunzie Furnace, had learned to trust him, to go to the Cambria Iron Works, Johnstown, Pa., as General Superintendent.

This may be regarded as the turning-point of his career. His preparation for it had occupied sixteen years, during which he had mastered every part of the manufacture of iron into commercial forms—the blast-furnace, the foundry, the puddling-furnace, the heating-furnace, and the rolling-mill—while he had also learned the higher art of commanding the enthusiastic loyalty of workmen, and the highest art of all, perhaps—that of securing the confidence of employers.

All these patiently acquired qualifications were immediately demanded and tested in his new position, and the lack of any one of them would have been probably fatal to his success. The Cambria Iron Company was in a bad way administratively, financially, mechanically, and metallurgically, although, to his hopeful vision, "Cambria was destined to be the greatest rail-plant in the world." He had to meet successively the problems of technical authority and responsibility, temporary repair and reform of an old plant, improvement in quality of product, and the procurement of means for new and needed construction. When these problems had been so far solved that the mill "was running about as well as could be expected, and making some money,"[2] the property was attached under judgments upon former claims. Fritz persuaded all parties to allow the work to go on, and Fritz was the only man upon whom all parties could agree as an agent to protect the rights of all. As he says:

"Every evening, after the day's work was finished, I received the rails, first to secure the pay of the men, and secondly, in the name of the railroad company (the purchaser), to see that it got them."[3]

[2] *Autobiography of John Fritz*, p. 101.
[3] *Autobiography*, p. 103.

Under this *modus vivendi*, operations went on under the shadow of impending bankruptcy, until a reorganization with adequate capital was decided upon. This was not easily effected, under the complicated and discouraging financial circumstances of the company; and it is fair to say that confidence in the technical ability, good judgment, integrity, and loyalty of John Fritz, on the part of capitalists who knew him and his record, was the influence which turned the scale in favor of the enterprise. The capital was subscribed, and operations were resumed. But Fritz's troubles were not over. He was determined to put into the works, at the first opportunity, a three-high roll-train, in accordance with his prophetic vision of earlier years; and this plan was vehemently opposed by many of the stockholders, who were supported in their position by the opinions of leading iron-masters in all parts of the country, and the declarations of the laboring "heaters" and "rollers," who were, on principle, hostile to anything new. Fritz had to overcome this double antagonism; and it was by the sheer force of personal character that he secured, at last, authority for the execution of his plan. Against the denunciation of critics and the warning of friends, he introduced the three-high rolls into the Cambria Company's mill, laying thereby the foundation not only of unexampled prosperity for that establishment, but also of an improvement which was rapidly adopted throughout this country and the world, and has been justly called the last great step of progress in iron-manufacture preceding the Bessemer process.

But this triumph was followed by further trials. The day after the success of the three-high rolls had been demonstrated in the Cambria mill, the mill itself was destroyed by fire. Fortunately, the demonstration had been conclusive, so that, if the fire was the work of an enemy, it came too late to defeat the new invention. Fritz was equal to the emergency. Inside of thirty days, he had the mill running again, though without a roof to cover it; and it was one of the proudest recollections of his after-life that he subsequently erected a building 1,000 ft. long by 100 ft. wide, with trussed and slated roof—the finest rolling-mill building, at that time, in the United States—without interrupting the running of the mill which it covered, and without injury to a single person. He says of this work, finished in December, 1857:

"I was on the building while every member of every truss was being raised and put in place. This was the most trying ordeal that I ever had. The falling of a member of the truss, or a bolt, nut, wrench or tool of any kind, striking a man on the head, would cause instant death; and no person but myself knew what a load was off my mind when the last truss was in place, and I came down off the building for the last time."[4]

[4] *Autobiography*, p. 120.

It goes without saying that, in the progressive reconstruction of the Cambria works, Fritz introduced many improvements which he had conceived in previous years, when as yet there was no opportunity to realize them—improvements in puddling-furnaces, gearing, boilers, etc. One of his most characteristic and radical measures was the abandonment, in connection with the roll-trains, of light coupling-boxes and spindles, and a special "breaking-box," holding the rolls in place—all of which were intended to break under special strain, so as to save the rolls from fracture. The continual breaking of these weak parts was to him a source of greater delay and loss than was likely to occur in their absence. He said he "would rather have a grand smash up once in a while" than be thus annoyed all the time! And this remained his guiding principle as a mechanical engineer throughout his life. The structures and machines designed by him have been occasionally criticized, from the standpoint of theory, as unnecessarily costly at the outset; but, so far as we know, none of them ever failed in service. His trusses are still standing; his engines are still running; and perhaps his abundant "margins of safety" have proved to be worth more than they cost.

After six years of continuous hard work with the Cambria Iron Company, Mr. Fritz accepted in July, 1860, the position of General Superintendent and Chief Engineer of the Bethlehem Iron Company.

The works of this company, designed and erected by Mr. Fritz, were so far completed by September, 1868, as to begin the rolling of rails made from the product of its own blast and puddling furnaces. It is impossible, as well as unnecessary, to narrate here the history of his connection with this enterprise. A few features of it, however, deserve mention, by reason of their relation to the general progress of iron-metallurgy.

The first of these was the introduction of high-pressure blast in the iron blast-furnace. The iron-masters of the Lehigh Valley region were startled indeed, when they learned that Fritz was blowing air at 12 lb. per sq. in. into his furnaces, and was prepared even to blow at 16 lb. in an emergency. This method of overcoming by main force the internal difficulties which had previously been treated with so much old-fashioned skill, was the beginning of the new blast-furnace practice, in which rapid running, immense product and high blast, while creating fresh problems of blast-furnace management, have superseded many of the old ones.

Fritz's horizontal blowing-engines were much criticized at the time; but they have run continuously, day and night, for more than thirty years, blowing at from 10 to 12 lb. pressure, and frequently

more. He was so well satisfied with the result of his innovations in blast-furnace practice that he designed a larger furnace, with an engine that would supply a 20 to 30 lb. blast. But, to his great regret, the directors of the company were too conservative to authorize this experiment.

During the Civil War, the Government needed a rolling-mill somewhere in the South, in which rails torn from the track, twisted and deformed by Confederate raiders, could be re-rolled for renewed use. It was probably the advice of Abram S. Hewitt, whose patriotic services and wise counsel as an iron-master were so highly valued by Mr. Lincoln, which led to the selection of Mr. Fritz as one who could procure the necessary machinery and secure the erection of the mill with the least possible delay. He was surprised in March, 1864, by his appointment to this place with almost unlimited powers. His commission under the War Department declared that "any arrangements" he might make would be "fully carried out" by the Government, and continued:

"The early completion of this mill is important and indispensable to the advance of the Army; and all persons who may engage to furnish machinery or material therefor are directed to do so, to the exclusion of all other business!"

Mr. Fritz immediately prepared the plans and secured the necessary machinery for the mill, which was built at Chattanooga, Tenn., and of which his brother William was made Superintendent. William Fritz had been employed at Cambria and at Bethlehem until 1861, when he enlisted in the Union Army, and in 1864, he was on furlough, recovering from a serious wound. He ran the Chattanooga mill successfully until the end of the war.

The part taken by John Fritz at the Bethlehem works in the application and improvement of the Bessemer process in this country, was no small one. He was one of a notable group, comprising his brother George Fritz, then Superintendent of the Cambria Works, Robert W. Hunt, William Jones, and Alexander L. Holley, which used to meet frequently for the discussion of serious practical difficulties not communicated to the general public, or even to the technical societies and journals. He said afterwards of these conferences:

"We did not meet as diplomats, to find out what each other wanted; but we met as a band of loving brother engineers, trained by arduous experience, young, able, energetic, and determined to make a success. I doubt if ever five natural brothers were more loyal to each other than the five brother engineers above named. What each of us knew was common to all. Upon one occasion we all met at my house and talked over our troubles in detail; and they seemed so grave that some of us doubted if we could ever make the Bessemer process a financial success."

It is worthy of notice that these young engineers were all rail-makers; and it was in the manufacture of rails, more than in any other department, that the Bessemer process produced its widest and deepest effect throughout the civilized world, by its revolutionary improvement of the conditions, distances, speed, and economy of transportation. The troubles of which Mr. Fritz speaks in the foregoing quotation were those encountered in making good steel rails; and the chemists, physicists, and metallurgists would never have solved them without the aid of the practical rail-makers, of whom John Fritz was a leader and type.

During nearly thirty years of work with the Bethlehem Iron Co., Mr. Fritz, supported by the faith and courage which he inspired in other men, made that enterprise one of the most famous in the world —the Mecca of engineer-pilgrims from abroad and the pride and pattern of American practice. The introduction of open-hearth furnaces and of the Thomas basic process; the progressive improvements of strength, simplicity, and automatic handling in the rolling-mills; the adoption of the Whitworth forging-press; the manufacture of armor-plate; the erection of a 125-ton steam-hammer; and innumerable other improvements in the manufacture of iron and steel, owe their present perfection in large degree to his inventive genius, practical resourcefulness, and patient study. The stamp of his mind may be found on almost every detail of construction and operation throughout a wide range of processes and products.

In 1893, at the age of seventy-one, he retired from the responsible and arduous work at Bethlehem, which had occupied more than the latter half of the fifty-four years since his apprenticeship began. For nearly twenty years longer he lived to enjoy, as few men have been permitted to do, the fame and the friendships which he had amply earned. Indeed, he had received world-wide recognition before his retirement, and that event elicited numerous public expressions of the pre-existing fact. This Institute, of which he had been a loyal member since 1872, elected him its President in 1894;[5] the American Society of Mechanical Engineers, which he had joined in 1882, made him an Honorary Member in 1892, and President in 1895; the American Society of Civil Engineers, of which he became a member in 1893, conferred honorary membership upon him in 1899; the Iron and Steel

[5] Mr. Fritz made the following contributions to the *Transactions:*

Title.	Volume.	Year.
Remarks on the Fracture of Steel Rails	III.	1873.
Remarks on the Bessemer Process	XIX.	1890.
Early Days of the Iron Manufacture (Presidential Address)	XXIV.	1894.
Remarks on Rail-Sections	XXIX.	1899.

Institute of Great Britain made him an Honorary Member in 1893, and a perpetual Honorary Vice-President in 1909; and the recently organized American Iron and Steel Institute elected him an Honorary Member in 1910. Meanwhile, he had received the Bronze Medal of the U. S. Centennial Exposition in 1876; in 1893 the Bessemer Gold Medal of the Iron and Steel Institute; in 1902 the John Fritz Medal (the fund for which was established by subscription, to honor his eightieth birthday, by awarding a gold medal annually "for notable scientific or industrial achievement"—the first medal being bestowed with enthusiastic unanimity upon John Fritz himself); in 1904 the Bronze Medal of the Louisiana Purchase Exposition, in connection with which he served as Honorary Expert on Iron and Steel; and in 1910, the Elliott Cresson Gold Medal of the Franklin Institute of Philadelphia, "for distinguished leading and directive work in the advancement of the iron and steel industries." And he received *honoris causa* the following academic degrees: Master of Arts, from Columbia University, in 1895; Doctor of Science, from the University of Pennsylvania, in 1906; Doctor of Engineering, from the Stevens Institute of Technology, in 1907; and Doctor of Science, from Temple University, in 1910.

But these official distinctions could not tell fully the story of love and praise which pressed for the utterance which it found on two memorable occasions—celebrations of his seventieth and eightieth birthday anniversaries, in which hundreds of his friends and professional colleagues participated. The first took place at Bethlehem in 1892, and the second at New York in 1902. On the latter occasion, as has been said above, he received the first "John Fritz medal." Both are reported in the Appendix to his *Autobiography*, and need not be described here.

The conferment of honorary degrees by institutions of learning upon this self-educated workingman was a recognition not merely of his professional achievements, but also of his wise and generous aid to the cause of technical education, some account of which may fitly close this story of his life.

Lehigh University, located in the Lehigh Valley of Pennsylvania, was founded in 1866 by a Pennsylvanian—Asa Packer, who knew and appreciated the great qualities of John Fritz, and who named him as one of the original Board of Trustees. Established nearly half a century ago, at the period when men were beginning to feel that the material development of our natural resources and the extension of our transportation-facilities called for professional training as thorough as that previously given to the so-called "learned professions," this

institution had in its Board of Control, from the beginning, the strong common sense and the superlative engineering ability of John Fritz. For a wholly self-educated, self-cultured man, he was remarkably broad in his conceptions of education. Only a few years before his death, in commenting to a friend on the antagonism manifested by another distinguished iron-master to all forms of classical training, Mr. Fritz said:

"I think a well-educated man ought to know something of Greek and Latin. If I had a son I would see that he had some knowledge of those languages in addition to his more practical studies."

This from a man who, but for his natural culture and natural clarity of vision, might have been expected to look down on all but so-called "practical" education!

While not a wealthy man in the modern millionaire-sense of the term, Mr. Fritz, who though generous was thrifty, had laid aside and enjoyed a comfortable competence in his old age; and one day in the spring of 1909 he astonished President Drinker of Lehigh by saying,

"In my will I have left Lehigh University a certain sum of money to be expended in your discretion. I now intend to revoke that bequest, and instead of leaving money for you to spend after I am gone, I'm going to have the fun of spending it with you and Charley Taylor" (Mr. Taylor being a co-Trustee of Lehigh with Mr. Fritz, and an old and valued friend—a former partner of Andrew Carnegie). "I have long watched the career of a number of Lehigh graduates, and I have been impressed by the value of the training they have received at Lehigh. But you need an up-to-date engineering laboratory, and I intend to build one for you." [6]

Mr. Fritz acted as his own architect; designed the building (substantially on the lines of the large shop he had built at the Bethlehem Steel Works); selected, purchased and installed the superb testing-equipment; and renewed his youth in the task, which was a great pleasure to him. At his death, it was found that (after making generous provision for his near relatives, and for bequests to the Free Library of the Bethlehems, to St. Luke's Hospital at South Bethlehem, to Temple College at Philadelphia, to the Methodist Hospital at Philadelphia, to the American University at Washington, and to other charitable purposes) he had bequeathed his residuary estate, estimated to amount to about $150,000, to Lehigh University, as an endowment-fund for the maintenance and operation of this Laboratory.

Mr. Fritz retained much of his vigor and activity up to the autumn of 1911. He took frequent trips alone to Philadelphia and New

[6] *Autobiography of John Fritz.* Account of Fritz Engineering Laboratory, p. 216.

York, and attended many gatherings of his old engineering friends and associates. In the spring of 1911, he decided, at the urgent solicitation of friends, to put into shape the notes of incidents in his life which he had been making for years. His first thought was to put this material into the hands of some competent literary man who should be employed to frame it into a biography; but he finally decided, largely on the insistence of those same friends, to undertake the task himself; and this was done during the summer of 1911 in his office at Bethlehem. The large pile of penciled notes, made in his own handwriting on yellow slips, was arranged chronologically by his nephew George A. Chandler, who, as an engineer, had had a close life-long association with Mr. Fritz; then Dr. Drinker, who was admitted to participation in the task, procured a competent stenographer; and they, with Mr. N. M. Emery, another friend, spent day after day, during the summer vacation-season, on the task. First, the crabbed desultory penciled notes were read aloud, and commented on by Mr. Fritz—every now and then with the injection of some delightful reminiscence or story—all being taken down by the stenographer, of whose presence Mr. Fritz soon became unconscious, as she was an unobtrusive, most competent little woman. As soon as this mass of matter had been typewritten, it was all read over again to Mr. Fritz, who again corrected, commented, and amplified. It was then turned over to the publishers (William H. Wiley claimed this privilege as a labor of love), and again the galley-proofs were similarly read, and the matter improved in Mr. Fritz's painstaking way. Finally the paged proofs were all read to him. The *Autobiography* was absolutely his own individual work. All that the devoted friends who were admitted to participation in its preparation did, was (as Dr. Drinker expressed it), to do the "cooly work," to perform the manual operations of authorship; the literary work, the direct forcible expression, the loving reminiscences, the jocund incidents of home- and mill-life are all the work of Mr. Fritz.

And then came the beginning of the end. This literary work finished, the laboratory built, his affairs in good order, our dear old friend began to fail. He suffered from recurring attacks of bronchitis during the autumn and winter, and finally an abscess formed on his chest, resulting from an infection from a germ of a previous severe attack of pneumonia. This abscess was opened by his physician, Dr. John H. Wilson, in February, 1912. Mr. Fritz, in his weakness, shrank from physical pain; so the spot was frozen by the application of chloride of ethyl before the knife was applied. When the patient heard the hissing of the gas, he turned languidly in bed towards Dr.

Drinker, who stood by him, and said, "Doctor, that sound reminds me of my first Bessemer blow!"

In March, 1912, his medical attendants expressed the opinion that unless he would submit to a drastic operation for the removal of pus on his chest, blood-poisoning would set in and death must soon follow; and Dr. Drinker was appealed to by the family to exert his personal influence as a friend to persuade Mr. Fritz to submit to the operation. In this he was successful; and the operation was performed April 15, 1912, by Dr. William L. Estes, Mr. Fritz's old and intimate friend, with Dr. Edward Martin, of Philadelphia, as consulting surgeon, and Dr. John H. Wilson as physician.

At this time Mr. Fritz again gave evidence of his characteristic sense of humor under any and all conditions. Every precaution was of course taken to ease the patient, and the surgeons arranged to bring from Philadelphia a special operator with apparatus to administer nitrous oxide, before subjecting him to the influence of ether. When Dr. Drinker explained this to him, Mr. Fritz said, "All right, but don't let them pull out any of my teeth"—the joke being that he had not a natural tooth left. This from a man in a state of extreme weakness, following weeks of suffering!

The operation was highly successful in averting the immediate threatened danger. Mr. Fritz wished to live; and his life was prolonged until February 13, 1913, when he died quietly, without apparent pain, passing away in sleep.

His funeral, held at Bethlehem on February 17, was attended by a large concourse of his friends; and he lies at rest in the beautiful Nisky Hill cemetery of his home town, beside his only daughter, who died in childhood, and his beloved wife.

So lived and died a great man—strong, wise, brave, invincible; a good man—simple, generous, tender and true; a loving husband; a loyal friend; a public-spirited citizen; a real philanthropist, giving "himself with his gift!" To us who miss and mourn him now, the man shines even more illustrious than the famous engineer.

Topographic Maps for the Mining Engineer.

BY E. G. WOODRUFF, WASHINGTON, D. C.

(Butte Meeting, August, 1913.)

FEW authors of treatises and papers on engineering subjects have given adequate attention to topographic maps. The statement applies especially to mining engineering in all branches. Even those who have discussed such maps have treated the subject only in a general way. Therefore it is proposed to outline in this paper a few of the uncommon yet important relationships which topographic maps and their by-product, structure-contour maps, have to modern methods of mining investigation and development when the facts depicted on such maps are properly interpreted. Topography, as suggested by the etymology of the word, means a detailed description of particular places. Written descriptions have been found less effective than the pictorial representations, therefore attempts have been made in various ways to picture the surface features of places. Lines and shading have been used, hachures drawn, and, finally, the contour topographic map. To a large extent this style of map is the result of the demands of engineers. It is designed to meet their needs far more than those of the man untrained in engineering. In fact, the average man obtains a better idea of the topography of an area from the hachure system than from contour maps. Since the maps have been made chiefly for the use of the engineer, they ought to meet his demands on the one hand, and, on the other, should be used by him to the fullest extent. Such maps, when properly made, accurately portray a portion of the earth's surface and should present the topography better than a personal examination of the area without a map could present it, because the person making such examination views only a limited portion of the surface at a time, and estimates distances only roughly with his eye. Let the engineer consider carefully the fact that in many instances the topographic map is superior to a personal examination of the surface features of a property because the map is based on careful instrumental measurements, whereas the eye gives imperfect data because it can be used merely for the estimation and

not for the measurement of distances. Such estimation is frequently unreliable, since it depends upon the engineer's physical condition. When weary in the evening, after a hard day's tramp, he views a cliff or steep slope which he may have to climb in an entirely different way than he views it when he is fresh in the morning. The long mountain slope appears different in the ascent than during the descent. It is not uncommon for the traveler approaching the steep mountain front from the plains to feel that he is gradually descending, whereas he is actually going up a gentle slope. The experience is so common that, in irrigated regions along the mountains, the feeling is expressed in the conclusion that water runs up hill. These familiar experiences will no doubt suffice to demonstrate the statement just made, that, in some ways at least, topographic maps give better information than field examination.

That engineers rely upon topographic maps is emphasized by the following table, compiled under the direction of the Chief Geographer from the record of the U. S. Geological Survey to show the great number of maps purchased annually and also the growth of their use:

Fiscal Year Ending June 30.	Maps Distributed.
1897	101,974
1898	151,950
1899	168,641
1900	342,645
1901	327,603
1902	416,301
1903	440,422
1904	475,324
1905	566,016
1906	522,936
1907	595,915
1908	474,868 (including 369,521 sold).
1909	597,361 (including 444,230 sold).
1910	593,622 (including 478,737 sold).
1911	684,129 (including 517,777 sold).
1912	658,240 (including 529,656 sold).

This table shows that more than half a million maps are now purchased annually for work in the mapped portion of the United States, which is 38 per cent of the total area. At this same rate, if all the country were mapped, the demand during the year would have been for 1,400,000 maps. Furthermore, it seems very conservative to consider that at least half of the maps purchased during the preceding year would have been used also. On this assumption—namely, that the additional maps might have been sold, and that half of those sold during the preceding year were used—we infer a potential de-

mand for the use of 2,000,000 topographic maps. This conclusion does not seem unreasonable, because as people are educated to the use of topographic maps there is a greater demand, and as contiguous areas are completed the demand increases.

Both surface and subsurface features are now represented by contour maps, the former by the topographic maps and the latter by the somewhat less known structure-contour maps. Surface work is directly related to topographic maps and only indirectly related to structure-contour maps, whereas with underground work the reverse is true. Therefore, in this paper the two kinds of maps will be treated more or less independently. Furthermore, since most mining engineers are familiar with topographic maps and their uses, this paper discusses only certain data, presented on topographic maps but generally overlooked by the engineer because he fails either to read the map properly or to use the information which it contains.

One of the common problems in both the development and the operation of a mining property is to control the water supply which is to be used, and to protect the property from any excess that is not needed. In many plants the flood water of a nearby stream is a menace. It takes years of measurements to determine the high-water stage of a stream, but a close approximation may be made in another way. With the size of the drainage basin given and the daily rainfall known, the stream's flood stage can be roughly estimated. In the United States the observations of the Weather Bureau have given us data on the rainfall for most parts of the country. The other factors, such as the size and characteristics of the drainage basin, are shown on topographic maps. In this way two of the important elements in the solution of the water problem can be obtained. But more data can be actually obtained by proper interpretation of the map, especially with regard to flood. If the slopes are steep the run-off occurs rapidly and the streams are quickly flooded, but if the slopes are gentle the flow of the water is more gradual and lower flood stages may be expected. These considerations are not theoretical, but actual. They were real to the engineers of the Burlington railroad a few years ago when tracks were washed out by floods and bridges were overflowed. The railway's engineers attacked the problem just as indicated above. Rain-gauge measurements had shown the extremes of rainfall which were to be expected in a given area in southeastern Nebraska. They knew the character of the soil, forestation, and similar factors which control the run-off. Of course, the run-off as a result of the rainfall must pass under the bridges of the railroad. These bridges must be of a certain size in order to

permit the water to flow beneath, so as to avoid washing out the grade. The engineers recognized that, with their general knowledge of conditions and with specific data on the amount of rainfall and on the size and topography of the basin to be drained, they could closely estimate the maximum amount of water which might be expected to flow under any given bridge. As previously stated, they knew the amount of rainfall, but they did not know the second factor—namely, the topography; hence the railroad went to the expense of making a topographic map of the basins which drain towards the railroad in order that they might know the second factor, and thus construct their bridges properly.

A knowledge of the topography of a drainage basin is applicable in another way. Snow slides have wiped out more than one mining camp. With the fragrance of the pine enveloping him in the balmy days of summer, when claims are usually examined, the engineer does not fully appreciate the dangers from a winter's accumulation of ice and snow above the property in question. Possibly, as stated above, his point of view cannot give him the proper relations of slopes, which might warn him of the danger Yet many of us have seen those streaks on the mountain side bared of their normal verdant cover by the avalanche and know that slides are a menace. We have seen too that, as the avalanche plunged into the narrow valley below, the width of the area devastated was less, but the destructive power increased. Do all geologists and engineers, however, know that the topographic maps present data for the proper location of a camp to prevent its destruction by snow slides? The snow must accumulate somewhere, and it can slide only on steep slopes. The size and slope of the catchment basins and the angle of slopes over which the snow will move are shown on topographic maps. Reliable results can be determined from these data.

Commonly, topographic maps are used as a general guide and accurate surveys are made subsequently as a basis for detailed work; but in many cases the second and more expensive survey is unnecessary, because the accurate location of points can be determined from data shown on topographic maps. In this work the map is used as a base and needed refinements can be added to it. It may be that the map available is drawn with 50-ft. contours, whereas a 10-ft. interval is required for the work in hand. If so, the map may be accepted and supplemental contours and other refinements may be added. Ordinarily, the geologist takes his topographic map as a good guide, and a standard map is deserving of this confidence, but even good things have their limitations. On a 25-ft. contour map the elevation

between the two lines is generally estimated. In this case the error cannot be more than 25 ft. Likewise, horizontal positions are more or less a matter of estimation within certain limits, but the engineer is constantly demanding accurate measurements, not approximations. Good topographic maps like those of the U. S. Geological Survey furnish a basis for this accurate work.

To obtain these precise data it is necessary for the engineer to repeat some of the operations of the topographer at the points where such data are desired. To do this work it has been found convenient to place the topographic sheet on a suitable-sized plane-table board, and use it as an original sheet; or, better still, to have a photolithograph printed on good drawing paper on a scale suitable for the work in hand. The engineer proceeds to the field equipped with his sheets, telescopic alidade, and stadia rod. The bench marks and triangulation stations, shown on the sheet, give him points from which he can establish his locations. This is an important feature. Here are useful data presented on topographic sheets and seldom considered, yet they furnish a basis for accurate work. They comprise some of the best data on the sheet, such as triangulation stations, houses, water towers, oil derricks, etc. Some points are shown only by bench marks, and signals should be re-established. If this is done for the engineer when he enters the field, he proceeds to the point whose location and elevation are desired and orients his table by the magnetic or three-point method, locates his point by intersections, and determines his elevation by vertical angles. After he has determined the location and elevation of his station he records the observations about the outcrop of the vein or bed. So far, the engineer has worked alone and he can continue to do so if necessary, but if he is accompanied by an assistant who can serve as a rodman, or, better still, who can operate the plane table, the engineer, serving as a rodman, traces the outcrop and holds the rod at points to be located. The plane-table operator sights to each stadia station and determines the location and elevation accurately. With these data he can plot the outcrop of a vein, if that is what he is tracing, though it is exposed intermittently, and determine its position with regard to land lines which are also shown on the sheet. Furthermore, he can obtain data from which he can determine the pitch of the vein. For the sake of illustration, a simple problem is assumed by supposing that the engineer has found three exposures of a vein, two at about equal elevations on opposite sides of a valley, and a third in the bottom of the valley, 500 ft. lower. If the three fall in a line the vein is vertical, but if the valley point falls to either side of the line joining the other

two the vein dips in the direction of that point in an amount equal to the distance of the point from a line joining the two points on the sides of the valley. In the case assumed above, if this distance of the point from the line is equivalent to 100 ft. the vein dips 100 ft. in 500, or 20 ft. to the hundred.

There are many additional applications of the data other than contours, but this discussion is not prolonged to include them.

The remaining part of this paper relates to structure-contour maps, or maps that might well be called strata-form maps because they portray the form of the strata. Their discussion is introduced in this paper because they are very closely related to surface-contour maps in their construction, interpretations, and uses. Such maps are in demand in coal fields, and especially in oil and artesian water fields, but are also applicable to some metalliferous areas. The methods of construction of such a map vary with conditions which are receiving consideration in some geologic magazines; therefore this paper begins with the assumption that the maps have been made.

These maps, illustrated in Fig. 1, show the structure of some distinctive stratum, such as a coal bed, oil sand, or any persistent bed. Contours are drawn on equal elevations of the bed just as surface contours are drawn through points of equal elevation of the surface. Synclines are depressions just as valleys are depressions, and anticlines are ridges in the strata just as mountains and hills are land ridges. To illustrate the relation of structure contours to topographic contours, let us suppose that we have strata folded into a syncline and an adjacent anticline, and that a definite unbroken stratum is the surface rock over both. Then the structure and surface contours are coincident if the same datum plane is used for both. Sea-level is generally used in the construction of both kinds of maps. The structure lines and topographic contour lines agree exactly. Now, if a portion of the anticline is eroded and the débris partly fills the valley without attendant crustal movement, the structure contours remain the same, but the surface contours are shifted. On the crest of the anticline the surface contours show lower elevations than the structure contours, whereas the reverse is shown in the valleys. In the construction of the map, however, the portion of the structure contour map representing the crest of the anticline is generally shown by broken lines to indicate that the bed does not actually remain and that its position has been assumed. In the adjustment of the assumed eroded surface the contours are shifted from their former position of coincidence to new ones, depending on the amount of erosion or deposition. If the syncline and anticline trend north-

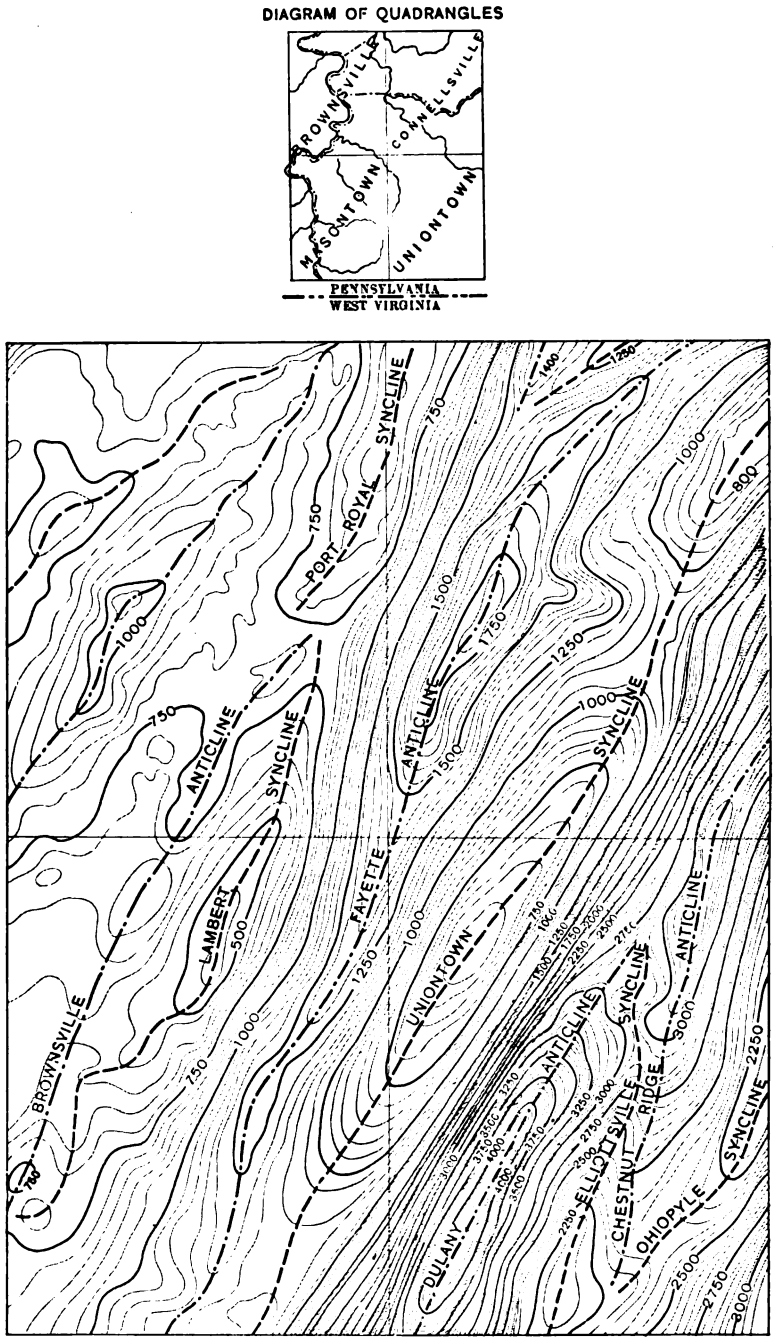


FIG. 1.—STRUCTURE-CONTOUR MAP OF FOUR QUADRANGLES IN SOUTHWESTERN PENNSYLVANIA. From *Folio No. 94, Geologic Atlas of the United States.*

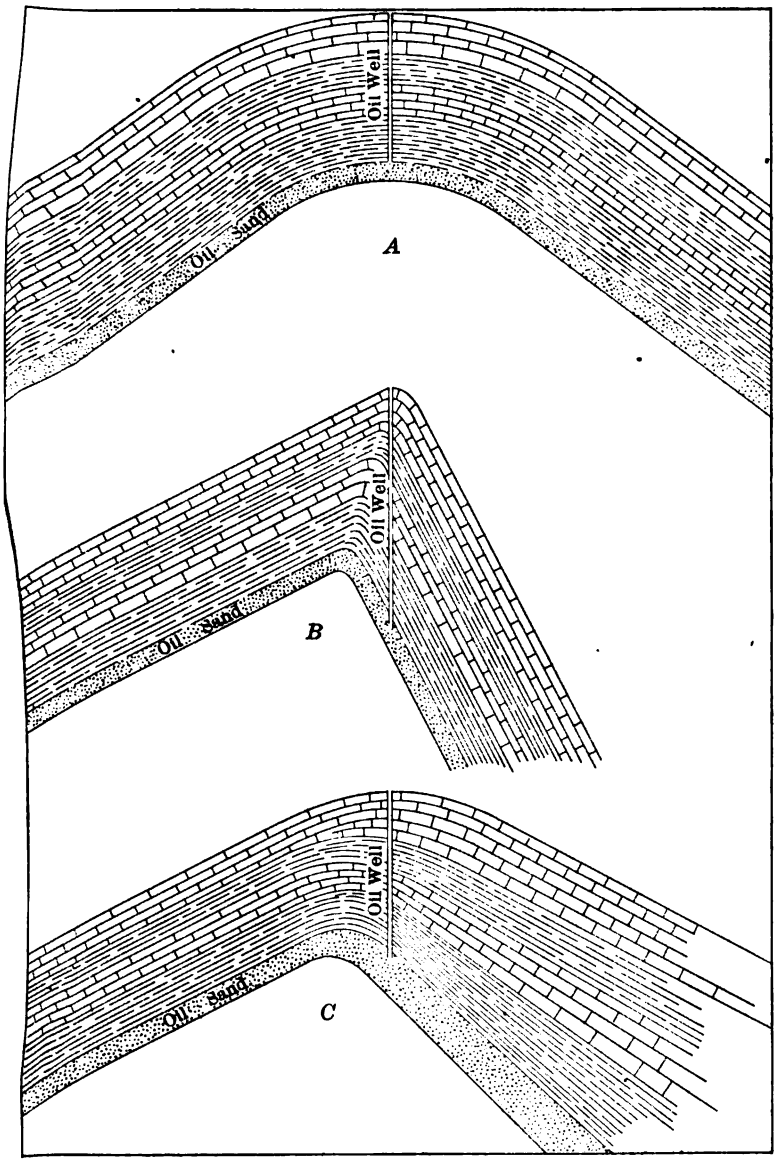


FIG. 2.—CROSS-SECTIONS OF HYPOTHETICAL ANTICLINES DRAWN TO SHOW THE RELATION OF AN OIL SAND TO THE SURFACE ROCKS.

south, the structure contours extend in the same direction; but if the syncline is filled so that the surface slopes to the north or south, the topographic contours extend east-west across the structure contours. The difference in elevation of any two lines at the point of intersection is the distance of the contoured strata from the surface. The depth at points between contour lines can be determined by interpolation. Thus, if a point lies midway between two surface contours, 2,100 and 2,150 elevation, its altitude is assumed to be 2,125, and if the same point is two-fifths the distance from the 1,150-ft. structure contour to the 1,200-ft. contour its elevation is assumed to be 1,170, and the depth of the bed below the surface is the difference between 1,170 and 2,125, or 955 ft. The uses of such maps are apparent and their accuracy is surprising. One such map, made by M. R. Campbell, of the U. S. Geological Survey, is published in the *Masontown-Uniontown Folio*, No. 82. The structure lines are drawn on 50-ft. contour intervals to indicate the position of the Pittsburg coal west of Laurel ridge, and the Upper Freeport coal east of that ridge. Diamond drilling and mining subsequent to the construction of the map have shown that the maximum error is only a few feet. Of course, it is unnecessary to point out the value of such a map to the mining engineer in studying a mining problem. Supposing he has both surface and structure-contour maps printed on one plate, as they usually are, then from the one sheet he can select the possible locations for the surface works, can infer the depth to the coal if it is a coal mine, the lowest point for drainage purposes, the slope for haulage, etc.

The construction of maps of this kind has often given surprisingly useful results. It is therefore urged that, whenever possible, the engineer dealing with such problems construct corroborative maps both for use and to reassure himself that he is correct. Two examples in which the engineer's judgment may be erroneous are given in Fig. 2 to illustrate this necessity. Supposing an oil well is to be sunk to reach the highest point in the oil sand of an anticline. If the anticline is symmetrical, as in *A*, Fig. 2, the well should be sunk upon the crest of the surface part of the anticline. If, however, the anticline is unsymmetrical, a location upon the previous supposition would be unsatisfactory, as shown at *B*. A structure map on oil sand would show the location of the highest part of the oil sand. In the construction of one such map the engineer found the structure in the immediate vicinity of possible oil location to indicate that the beds formed a fairly symmetrical anticline, but consideration of the data obtained in the surrounding area showed conclusively that the

crest of the contoured bed had been forced to one side at a depth of several hundred feet below the surface and was not directly under the crest, as the surface indications along the anticline would have led him to infer (*C*, Fig. 2). From these considerations the writer concludes that structure-contour maps are valuable to an engineer if constructed for him, but if forced to add details for himself he will receive great value from them both in making the proper field observations and in his subsequent interpretations. Of course, such work is hopeless without a topographic map; therefore, the preparation of topographic maps is advocated because of their usefulness in assisting the engineer to obtain a correct conception of the surface and also as a basis for the study of subsurface structural relations.

Discussion.

F. A. Linforth, Butte, Mont.:—I have not had an opportunity of reading this paper thoroughly, but I would like to call attention to the possible use of the contours on fault planes. It is obvious that if the contours of fault surfaces can be determined, we will be able to determine the axes of the hollows in the fault surface. It seems probable, and there are examples to point to the accuracy of the assumption, that the axes of these hollows may be the direction of movements on the faults, and if that is true it will aid materially in locating faulted ore bodies or the shoots of ore on the opposite sides of faults. A few such contour maps of faults have been attempted on some of the faults of the Butte district, and those faults which are believed to have the nearly normal movement exhibit for the axes of these hollows a line nearly up and down in the plane of the fault, but in those cases where the throw is believed to be nearly horizontal we find that the axes of the hollows are almost horizontal lines. In other words, the movements on faults, I believe, would be more nearly indicated by the larger grooves in the fault surfaces than by the small slickensides and minor hollows that are seen, so that contour maps might be valuable in geology in that way.

C. W. Goodale, Butte, Mont.:—I received a letter from Mr. Woodruff asking me to take part in the discussion of his paper, but I have not had time to give the paper any study. I can certainly add my opinion to that of all the engineers here that the topographic maps of the government are of great aid to the engineer. When I visit a new district, the first thing I do is to see if that district has been covered by the U. S. Geological Survey and if topographic map has been issued. And I find that some of the younger engineers frequently

come into my office to see if I have such maps. I know in some cases the reports made out by engineers contain copies of these topographic maps, or clippings from them. I am sure that the topographic maps furnished by the government are of very great value to our engineers.

E. P. MATHEWSON, Anaconda, Mont.:—I would like to express my appreciation of these maps. Referring to a matter that is not strictly one to come before this audience: I had occasion recently to assist in getting out a tour book for automobilists for the State of Montana. Montana is a big State; a great part of it is not surveyed, and we have not county maps for all the counties. The consequence is that many of our principal roads in the State of Montana are not marked on the maps, so we were at a loss to connect up some of the main highways on our tour maps, and the thought was suggested that possibly the topographic maps of the U. S. Geological Survey would help us. We immediately sought out topographic maps covering the districts in question, and to our great delight we found in most instances the county roads clearly marked, and they were entered on our tour books.

E. W. PARKER, Washington, D. C.:—I am not so familiar with the Topographic Branch of the Survey as I am with others, but we have with us to-day Mr. Manning, who is the Assistant Director of the Bureau of Mines, and I think he can explain something in regard to that if you can get him on his feet.

VAN. H. MANNING, Washington, D. C.:—I do not hesitate to get on my feet, but I know so much about the work of the Topographic Branch of the Geological Survey that I hardly know where to begin. I can, therefore, qualify to you as to my ability to speak authoritatively on this subject by saying I was a member of the Topographic Branch of the Geological Survey for over 20 years. However, I will start by replying to the inquiry as to why the U. S. Geological Survey has not been more liberal in the topographic mapping of the United States. The reason for this is because Congress has not made sufficient appropriations to survey a larger area.

I left the Geological Survey in 1910 and at that time there was about 33 per cent. of the total area of the United States surveyed. However, some of the topographic surveys covering this area were not up to the present standard of efficiency. Commenting upon the statement that has been made here of the accuracy of those topo-

graphic maps photographed up to a larger scale, I want to say that it is not always safe to make this enlargement by photography or by any other means, for such enlargements accentuate any error which in not appreciable on the smaller scale on which the map has been made. The maps should be revised about every 10 years, not because the topography will materially change, but for the purpose of adding the culture, such as roads, houses, etc., from time to time.

The Geological Survey is making better topographic maps to-day than it ever has before and the cost of making these maps is increasing. In the early history of the Survey the cost of making these maps amounted to from $2 to $8 per square mile, and to-day it costs from $20 upward for mining districts, the cost per square mile depending on the scale of the map and the character of the topography.

MR. MATHEWSON:—Is it not true that the Survey has given particular attention to the large mineral districts?

MR. MANNING:—Yes sir, and I may add, for the information of the mining engineers, the government has done away with the old contract system of land or rectangular surveys. Under the present system of having the work done by employees of the government working upon an annual salary basis these surveys are accurate and all fraudulent errors are thereby eliminated.

During my connection with the Geological Survey, I worked for a great many years in the public land States, and the locating of section corners set by contracts was in many areas a difficult thing to do. It was not always that I could find that a mile was a mile square. We of the Geological Survey tried to help you all we could in locating these corners accurately on the topographic maps, but we could not always locate the corners. Sometimes we thought we had them in their relative position, but the corners had either been moved by somebody, or they had been improperly set by the surveyor. The policy of the government is to give you a good survey, and a proper co-ordination of the topographic surveys by the Geological Survey and the rectangular surveys by the United States Land Office will bring about an improved condition both in accuracy and in economy.

JOHN GILLIE, Butte, Mont.:—I would like to commend the Geological Survey for the great assistance it has rendered the mining engineers in the West. Having been located here for more than 33 years, I have seen the start of it, and have been acquainted with many of the men who actually made the surveys. Also, I found the

Director of the Survey has always been anxious to co-operate with the engineers in the localities and accept their suggestions as to the importance of carrying on any particular piece of work first. For that reason we have been enabled in Montana to secure quite a lot of topographic work. The districts like Butte, Helena, Philipsburg, Boulder, and others have been quite important in a mining way at different times, and upon our joint request we have always found the Survey courteous, and they have carried on good work. Mr. Mathewson and Mr. Goodale are familiar with an instance that happened in my own private practice a number of years ago in regard to the lost corners that Mr. Manning spoke of. A homestead locator, about 12 miles east of Butte, in Elk Park district, was desirous of knowing the location of his farm corners—his 160 acres. The public surveys in this locality were quite limited at that time, and it was often necessary, if you did not know the location of the corners, to go a long distance to start from in order to identify the particular section. Upon applying at the office we made an appointment, and I suggested that he look up a corner, the nearest one that he could find, that we could start from. He said: "I have always known that that would be useful, and I have a corner carefully established." So, in a few days I went out there by appointment, and upon unhitching the team I asked if he could show me the corner he had located, and he said "Yes," and he took me up to his cabin, and pulled down a 4 by 4 post which he had carefully stowed away in the rafters of his cabin and properly marked. I said, "I don't want the actual corner; where was the actual corner?" and he said it was down in the meadow there somewhere; that he did not know exactly where. That is the idea some of them have. A few days ago I was out in the Swan River country, in which the General Land Office is extending its surveys, and on the trail I passed about 10 or 15 pack horses loaded with the present type of corner that the government is now establishing in the heavy timber, where it is likely to be destroyed by fire. These are metal posts with turned ends, and after seeing them planted I am certainly very much in favor of the government carrying on its own survey work.

MORTIMER A. SEARS, Denver, Colo. (communication to the Secretary *):—Mr. Woodruff's contribution calls to mind the excellent paper[1] on The Application of Descriptive Geometry to Mining Problems, by Joseph W. Roe. The latter paper shows in detail and by

* Received July 15, 1913.

[1] *Trans.*, xli., 512 to 533 (1910).

specific problems how topographic and structural contour maps may be used in practice.

The writer is in almost constant use of topographic sheets published by the U. S. Geological Survey, and has found them of the greatest value. However, these maps should be used with caution, owing to the fact that formerly they were referred to an astronomic base and not sectionalized. Therefore, because of the notoriously bad condition of the public land surveys, it is difficult to correlate a definite point on the map with a certain definite point on the ground, the tendency being to assume a regularity of network which is far from correct. At the present time the topographic sheets are based upon a survey no less correct, but the government corners are tied in as they are found, so that the maps are much more satisfactory for quick reference. Also, in finding one's way about in an unfamiliar region it is well to bear in mind that some of the maps were published so long ago that many of the roads shown have now fallen into disuse or have been washed away.

In figuring drainage area the topographic sheets furnish an accurate and speedy basis for computations, especially when a planimeter is used, and Mr. Woodruff has very properly laid considerable stress on this point.

In attempting to determine the line of outcrop where there are few exposures and where one must frequently rely upon the records of deep wells a vast amount of work may be eliminated through a proper use of the maps.

An Assay for Corundum by Mechanical Analysis.

BY W. SPENCER HUTCHINSON, BOSTON, MASS.

(Butte Meeting, August, 1913.)

IT is the purpose of this paper to describe a method used to determine the corundum contents of samples of hard crystalline gneiss containing both corundum and red garnet. A chemical analysis of the rock for alumina would not fix the amount of corundum because at least three other constituent minerals of the rock, mica, feldspar, and garnet, contain alumina combined with silica.

Following are the principal minerals found in the samples, together with their specific gravity and hardness as given by Dana:

	Specific Gravity.	Hardness.
Quartz	2.6	7
Mica and biotite	2.75 to 3.1	2.7
Feldspar	2.6 to 2.7	6
Garnet	3.15 to 4.3	6.5 to 7.5
Corundum	3.9 to 4.16	9

The corundum occurred in crystals of a gray or bluish gray color, in minute grains and coarser up to 0.5 in., and no massive or block corundum was observed. The larger crystals were commonly quite slender, well defined, and the normal hexagonal form clearly developed. They lay with the structural planes of the gneiss, and sometimes appeared prominently in relief on weathered surfaces. In some cases these crystals had suffered partial or total alteration to mica, margarite, or other soft minerals, a condition illustrated by sample D, which appeared to be studded abundantly with corundum on the flat faces. Garnet, a striking feature of nearly all the samples, was present in small rounded crystals of a pink to red color.

The samples were first broken to pass a No. 4 mesh screen. This was done with a hammer on an iron plate, particular care being taken to avoid the breaking of the crystals finer than need be to pass the screen. A portion of the sample weighing 500 g. was then taken and, with a little water added, was scoured round and round in an iron bucket with a wooden block, until the coarse crystals of corundum were cleaned of adhering pieces of mica. The bucket was then re-

peatedly filled with water and much of the fine light mica floated off. The remainder of the sample was dried and sifted, making six sizes between No. 4 and No. 14 mesh; each portion was picked over by hand and the pure corundum crystals separated and weighed. The rejected remainder was then reunited with the fine portion of the sample and all crushed to pass No. 30 mesh.

All was next concentrated by panning in a gold pan to produce a garnet-corundum concentrate. The bright red color of the garnet served as a reliable indicator and made possible the production of a clean concentrate. The tailings were panned repeatedly, usually six or eight times, until the additional amount of concentrate recovered was so small as to be negligible. This concentrate was assumed to contain practically all the corundum and garnet in the original sample—certainly all that could be recovered by any known milling treatment. It also contained a small proportion, one-fifth or one-sixth, of quartz and feldspar.

The dried concentrate was next put under an electromagnet. This picked out the garnet; but, as in most of the samples the garnet was present in amount several times the corundum, there was a relative increase in the proportion of quartz and feldspar in the residue, which still contained all the corundum. To determine the actual quantity of the latter, it was separated by floating the grains of quartz and feldspar in a heavy solution. The solution, of 3.1 specific gravity, was made up of a mixture of potassium iodide and mercuric iodide in water, and an apparatus was devised consisting of an upright glass tube with a bell top and a closed bottom, the lower 2 in. detachable, and connected by a rubber sleeve with a pinch cock. To this tube, filled with the heavy solution, the residual corundum concentrate was added and stirred in, allowed to settle, the pinch cock closed, and the clean corundum removed from the lower tube, washed, dried, and weighed.

The following is a tabulated statement of some results selected from the samples tested:

Sample.	A.	B.	C.	D.
	Per Cent.	Per Cent.	Per Cent.	Per Cent.
Coarse corundum	1.2	0.6	3.8	0.01
Fine corundum	1.3	1.8	4.8	0.4
Total corundum	2.5	2.4	8.6	0.4
Garnet	6.4	8.0	4.9	1.2

Assay of Gold and Silver by the Iron-Nail Method.*

BY E. J. HALL,† NEW YORK, N. Y., AND C. W. DRURY,‡ KINGSTON, ONT., CANADA.

(Butte Meeting, August, 1913.)

THE iron-nail method of assaying has been used for a number of years, but has not met with the approval of all assayers. The method possesses advantages which may be given as follows: (1) no preliminary treatment is required; (2) a lead button of proper size can be obtained; (3) it is economical. On the other hand, it has its disadvantages, which may be summed up as follows: (1) it is not applicable to ores containing large amounts of impurities other than sulphur, as practically all the base metals will contaminate the lead button; (2) the slag is unsatisfactory in the larger number of fusions; (3) the method was known to give low silver results under certain conditions and the gold results were questionable. It was with the idea of determining the extent of this latter disadvantage that the following investigation was undertaken.

It is well known that the fusion obtained in the lead assay is far more satisfactory than that in the "nails method" for gold and silver. The principal difference in the charges as a class is the presence of considerable reducing agent in the lead charge, and a limited oxidizing action in the nails charge, due to PbO.

The possibility of improving the nails fusion by simulating the lead charge was the reason for using argol in some tests, even though there were reasons for expecting this would have an objectionable effect on the results.

Preparation of the Gold-Bearing Material.

The materials containing gold and silver used in the following experiments were prepared rather than selected, in order to have a definite working product excluding undesirable elements. A high-

* An investigation conducted in the Assay Laboratory at Columbia University, New York.

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grade material was made so that any difference in the results obtained from the several methods employed might be more marked.

The material containing the gold was prepared as follows: Some pure quartz was ground to pass 60 mesh, and sized on an 80-mesh screen. Cupels were made of the —60 + 80 material, using sodium silicate (1 of silicate to 6 of water) as a binder. These were very soft and crumbly, so the finer (—80) quartz was tried, with sodium silicate as above. With the finer material the cupels were very firm; they were dried and heated at a red heat for five minutes. Approximately 5 g. of metallic gold was dissolved in aqua regia, heated until all the chlorine was removed, and the solution diluted to 20 cc. The cupels weighed 15 g. apiece, and into each was poured 3½ cc. of gold solution. These were then dried and heated at a red heat to break up the gold chloride and precipitate the gold in the metallic state. At the points of contact of the cupel, due to the unevenness of the base and the asbestos on which they were dried, metallic gold, which was afterwards removed, was seen; otherwise the gold was in a very fine condition, not visible with a magnifying glass. The cupels were ground to pass 120 mesh, and no metallic particles were visible on the screen.

Preparation of Silver Sulphide.

Cupels similar to those used to absorb the gold chloride solution were made, and in these were placed 3½ cc. of silver nitrate solution prepared by dissolving 23.8 g. of silver nitrate in 20 cc. of water. The solution was absorbed in about 2 min., but not as quickly as the gold solution. The cupels were placed in a 20 per cent. sodium sulphide solution over night, and then in a 5 per cent. sodium sulphide solution for 5 hours. They were washed to remove the excess of sodium sulphide, and dried, not above a temperature at which they could be handled with the fingers at any time.

The iron pyrite used in the following experiments was ground to pass a 120-mesh screen. It contained 47.4 per cent. of sulphur (= 88.9 per cent. of FeS_2) and had a reducing power equal to 10 g. of lead.

Assay of the Gold- and Silver-Bearing Materials.

The assays were made by the crucible method, and a check was also run with each assay. The charge used to assay this material was as follows:

	Grams.		Grams.
Gold material	0.5	Litharge	30.0
Silver material	1.0	Borax glass	7.0
Soda	10.0	Argol	2.0

For the checks weighed quantities of gold and silver were taken and added to a charge similar to the above.

The results were as follows:

Test No.	Original Weight.		Recovered Weight.		Loss.	
	Gold.	Silver.	Gold.	Silver.	Gold.	Silver.
	Mg.	Mg.	Mg.	Mg.	Mg.	Mg.
1	23.86	73.09	23.84	71.84	0.02	1.25
2	23.38	72.46	23.34	71.24	0.04	1.22
3	23.90	72.76	23.82	71.04	0.08	1.72

Average gold loss..... 0.05 mg.
Average silver loss..... 1.41 mg.

Assay of Materials by the Crucible Method.

Test No.	Uncorrected Assay.		Corrected Results.	
	Gold.	Silver.	Gold.	Silver.
	Mg.	Mg.	Mg.	Mg.
5	24.08	74.20	24.13	75.61
6	24.12	73.72	24.17	75.13
7	24.08	73.76	24.13	75.17
8	24.04	73.40	24.09	74.81

	Uncorrected.	Corrected.
	Mg.	Mg.
Average gold results, in 0.5 g. of material.....	24.08	24.13
Average silver results, in 1 g. of material.....	73.77	75.18

The different sulphur determinations in the slags were made as follows:

Total Sulphur.[1]—With 0.5 g. of finely powdered slag, was mixed 3 g. of zinc oxide and 0.75 g. of dry sodium carbonate. The mixture was heated at a dull red heat for 15 min. After cooling, the mixture, which is easily removed from the crucible if the temperature was not too high, is treated with water and brought to a boil, filtered, the filtrate acidified, boiled, and precipitated with barium chloride.

Sulphide Sulphur.—Was determined by treating the sample with HCl, absorbing the H_2S gas in ammoniacal cadmium chloride and titrating with iodine solution, using starch as an indicator.

The following experiments were carried on with varying mixtures

[1] W. C. Ebaugh and C. B. Sprague, *Journal of the American Chemical Society*, vol. xxix, No. 10, p. 1475 (Oct., 1907).

of pyrite and silica, to which were added definite amounts of the gold and silver material. The mixtures contained 6, 13, 20, 40, 60, 80, and 88.9 per cent. of pure pyrite, or 6.7, 14.6, 22.4, 44.9, 67.4, 90, and 100 per cent. of the impure pyrite (88.9 per cent. pure), enough silica being added to the pyrite to make 0.5 assay ton.

While these tests were made primarily to determine the value of the nails charge, it was thought advisable to run niter fusions on the same series, particularly in view of the oft-repeated statement that charges requiring more than 20 g. of niter were not satisfactory. The nails charges (Tests A and B) were:

Pyrite mixture	0.5 A. T.
Sodium carbonate	30 g.
Litharge	25 g.
Borax glass	10 g.
Nails, four	10 d.

Test A contained no argol, while Test B contained 5 g. in each assay.

For the niter tests (Test C) the following charge was used:

Pyrite mixture	0.5 A. T.
Sodium carbonate	15 g.
Litharge	105 g.
Silica	5 g.
Borax glass	7 g.
Niter or argol to give the required amount of lead.	

The silica and so large an amount of flux is not required in the lower pyrite mixtures, but was used to make the flux charge uniform. The number of grams of pyrite in 0.5 assay ton mixture is indicated in Table I.

The fusion of the nails charges which contained no argol was of the usual character. Those containing small amounts of pyrite were fairly satisfactory, but as the percentage of pyrite increased there was less complete dissolution of constituents; the nails were badly corroded, and in some cases to such an extent that they sank beneath the slag. The lead did not collect perfectly in all cases, leaving small particles in the crucible on pouring.

The charges containing argol were much better, but showed the same general effect with the increased amount of pyrite. Complete dissolution seemed to take place, and in no case were the nails corroded to such an extent that they disappeared. The charges poured much better, showing a good collection of the lead. This improved condition is unquestionably due to the decrease in oxidation of constituents in the charge. As this charge is highly basic, containing

TABLE I.—*Results of Tests by the Nail, Nail-Argol, and Niter Methods.*

Test No.	Charge.			Assay.		Sulphur in Iron Pyrite.	Weight of Slag.	Sulphur in Slag.						Sulphur Removed.	
								Total Sulphur.		Sulphide Sulphur.		Oxidized Sulphur.[c]			
	Pyrites (88.9% FeS_2)	Argol.	Niter.	Gold.	Silver.										
	Grams.	Grams.	Grams.	Mg.	Mg.	Grams.	Grams.	Per Cent.	Grams.	Per Cent.	Grams.	Per Cent.	Grams.	Per Cent.	Grams.
1–A	0.96	.....	.....	24.14	73.78	0.455	43.7	1.1	0.48	0.095	0.043	1.01	0.43	.....	.....
B	0.96	5	.....	23.68	71.46	0.455	36.5	1.0	0.365	0.33	0.117	0.66	0.248	19.7	0.09
C	0.96	1	.....	24.08	74.10	0.455	.....	.....	.....	.....	.....	.....	.....	.....	.....
2–A	2.08	.....	.....	24.18	73.42	0.982	45.0	2.0	0.90	0. 8	0.216	1.52	0.69	8.1	6.08
B	2.08	5	.....	23.65	72.09	0.982	.....	1.9	.....	0.84	.....	1.06	.....	.....	.....
C	2.08	.....	.....	23.92 *a*	72.96 *a*	0.982	.....	.....	.....	.....	.....	.....	.....	.....	.....
3–A	3.2	.....	.....	23.97	72.92	1.52	48.2	2.6	1.25	0.97	0.467	1.63	0.79	17.7	0.27
B	3.2	5	.....	24.10	72.26	1.52	38.5	2.9	1.1	1.9	0.731	1.0	0.37	26.9	0.41
C	3.2	.....	2	24.12	74.88	1.52	.....	.....	.....	.....	.....	.....	.....	.....	.....
4–A	6.4	.....	.....	23.88	71.02	3.03	50.7	5.4	2.73	3.07	1.55	2 33	1.18	10.0	0.3
B	6.4	5	.....	21.36 *b*	60.78 *b*	3.03	37.5	5.7	2.13	3.7	1.61	2.0	0.52	29.7	0.9
C	6.4	.....	9	24.14	73.82	3.03	.....	.....	.....	.....	.....	.....	.....	.....	.....
5–A	9.6	.....	.....	23.98	70.14	4.55	52.8	7.6	4.0	4.87	2.57	2.73	1.5	12.1	0.55
B	9.6	5	.....	23.92	66.10	4.55	42.5	7.5	3.28	5.3	2.25	2.2	1.03	27.9	1.27
C	9.6	.....	17	24.16	74.84	4.55	.....	.....	.....	.....	.....	.....	.....	.....	.....
6–A	12.8	.....	.....	23.83	67.45	6.02	57.5	8.8	5.06	6.01	3.45	2.79	1.61	16.5	0.96
B	12.8	5	.....	24.00	63.68	6.02	48.5	8.4	4.15	6.71	3.25	1.69	0.90	31.0	1.87
C	12.8	.....	24	24.18	74.18	6.02	.....	.....	.....	.....	.....	.....	.....	.....	.....
7–A	14.5	.....	.....	23.99	66.13	6.86	62.4	6.8	4.25	5.5	3.43	1.3	0.82	38.0	2.61
B	14.5	5	.....	24.12	62.56	6.86	46.0	8.8	4.0	7.6	3.49	1.2	0.55	40.5	2.80
C	14.5	.....	32	24.09	73.90	6.86	.....	.....	.....	.....	.....	.....	.....	.....	.....

Uncorrected results of gold material..... ..... 24.08 mg.
Uncorrected results of silver material..... ..... 73.77 mg.

Tests marked A, Nails Assay. Tests marked B, Nails assay + 5 g. argol. Tests marked C, Niter assay.
Tests Nos. 1, 2, 3, 4, 5, 6, 7, corresponded to 6, 13, 20, 40, 60, 80, 89.9 per cent., respectively, of pure pyrite on 0.5 A. T.
a Bead No. 2 C froze. *b* In assay No. 4 B a little lead was lost. *c* Oxidized sulphur obtained by difference.

large amounts of sodium carbonate, the sulphides are readily soluble; but if the iron present is converted to oxide this will not be readily taken into solution. It is not permissible to make this slag highly acid to take these oxides into solution, as the sulphides would then fail to dissolve.

It was noticed in the different fusions that the character of the furnace atmosphere, whether oxidizing or reducing, had considerable effect on the charge. If the atmosphere was strongly oxidizing the fusion was not nearly as good as when it was reducing, this apparently being due to the same causes as where argol is present in one case and absent in the other.

The largely increased weight of slag in charges high in pyrite is due partly to the greater amount of iron dissolved, and partly to the material corroded from the crucible on account of the much more basic character of these slags.

From the results, it will be noted that the silver losses increase with the sulphur content. Also, that in charges containing argol the loss is somewhat greater than where the addition of argol was omitted. The principal difference in the slags of these two methods, which could affect the results, is in the percentage of sulphide sulphur present, the charges containing argol having a much greater percentage. This indicates that the silver losses are due to sulphide sulphur.

This explanation seems quite reasonable, as these slags are somewhat of the nature of a matte, particularly where sulphide sulphur is high, and it is well known that mattes are good solvents for silver.

For the same reasons, it was expected that the gold loss would also increase to a considerable extent. It will be noted from the table that the increased loss is not proportionate to the silver, nor does it seem to increase progressively with the sulphur.

In a number of the experiments, the nails were weighed before and after fusion of the charge. It was seen that considerably more iron was removed from the nails than was necessary to form iron sulphide; also, on examining the nails, that they were covered with a dull brown coating. The following figures show the amounts of iron removed from the nails. Tests Nos. 8 and 10 were nail assays; and, to Tests Nos. 9 and 11, 5 g. of argol were added.

Test No.	Iron in Pyrite.	Sulphur in Charge.	Weight of Nails.		Loss of Iron from Nails.	Weight of Slag.	Iron in Slag.[a]	
			Before Fusion.	After Fusion.				
	Grams.	Grams.	Grams.	Grams.	Grams.	Grams.	Per Cent.	Grams.
8	6.0	6.86	29.2	13.2	16.0	56.2	27.95	18.1
10	6.0	6.86	29.4	13.9	15.5	58.5	28.7	19.1
9	6.0	6.86	29.7	17.7	12.0	44.5	21.5	13.1
11	6.0	6.86	29.1	15.5	13.6	46.4	25.1	15.3

[a] This weight includes the weight of iron on nails as scale.

Some of the coating or scale on the nails was removed and analyzed for total sulphur, sulphide sulphur, and iron. The results were as follows:

Test No.		Total Sulphur.	Sulphide Sulphur.	Iron.
		Per Cent.	Per Cent.	Per Cent.
10 A.	Nail quenched in water after removing from fusion...	3.55	1.6	58.05
10 B.	Nail removed from fusion, cooled in air.....	3.05	0.4	56.65
11 A.	Nail quenched in water after removing from fusion...	3.35	1.3	60.9
11 B.	Nail removed from fusion, cooled in air.....	3.8	0.5	54.5

The scale was considered to be iron sulphide, but this was impossible from the small percentage of sulphur. A portion of the scale from Nos. 10 A and 10 B was ground and did not show any metallic particles. Further, a portion of this scale was magnetic, and the magnetic portion from No. 10 A contained 59.4 per cent. of iron, and the magnetic portion from No. 10 B contained 59.35 per cent. of iron.

The non-magnetic portion from Nos. 10 A and 10 B became magnetic when heated in the reducing flame of a blowpipe, which indicates the presence of ferric oxide (Fe_2O_3). The literature gives this as iron sulphide (FeS), but indications are to the contrary, and the point was thought worthy of further investigation, therefore the following tests were made:

Fusion (Test No. 12) was made, using this charge:

	Grams.
Soda	20
Silica	8
Borax	8
Sodium sulphate	5.5
Nails, two	29

Also, fusion (Test No. 13), using the same charge, except that no sodium sulphate was added.

Test No.	Weight of Nails.		Sulphide Sulphur in Slag.
	Before Fusion.	After Fusion.	
	Grams.	Grams.	Per Cent.
12	29.1	22.5	0.57
13	29.8	26.5	.....

In fusion No. 12, the nails were corroded badly beneath the slag and also the portion above the slag. The scale on the nails (from the part beneath the slag as well as the part above it) was found to contain a magnetic portion, as before, and analyzed 52 per cent. and 71.4 per cent. of iron, respectively.

In Test No. 13, there was also a scale on the nails above the slag, but the portion of the nail in the slag was bright and not corroded.

This scale, as before, contained a magnetic portion, which analyzed 68.6 per cent. of iron.

These results clearly indicate that metallic iron under above conditions reacts with sodium sulphate (Na_2SO_4), producing sulphide sulphur and iron oxide, and explain the excessive corrosion of nails during long fusion in an oxidizing atmosphere. Apparently, there is a cycle in the oxidation of sulphide sulphur to sulphate by the air, reduction of sulphate to sulphide by the iron, and reoxidation by the air: The presence of an organic reducing agent interferes with this oxidation, which is the reason for less corrosion of the nails in charges using argol. The production of this large amount of iron oxide accounts for the unsatisfactory nature of the slag, as the slag is not of a character suitable to dissolve quantities of this material, which remains more or less in suspension. Further, it is responsible for rapid attacking of the crucible, which is composed of constituents that are not conducive to a satisfactory slag with the fluxes employed.

Conclusions.

1. The nails method gives low results for silver with ores containing more than certain amounts of sulphide sulphur, this loss increasing with the percentage of sulphur in the ore, reaching a maximum of about 10 per cent. in the straight nails charge under the conditions of this test. In the case where argol was used, giving increased sulphide sulphur in the slag, the maximum loss was about 15 per cent.

2. The gold results are irregular and somewhat lower, possibly 1 per cent. The loss does not appear to be affected by the addition of argol to the charge.

3. The method should not be used for silver determinations in high sulphide ores, except for approximate results. When the sulphur in the ore is not much greater than will be oxidized by the PbO, a fair approximation may be obtained. The method will give fairly good gold results on medium- and low-grade gold ores, say one ounce, as the loss is less than the accuracy of ordinary work.

4. The niter method gives good results even with as much as 30 g. of niter present in the charge and a 30-g. button is necessary in assays containing a large amount of niter.

5. The portion of the iron nails which projects into the lead is converted to sulphide, but the greater amount of iron, that in contact with the slag, is removed as iron oxide.

6. The scale on the iron nails is not iron sulphide, but a mixture of ferric oxide, magnetite, and slag.

7. To obtain a good slag by the nails method with slight corrosion of nails, it is necessary to protect the fusion from oxidation. This, however, will lower the silver results.

Discussion.

A. M. Smoot,* New York, N. Y. (communication to the Secretary †):—The nails method is given in many text books on assaying as a standard method for gold or for gold and silver in ores. It appears to be taught in the laboratories of technical and scientific schools as a standard method, and to some extent it is used by assayers in actual practice, although the class of ores to which it may be applied is limited to those free from reducible metallic compounds, the metals of which would enter the lead button.

The experiments of Messrs. Hall and Drury show the inaccuracy of this method for silver, in heavy sulphides, but not so clearly the shortcomings with regard to gold. It seems certain that low silver is caused by the solubility of silver in iron-alkali sulphide as compared with its solubility in molten lead; that is, silver is distributed between iron-alkali sulphide and metal in the charge in proportion to solubility at the particular temperature of the fusion. Lead is a fixed quantity in these assay charges, but iron-alkali sulphide increases with increased sulphur, so the more sulphur in the charge, the lower the silver results.

Considering tests 1A and 2A, in Table I, it will be seen that the amount of pyrite in these charges was so small as to be fully oxidized by the litharge; consequently, the only sulphide sulphur likely to be in the slag would be produced from the reducing action of iron on sodium sulphate. Analyses of the slag show the sulphide sulphur to be very small, practically negligible in these charges. Both silver and gold results are substantially correct, gold being fully as high as in any of the niter-excess litharge charges (tests C), and silver, although a little lower, yet within the limits of ordinary assay errors. When sulphur in the charge increases beyond the amount which can be oxidized by 25 g. of litharge the silver figures drop proportionally to the increase in sulphur. It is quite evident that no iron at all was necessary in 2A, and that in 1A it was only necessary as a reducing agent to produce a proper sized lead button. In this charge a little carbonaceous reducing agent—say a gram of argol—could advantageously have been substituted for nails. The 1A and 2A charges are

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† Received July 21, 1913.

substantially excess-litharge charges, the slags being of the order of bisilicates. In all the other A charges, 3, 4, 5, 6, and 7, wherein pyrite exceeds the amount that can be oxidized by 25 g. of litharge, silver is markedly and increasingly low as the sulphur increases, and gold is also low, but by a practically fixed amount, averaging, say, 0.20 mg.

In the B experiments, wherein a large excess of argol was used, the silver results are of the same order as in the A experiments, going uniformly lower as the sulphide sulphur increases. The gold results, however, are different; charges 1B and 2B, containing only small amounts of pyrite, yield markedly low gold, whereas, omitting 4B, from which some lead was lost, the other B charges are only slightly low. It seems that, unlike silver, gold is not soluble in alkali-iron sulphide. A reasonable explanation for the low gold in 1B and 2B might be found in the nature of the slag. This is nearer a bisilicate than a monosilicate because of the large amount of silica added to make the weight of the "ore" equal to 0.5 A. T. Thus in 1A and 1B, 13.5 g. of silica were added; in 2A and 2B, 12.5 g. In these charges there is, therefore, no excess of soda to form iron-alkali sulphides, but as the pyrite is increased and the silica correspondingly decreased the charges become more basic, with higher results in gold in the case of the B experiments. It should be kept in mind that in the A charges no extra reducing agent is present, the slag is unsatisfactory, and the ore is probably not fully decomposed, while in the B charges argol is present in large excess. Probably in the B charges the litharge is reduced early in the fusion period, whereas in the A charges it is more slowly reduced by the sulphur in the ore. It is reasonable to suppose that the sulphide formed in 1B and 2B is really present as an iron matte disseminated through the slag, but not dissolved in the form of an iron-alkali sulphide, because there is no excess of alkali in these charges to form such a compound. As iron matte is an excellent solvent for gold, low results may be expected where it is formed.

In the case of the other B experiments, Nos. 3, 5, 6, and 7, where sulphur increases and silica decreases, presumably with the formation of alkali-iron sulphide in the slag, the gold figures are considerably higher than in the A experiments, subsequent to 3A.

This argument, based on Hall and Drury's experiments, points to the conclusion that the nails method would be improved for gold by increasing the alkali, so as always to form alkali-iron sulphide instead of iron matte, as well as by adding an excess of an organic reducing agent, thereby producing a better slag. Experimental evidence in

support of this suggestion is at present slender, but so far as it goes it supports the theory.

Two samples of heavy sulphide (pyritic) ores, assayed in Ledoux & Co.'s laboratory by the excess-litharge method, gave the following in ounces of gold per ton:

Sample A.	Sample B.
1.20	1.51
1.20	1.51
1.21	1.515
1.203	1.512

The same sample by the nails method, using the conventional charge of 0.5 A. T. ore, 30 g. sodium carbonate, 10 g. borax glass, 25 g. litharge, and 4 nails, gave:

Sample A.	Sample B.
1.16	1.45
1.16	1.39
.....	1.46
1.16	1.433

These results fully confirm prior experience, that the nails method, as ordinarily conducted, gives low gold. The nails method, modified by adding 5 g. of argol to the preceding charge and increasing the sodium carbonate to 60 g., gave:

Sample A.	Sample B.
1.22	1.50
1.24	1.50
1.23	1.50

It will be seen from the above that while the usual "nails" charges give markedly low results, the modified "nails" charges are commensurate with the excess-litharge method.

W. J. SHARWOOD, Lead, S. D. (communication to the Secretary*): —It seems possible that one of the principal differences between nail-method slags and the slags formed in the ordinary lead assay, in addition to those mentioned by the authors, and perhaps the chief cause of the less satisfactory character of the former, may be the presence of a larger proportion of ferrous sulphide, or, as usually stated, of ferrous alkaline sulphide. This is due to the relative smallness of the usual lead charge and its commonly lower proportion of sulphur.

* Received Aug. 1, 1913.

Thus, with the ordinary 5-g. lead assay charge, a sample of pure galena would carry only 0.67 g. of total sulphur. With an ore consisting of one-third each of galena, blende, and pyrite, there would be 1.66 g. of sulphur. The proportion of alkaline flux per unit of ore, calculated to Na_2O and K_2O, is commonly somewhat greater in the lead assay than in the mixture used by the authors, and the large ratio of potash to soda also makes it decidedly more fusible than an unmixed soda flux.

The valuable data in Table I confirm what is, I believe, the general experience of assayers, that the nail method tends to give decidedly low silver results, but is reasonably accurate in regard to gold; while the niter fusion, if carefully performed, is fairly satisfactory for both gold and silver. The silver losses reported in this table for the nail method seem, however, unusually large.

Thus, in the four most highly sulphuretted charges (40 to 90 per cent. pure pyrite) the silver results, using nails and argol together, average 14 per cent. below the truth. Without argol the two highest in pyrite give errors of nearly 10 per cent., while with 40 and 60 per cent. of pyrite the silver is about 4 per cent. too low. On the other hand, the gold tends to be slightly higher with increasing pyrite—a result which might be modified were a large number of determinations made and averaged. The average gold value given in the table for the nail method is very nearly that of the uncorrected assay without pyrite. Is it possible that there is a trace of gold in the pyrite used?

The authors unfortunately do not mention the amounts of iron and lead in the slags, nor the size of the lead button obtained; presumably nearly all the lead was reduced. In the accompanying table I have taken the liberty of assuming, as an approximation based on the authors' data (p. 37), that 14 g. of iron was removed from the nails in each of the tests numbered 5 and 7, and that 5 g. of lead remained in the slag. The tabulated molecular ratios indicate the nature of the slags obtained, from a chemical standpoint, perhaps more clearly than the mere percentage analysis. Even if as much as 5 g. of lead remains in the slag, it is evidently of relatively small importance compared with the other constituents, mostly of low molecular weight.

In some assays made several years ago, with charges of 0.5 and 1 assay ton of Frue concentrates containing up to 50 per cent. of pyrite and pyrrhotite, using nails or iron filings, a few slags were analyzed, and it was found that nearly half the sulphur remained in the slag as sulphide. It is interesting to note that the same is true of the high-pyrite charges tabulated in this paper.

The size of the nails employed in these experiments is not stated. This seems to be a matter of some importance. Frequently a 20d. (4-in.) nail is used, either wire or cut. The late Richard Smith, when teaching assaying at the Royal School of Mines, used to recommend the cut nails then in vogue, used head downward to expose the greatest surface possible; alternatives being lengths of nail rod or pieces of 0.5-in. hoop iron, sometimes bent U-shape; or substituting a suitable amount of iron filings or turnings.

The manipulation necessary when large nails or long pieces of metal are used and removed, is a time-consuming inconvenience, while the waste of metal is considerable, as it is rarely safe to use a nail more than once. It seems desirable, therefore, to use iron in some form exposing more surface, adjusting the amount to the requirements of the case, so that all may be consumed, leaving no excess to be removed.

The idea is not a new one. In the case of the sulphide ores of lead, which are partly decomposable by sodium carbonate alone, partly by iron alone, but completely by both together, the use of iron filings is mentioned by Mitchell and by Percy, both of whom refer to still earlier work. In fact, this seems to have been one of the earliest forms in which iron was used for assay purposes. In Cramer's work on assaying (Mortimer's translation, 2d ed., 1764) one section is devoted to "Precipitation by Iron and Lead of Silver out of a Mixture containing a great Deal of Sulphur," in which the reader is cautioned "You must not use Filings quite spoiled with Rust: For they have no Virtue for absorbing the Sulphur."

At the Homestake assay office the practice, introduced about 10 years ago, is to use small steel wire nails, a number of which, suited to the material to be assayed, are stuck in the top of the charge before adding the borax cover. A size found well adapted to charges of tailing carrying from 3 to 5 per cent. of sulphur is a $\frac{1}{8}$-in. brad of 18-gauge wire, the diameter of which is about 0.045 in. An old Californian book — Barstow's *Sulphurets*, I believe — recommended small tacks for this purpose.

The authors evidently realize the advantage of using normal sodium carbonate (soda ash) as a flux, rather than the bicarbonate to which, for some unexplained reason, so many assayers still adhere. It may be worth while to point out here that soda ash usually costs less per pound, and that a pound of it contains over 50 per cent. more actual fluxing material than a pound of bicarbonate, though the latter gives off about 90 per cent. more gas per pound before action ceases. For every gram of Na_2O available as flux, one must use 1.7 g. dry sodium

Computations from Data of Assays by Iron-Nail Method (Hall and Drury).

Charge. No.	Charge. Grams.	S in FeS_2. Gm.	Fe in FeS_2. Gm.	Fe from Nails. Gm.	Total Iron. Gm.	SiO_2. Gm. a	K_2O from Argol. Gm. a	Na_2O from Na_2CO_3. Gm. a	$Na_2B_4O_7$. Gm. a	Slag Constituents. Svi Gm.	SO_3 Gm. a	S'' Gm. a	Fe in FeS. Gm. a	Fe in FeO. Gm. a	FeO. Gm. a	Total Slag Calc. Excl. PbO. Gm. b	Actual Weight Slag. Gm.	PbO. Calc. 5 g. Left in Slag. Gm.
5A	14.6	4.55	4	14	18	6.07	0	17.55	10	1.5	3.75	2.57	4.5	13.54	17.4	61.84	52.8	5
5B	14.6	4.55	4	14	18	6.07	1.25	17.55	10	1.03	2.58	2.25	3.9	14.1	18.2	61.79	42.5	.....
7A	14.6	6.86	6.14	14	20.14	1.6	0	17.55	10	0.82	2.05	3.43	6.0	14.14	18.2	58.83	62.4	.....
7B	14.6	6.86	6.14	14	20.14	1.6	1.25	17.55	10	0.55	1.38	3.49	6.1	14.04	18.05	59.42	46.0	.....
5A	Molecular ratios SiO_2 and R''O or $R_2'O$.					100	0	283	50		47		80		240	.....	.....	22.5
5B						100	13	283	50		32		70		252	.....	.....	22.5
7A						26	0	283	50		25		107		252	.....	.....	22.5
7B						26	13	283	50		17		109		250	.....	.....	22.5

a Figures added to compute total slag weight. b Total weight, assuming all PbO reduced and CO_2 expelled, and neglecting corrosion of Pot.

Molecular Ratios.

	Na_2SO_4	$Na_2S.FeS$.	$K_2O + Na_2O$ Combined with SiO_2, CO_2, B_2O_3.	FeO.	SiO_2.	B_2O_3.	PbO.
5A	47	40	246	280	100	100	22
5B	32	35	269	286	100	100	22
7A	25	53	255	305	26	100	22
7B	17	55	275	302	26	100	22

carbonate (soda ash) or 2.7 g. of bicarbonate. If both are pure the volume of gas given off by the soda ash is just one-third of that evolved by an equivalent (not equal) weight of bicarbonate. Thus the amounts finally given off per gram of Na_2O available are nearly 500 and 1,500 cc. respectively if measured at 100° C., or about double these volumes at 400° C.

The authors' data indicate the superiority of the niter assay to the nail method for high-sulphide ores, when silver is of importance. Some assayers, however, avoid large proportions of niter, on account of its tendency to boil over. Others do not hesitate to use 35 g. or more per charge, apparently without disastrous effects. This seems to be one of the instances where experience and close attention to details enable the successful use of a method which presents considerable difficulty to a novice.

A reagent which presents some advantages, and was recommended by Percy but is now little used, is red lead as a substitute for litharge. It contains at least 25 per cent. more oxygen, retaining it up to a fairly high temperature, so that the extra oxygen is available for the oxidation of sulphur or other reducing agents. Some of the redder samples of litharge in the market contain an appreciable amount of extra oxygen.

While it would not be permissible to make the slag highly acid in carrying out the nail assay, it seems desirable to add some silica whenever the percentage of pyrite is high, if only to check the corrosion of the crucibles. I believe this is common practice. One writer on assaying (W. L. Brown) has gone to the extreme of recommending the addition of silica to all assay charges—even with quartzose ores.

L. S. AUSTIN, Salt Lake City, Utah (communication to the Secretary *):—Messrs. Hall and Drury have investigated the limitations of the nail assay under certain specified conditions, but one need not accordingly infer that it is to be set aside. It remains a quick and satisfactory method if judiciously used.

The nail assay has been long since tested out in practice, the results obtained by it having been compared with those from scorification and niter-fusion. This has been within the experience of many assayers, who, as the result of their long-continued experiences, have felt that they know how and when to use it.

The prospector frequently brings to the custom assayer samples of ore for assay, sometimes a single piece, sometimes an approximate sample of a face of ore, sometimes a "grab" sample, this latter a

* Received Sept. 9, 1913.

handful of ore taken at random from a heap, and mistakenly assumed by the taker to represent the heap. Under such circumstances a result a little low, as, for example, an ounce in twenty-five of silver, would be on the safe side, and would make no practical difference to the prospector. For the assay he wishes to pay, or can afford to pay, very little.

Again, a mine assayer may have dozens of assays to make daily, results from faces of stopes, grab samples from ore piles, from ore passing through the receiving bins and from other places, where approximations would serve every purpose.

In the paper several reasons are given in favor of the nail assay and these should have weight. These are: (1) That no preliminary treatment is needed, as in the niter assay. By this I understand the preliminary assay for reducing power, though I find that the skilled assayer, by doing a little panning of the ore or simply by inspection, can, four times out of five, determine the niter which should be added. The object of the preliminary work is of course (2) to insure a lead button for cupelling of the proper size. As to the third point, the method is economical. If the economy is in saving supplies, this should not influence one, but if time is economized, then such economy is another and an important consideration.

Certainly, as stated, the nail assay is hardly suited to ores containing appreciable quantities of impurities: viz., copper, arsenic, antimony, or tellurium. Such an ore would yield a hard button and this would have to be scorified, thus introducing double work, which it is the object of the nail assay to avoid. While this might indeed happen even to an experienced assayer, still it need seldom occur, and he could well afford to take the risk.

If the charges are properly made up, I am hardly prepared to agree with the authors that the slags are unsatisfactory in the larger number of fusions. As to low results with silver, we know that results are lower than by the niter assay, and when ore lots are assayed this latter method is indicated. In such cases, however, the assayer must use his best efforts, and spare neither time nor pains to insure the proper result. One experienced assayer, using the nail assay, asserts that the difference between it and the niter assay should not exceed 0.5 per cent.

The scorification method, formerly so much used, appears of late years to have dropped more out of sight. For all that, it is a most excellent method, and as a control on the crucible assay it may be used to great advantage, especially in the case of impure ores.

Since the nail assay is so convenient, so simple, and so inexpensive,

why not use it, preparing for it a charge which can be rapidly measured out? Such a charge might be thus specified:

Ore	0.5 assay ton.
Soda	25 to 35 g.
Litharge	20 g. or less.
Argol	3 to 4 g.
Borax glass	5 to 15 g.
Quartz sand	0 to 10 g.
Ten-penny nails	1 to 6.

Considering these fluxes in order:

For most ores 25 g. of soda is enough, but if the ore is nearly all silica 35 g. is needed. When the ore contains but little silica the deficiency is made up by the addition of sand up to the amount of 10 g., knowing that any shortage will be made up from the crucible itself. Glass may be used in place of sand, but to twice the quantity of the latter. I may say that for rather siliceous ores the crucible corrosion is not serious, but one is naturally desirous of saving the crucible for other assays, and here again one must be cautious never to use it again on ores or ore lots when the results must be exact.

Twenty grams of litharge, which will be entirely reduced by the argols, gives a suitable-sized button, and it is not intended that any lead shall be left in the slag. In case the ore contains lead then the litharge is lessened to insure 18 g. of a button.

The quantity of argol is so small that its presence hardly affects the slag. Added to insure the reduction of all the lead in an oxidized ore, it is left standing even when not needed.

One can hardly use less than 5 g. of borax glass and be sure that the top of the charge is covered to exclude the air; 15 g. is added where the ore is decidedly basic.

The nails will vary, according to the judgment of the assayer. Though in an apparently oxidized ore the nails could be omitted, still one is retained as a precaution.

I will give now some examples of fluxing, applying the above principles, and using 0.5 assay ton of ore.

1. A quite siliceous ore with little base: Soda, 25 g.; litharge, 20 g.; argol, 3 g.; borax glass, 5 g.; and one nail.

2. An ore largely galena: Soda, 25 g.; litharge, 10 g.; argol, 3 g.; borax glass, 5 g.; and two nails.

3. A basic, oxidized ore with little or no lead: Soda, 25 g.; litharge, 20 g.; argol, 4 g.; borax glass, 15 g.; sand, 10 g.; and one nail.

4. A basic, blendy ore having little lead: Soda, 25 g.; litharge, 20 g.; argol, 3 g.; borax glass, 15 g.; with three nails.

5. An ore high in pyrite and having little silica: Soda, 25 g.; litharge, 20 g.; argol, 3 g.; borax glass, 15 g.; sand, 10 g.; and six nails.

Where the work is run hot, with pieces of coke placed at the mouth of the muffle, one should have but little trouble from drops of lead sticking to the nails, and with but little oxidizing effect upon the charge.

In conclusion, I would say that I consider the nail assay to be a speedy and practical method of determining ores, especially those where fairly approximate results will serve; but for obtaining the best and most accurate returns, one should use the niter assay or scorification.

Hardinge Mills vs. Chilean Mills.

BY ROBERT FRANKE, MIAMI, ARIZ.

(Butte Meeting, August, 1913.)

In view of the prominence which the conical mill has attained in the fine-crushing field within the few years since its introduction, the following comparison with its more mature forerunner, the Chilean mill, based on extensive tests, is submitted in the interest of the milling profession.

Soon after designing the concentrating plant of the Miami Copper Co. in 1909, the Hardinge conical mill made its appearance in the milling forum. Its possibility as a suitable crushing device for the plant was well recognized, but in view of the lack of commercial demonstration, at the time, as to capacity, efficiency for desired product, and the still more uncertain factors of cost of maintenance and power consumption, it was deemed that the immediate adoption of this machine throughout the plant would be a hazardous undertaking. For these reasons it was decided to equip the majority of the immediately required units of the plant with Chilean mills, the fine-crushing proficiencies of which were better known, and one section with Hardinge mills, to serve as a test unit for the guidance of future installations and replacements. Thereby, after 1.5 years' operation with both types of mills, a thorough test as to metallurgical efficiency and cost economy has been obtained.

The conical mill used in these tests is the 8-ft. Hardinge pebble mill, having a cylinder 22 in. in length. The cylindrical portion of this mill is lined with cast-iron liner plates, and the conical extensions with silex bricks bound together by cement. Each liner plate carries a projecting lifter, the function of which is to increase the height of drop of the lifted material. Danish No. 5 pebbles, obtained from the coast deposits of Denmark, are used for the grinding charge. The Chilean mill used is a fast-running, 3-roller, 6-ft. Saturn mill, with screens of 0.037-in. opening. The feed to these mills is the oversize of Callow screens having 0.029-in. openings, which follow rolls crushing to 0.5 in.

For the ore of this mine, a moderately hard but very fissile schist,

impregnated with finely disseminated granular chalcocite, the conical mill has proved itself superior, metallurgically and economically, as a fine-grinding machine. This superiority it has attained by a combination of commendable characteristics, namely: Smoothness and steadiness of operation, delivery of a product enabling better extraction, more economical water consumption, a lower operating and maintenance cost, and a very low rate of depreciation.

Steadiness in operation, of paramount importance in plants of such large capacities, is effected in this type of mill by its simplicity in principle and the consequent simplicity in construction. Discharge screens, dies, and mullers are eliminated, and in their place more desirable crushing equivalents are substituted. Thus, the screen of the Chilean mill is replaced by a perpetual device; the dies by linings which have long life; and the mullers by flint pebbles which are replaceable without interruption to operation.

Delays with these two types of mills in this plant have been found to be as follows:

Chilean Mills.	Per Cent.	*Hardinge Mills.*	Per Cent.
Screen delays,	0.57	Relining delays,	0.71
Repair delays,	1.54	Repair delays,	0.58
Total delay,	2.11	Total delay,	1.29

From the above it is seen that the delays of the Hardinge mill approximate 60 per cent. of those of the Chilean mill, and that relining constitutes more than one-half of the total delay. This is mainly due to the fact that when a mill is relined it must be idle about 48 hours, so as to give the cement used for binding time to set. This delay, however, can be materially reduced by means of shell stands, so that a newly relined shell will always be ready to be replaced by an overhead crane when a worn-out shell is to be removed. Allowing 2 hours for this replacement, the delay from this cause will be reduced to about 0.05 per cent. Thus the necessary delays with this mill simmer down to those of repairs to bearings and pinions, lifting out a shell with worn lining and reinstating a renewed one, and the occasional replacement of feed scoops, which approximate a total delay of about 0.6 per cent.

Less actual attendance is required by the Hardinge mill. This makes it possible to reduce the operating cost in plants where the duty of the attendant can be so distributed as to include the supervision of other apparatus. Occasional pebble feed, lubrication, and a look-out for obstructed discharge boxes, are the only services required. By their adoption in this plant the operating labor cost of fine crushing has been reduced about one-half.

For the reduction practiced at this plant, the conical mill has proved itself to be the more suitable fine-grinding machine. Because of the generally granular character of the chalcocite of this ore, it is the aim to produce a product of such size as will liberate a maximum mineral content with as small a production of ultra fines as possible. It has been found that a product which contains a maximum percentage between the sizes of 60 and 200 mesh is best. Below is given a typical screen analysis of feed and product for both types of mills. From this it is seen that the Hardinge mill yields 37 per cent. of the desired size of material or about 50 per cent. more than the Chilean mill, and with a smaller production of slime.

Mesh.	Chilean Mill.		Hardinge Mill.	
	Feed.	Product.	Feed.	Product.
+ 4	13.9	0	12.9	0
+ 10	47.5	0	47.3	0
+ 20	22.9	2.3	26.8	0.2
+ 30	5.2	11.8	5.0	3.2
+ 40	0.9	6.7	0.8	4.9
+ 60	1.0	11.4	0.8	13.8
+ 80	0.5	6.7	0.4	10.4
+ 100	0.4	5.4	0.3	8 6
+ 150	0.5	6.3	0.3	8.0
+ 200	0.7	7.2	0.5	10.0
— 200 sand	1.2	10.6	0.8	10.0
— 200 slime	5.3	31.6	4.1	30.9

The Hardinge mill also consumes less power. At this plant, a 150-h-p. induction motor operates three 8-ft. Hardinge mills or two 6-ft. Chilean mills, and the power consumption, for the above reduction, is as given below. The consumption is given on the basis of both crude-ore tonnage and actual feed tonnage, the latter being approximated at 70 per cent. of the former.

	Crude-Ore Tonnage.	Actual Feed Tonnage.
	Horse-Power-Hr. Per Ton.	Horse-Power-Hr. Per Ton.
Chilean mill, ...	7.5	10.7
Hardinge mill, ...	6.7	9.6

A striking feature brought out by the comparative operation of these mills, is the difference in the duty exacted of the cone tanks. The sections of the plant operated with Hardinge mills have shown an average reduction of nearly 75 per cent. in the solid feed and a 40 per cent. reduction in the water feed to these tanks, as compared with the Chilean mill. This is to be attributed to the combined result of the smaller quantity of water fed to the grinding mill and

of the smaller production of slimes. For plants where the production of extreme fines is not desired, the opportunity is thereby offered of lessened outlay for dewatering equipment.

The maintenance costs of Chilean and Hardinge mills are shown in the accompanying tables. The cost for each mill is based on crude-ore tonnage so that the cost per ton of actual feed would be 40 per cent. greater than the cost shown, as in the reduction practice of this plant about 70 per cent. of the crude-ore tonnage passes through the fine-grinding mills.

Chilean Mill.

Tons milled, 826,000.

	Cost Per Ton.	
Driving Mechanism.		
Shafts, pinions and gears,	$0.00230	
Spindles, muller bushings, etc.,	0.00275	
Miscellaneous,	0.00042	
		$0.00547
Crushing Mechanism.		
Dies (12,910 tons per die),	$0.01079	
Tires (7,310 tons per tire),	0.00987	
Screens (183 tons per screen),	0.00893	
Miscellaneous,	0.00176	$0.03135
Total supplies,		$0.03682
Repair labor,		0.00841
Shop expense,		0.00543
Total maintenance cost,		$0.05066

Hardinge Mill.

Tons milled, 450,000.

	Cost Per Ton.	
Shafts, pinions and gears,		$0.00036
Lining.		
Liner plates and lifters,	$0.00403	
Silex,	0.00300	
Cement,	0.00050	
		0.00753
Pebbles (2.51 lb. @ $0.013),		0.03262
Miscellaneous,		0.00012
Total supplies,		$0.04063
Repair labor,		0.00268
Shop expense,		0.00214
Total maintenance cost,		$0.04545

From this it is seen that the Hardinge mill, for the practice of this plant, shows a maintenance cost of about 0.5 c. per ton less than the Chilean mill. It is to be noted, however, that pebble consumption constitutes 70 per cent. of this cost, and the freight on pebbles comprises approximately 50 per cent. of their expense in this locality. The item of "shafts, gears and pinions" is probably somewhat low, in that these mills have not been operated sufficiently long to obtain

a true average. Nevertheless, this part of the cost is small since these gears have a long life, and, constituting but a small percentage of the total, is inconsequential.

However, the decisive factor of the lower cost of the Hardinge mill is its low rate of depreciation. The life of its shell, if proper care is taken that the lining is not allowed to wear through to it, is very long. Six Chilean mills have shown an efficient life of 825,000 tons, making the rate of depreciation, inclusive of transportation and installation costs, about 3 c. per ton. Allowing a life of 10 years for the Hardinge mill, its depreciation cost would be less than 0.5 c. per ton.

Summarizing these factors, the net gain in cost by operating with the Hardinge mill, for the practice of this plant, shows as follows:

Operating.	Cost Per Ton. Cents.
Labor,	0.50
Power—0.6 kw-hr.,	0.75
	1.25
Maintenance,	0.50
Depreciation,	2.50
Saving,	4.25

To the above are to be added other advantages, the more conspicuous of which are: greater capacity by reason of lower power consumption and lower delays; superior product enabling a better extraction to be made; smaller water consumption; and for minimum slime practice requires less dewatering equipment.

Furthermore, this mill is not yet out of the experimental stage, and there are possibilities of still better performances. For instance, by lengthening the cylinder of a 6-ft. middlings recrushing mill from 22 to 38 in., it was found that the capacity of the mill was doubled, the power consumption lessened, and the pebble cost decreased to nearly one-half. It would seem, however, that this idea can be carried too far, for the more the cylinder of this mill is lengthened, the more it tends to approach a tube mill, and so become a slimer. For regrinding middlings, however, this variation in dimension is a step in the right direction, in that the liberation of occluded mineral necessitates a fine product. Also large percentage variation in the sizes of feed seems to have a considerable influence on the consumption of pebbles. Thus it was found in a test in which all the feed was sized through 2.5 mm., that the pebble consumption was 1.85 lb. per ton of actual feed as against a consumption of 3.60 lb. with the oversize feed shown in the table. Experimental variations in speed, in dilution of the feed, and in size of pebble charge, may lead to further economies.

An interesting comparison, which, while based on rather ideal assumptions, is so decisive in result as to be given credit, is the mechanical crushing efficiency of these machines determined by the method of calculation discussed by Algernon del Mar in his article on Mechanical Efficiency of Crushing.[1] These calculations, as shown in the accompanying tables, are based on Rittinger's law that "the work done in crushing is proportional to the reduction in diameter." This assumes that all surfaces exposed give the same unit resistance to crushing, whereas it is to be inferred that there are some surfaces which, by reason of inherent fissility of the ore, offer a lower unit resistance than surfaces not so favored. However, in view of the large number of surfaces produced, it would seem reasonable to assume that an average unit resistance to crushing will prevail in a not unduly long test. Furthermore, since in these calculations both machines are treated equitably with regard to the practical variations which do not enter into the law, the comparative results can be considered fairly reliable.

Table I. shows the crushing efficiency without regard to quality of product made, from which it is seen that the units of reduction performed by the Hardinge mill exceed those of the Chilean mill by from 18 to 23 per cent., depending upon the degree of accuracy attained in the assumptions made in the calculations, and considering 5 per cent. as a safe limit. This evaluation proves that the Hardinge mill converts more of the power consumed into reduction of the charge than does the Chilean mill.

Table II. shows the comparative crushing efficiency with regard to size of product made. From this it is seen that in amount of work performed on the various sizes of the feed, the Hardinge mill exceeds the Chilean mill in all cases, again showing that this mill converts more of the power taken by it into actual work done. Furthermore, the excess work done is mostly expended on the grades of product desired, thereby proving that this mill more efficiently fulfills the duties assigned. It is here that the cone comes into play. This geometrical device serves the function of adjusting the crushing energy expended so as to be proportional to the force required to reduce the particles to a given size. This is effected by two principles that are inherent with the operation of the mill. First, through the continual displacement of the larger particles of the charge upon the smaller, there takes place a segregation of the particles in the cone according to size—the larger assuming positions at the greater diameter and the smaller receding toward the smaller end of the cone

[1] *Engineering and Mining Journal*, vol. xciv., No. 24, p. 1129 (Dec. 14, 1912).

of the mill; second, through the combined action of this segregation and the diminishing action of centrifugal force toward the apex of the cone, varying intensities of energy are imparted to the pebbles —the larger receiving greater inertia by reason of greater mass and greater lift and the smaller less and less inertia by reason of the smaller mass and lower lift. Thus there exist within the mill an orderly arrangement of zones of ore particles, each requiring a certain amount of impact to be reduced to a given size, and a series of zones of forces so arranged as to impart impacts that tend to be proportional to the crushing energy required by the ore particles upon which these forces are exerted. For these reasons the production of slimes is minimized and the accumulated forces are utilized to best advantage, whereas in the Chilean mill the crushing forces are uniform and disadvantageously expended upon a mixed aggregation of coarse and fine particles.

The Hardinge ball mill has also been tested in this plant as a substitute for rolls, for intermediate crushing on 0.5-in. material. This mill, however, was soon discarded, since it was found that the desired product could only be obtained at too low a capacity, and the consumption of steel balls was too great to be economical.

Thanks are due J. Parke Channing, Consulting Engineer and Vice-President of the Miami Copper Co., for permission to publish the above data. I am also indebted to B. Britton Gottsberger, General Manager, for his kindness in placing at my disposal the metallurgical data of these tests.

TABLE I.—*Mechanical Crushing Efficiency—Hardinge vs. Chilean Mills.*

On basis of law, that "the work done in crushing is proportional to the surface exposed in crushing" and therefore "nearly proportional to the reduction in diameter" or "nearly proportional to the reciprocals of the diameters crushed to."

Hardinge Mill.						Chilean Mill.					
Mesh.	Reciprocal of Average Size.	Feed. Per Cent.	Relative Surface in Feed.	Product. Per Cent.	Relative Surface in Product.	Mesh.	Reciprocal of Average Size.	Feed. Per Cent.	Relative Surface in Feed.	Product. Per Cent.	Relative Surface in Product.
+ 4	4.1	12 9	53	0	. . . .	+ 4	4.1	13.9	57	0	. . .
+10	7.2	47.3	341	0	. . . .	+ 10	7.2	47.5	342	0	. . .
+20	18.3	26.8	490	0.2	4	+ 20	18.3	22.9	419	2.3	42
+30	37.7	5.0	189	3.2	121	+ 30	37.7	5.2	196	11.8	445
+40	58.4	0.8	47	4.9	286	+ 40	58.4	0.9	53	6.7	391
+60	83.6	0.8	67	13.8	1,154	+ 60	83.6	1.0	84	11.4	953
+80	138	0.4	55	10.4	1,435	+ 80	138	0.5	69	6.7	925
+100	163	0.3	49	8.6	1,402	+100	163	0.4	65	5.4	880
+150	220	0.3	66	8.0	1,760	+150	220	0.5	110	6.3	1,386
+200	303	0.5	151	10.0	3,030	+200	303	0.7	212	7.2	2,182
—200	400	4.9	1,960	40.9	16,360	—200	400	6.5	2,600	42.2	16,880
		100.0	3,468	100.0	25,552			100.0	4,207	100.0	24,084

SUMMARY.

	Hardinge.	Chilean.
Units of work in product	25,552	24,084
Units of work in feed	3,468	4,207
Units of work done by mill uncorrected for capacity	22,084	19,877
Units at capacities of 2.50 tons and 2.25 tons per h-p. day, respectively	55,210	44,723
Excess units of work done by Hardinge mill	10,487	
Excess efficiency, assuming method of calculation correct	23.45 per cent.	
Excess efficiency, assuming 5 per cent. as the limit of error	18.45 per cent.	

TABLE II.—*Screen Size Crushing Efficiency.*

Hardinge Mill.

Mesh.	Reciprocal of Aperture.	Feed. (Cumulative Per Cent.)	Relative Surface in Feed.	Product. (Cumulative Per Cent.)	Relative Surface in Product.	Relative Surface Produced.
4	4.9	87.1	427	100.0	490	63
10	13.3	39.8	529	100.0	1,330	801
20	29.4	13.0	382	99.8	2,934	2,552
30	50.5	8.0	404	96.6	4,878	4,474
40	66.7	7.2	480	91.7	6,116	5,636
60	115	6.4	736	76.9	8,844	8,108
80	147	6.0	882	66.5	9,776	8,894
100	182	5.7	1,037	57.9	10,538	9,501
150	272.5	5.4	1,472	49.9	13,598	12.126
200	333	4.9	1,632	40.9	13,620	11,988

Chilean Mill.

4	4.9	86.1	422	100.0	490	68
10	13.3	38.6	513	100.0	1,330	817
20	29.4	15.7	462	97.7	2,872	2,410
30	50.5	10.5	530	85.9	4,338	3,808
40	66.7	9.6	640	79.2	5,283	4,643
60	115	8.6	989	67.8	7,797	6,808
80	147	8.1	1,191	61.1	8,982	7,791
100	182	7.7	1,401	55.7	10,137	8,736
150	272.5	7.2	1,962	49.4	13,462	11,500
200	333	6.5	2,165	42.2	14,053	11,888

Comparison of Efficiency.

With Units Corrected for Capacity.

Size.	Hardinge Mill. Energy Units.	Chilean Mill. Energy Units.	Difference in Favor of Hardinge.	Distribution. Per Cent.
At 4 mesh	158	153	5	0.02
At 10 mesh	2,002	1.838	164	0.57
At 20 mesh	6,380	5,423	957	3.32
At 30 mesh	11,185	8,568	2,617	9.08
At 40 mesh	14,090	10,447	3,643	12.64
At 60 mesh	20,270	15,318	4,952	17.18
At 80 mesh	22,235	17,530	4,705	16.32
At 100 mesh	23,752	19,636	4,116	14.28
At 150 mesh	30,315	25,875	4,440	15.41
At 200 mesh	29,970	26,748	3,222	11.18

Discussion.

Arthur O. Gates, Lafayette, Ind. (communication to the Secretary*):—In connection with the comparison of mechanical crushing efficiencies in Tables I and II of Mr. Franke's paper, I wish to suggest a somewhat simpler way of making this comparison, based upon what the writer has called the "crushing-surface diagram," as published by him in the *Engineering and Mining Journal* for May 24, 1913. Such a diagram, based on the data given in Table II, is submitted herewith, Fig. 1, cumulative percentages being plotted as abscissæ and reciprocals of diameters (theoretical mesh per inch) plotted as ordinates. Such a diagram averages the diameters without calculation, and areas upon it are proportional to surface produced, and, in accordance with Rittinger's law, to energy spent on crushing alone.

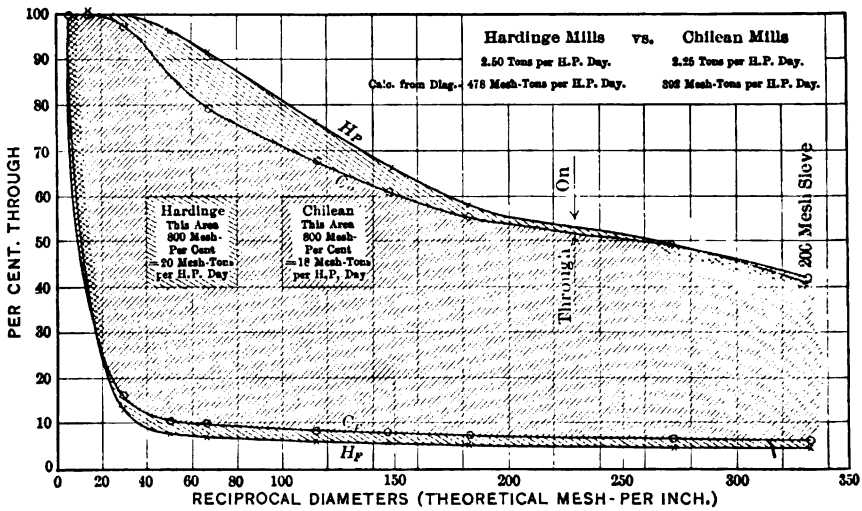

Fig. 1.—Crushing Efficiencies of Hardinge and Chilean Mills, Plotted on Crushing-Surface Diagram.

The Chilean mill diagram has been superimposed upon that of the Hardinge mill, the excess area of the latter measuring the excess of work done, based upon equal capacity. Measuring these areas up to 200 mesh (reciprocal 333), and multiplying by the tons per horse-power day, the writer gets a production of 392 mesh-tons per horse-power day for the Chilean mill, and 478 mesh-tons per horse-power day for the Hardinge, an increase of some 22 per cent. in favor of the

* Received Aug. 8, 1913.

Hardinge. (The term *mesh-ton* represents the increased surface produced by crushing all particles of a ton of rock to a diameter whose reciprocal is one greater than in its previous condition. Diameter should be in inches, although of course this can be adapted to other units. For example, a ton of evenly sized pieces 1 in. in diameter would have 1 mesh-ton of surface; a ton of similar pieces just passing a hole 0.01 in., and retained on a screen with holes the reciprocal of whose diameter was 101, would have 100 mesh-tons of surface; the difference between two lots of 1 ton each, whose diameter reciprocals were respectively 99 and 100, is 1 mesh-ton.)

While the 22 per cent. in favor of the Hardinge mill checks with Mr. Franke's results, I wish to question his adoption of the value 400 as the reciprocal of average size of the material passing the 200-mesh sieve. From the way the curve of products is running (in the plotted crushing-surface diagram) there is every indication that there is 1,000 and 10,000 reciprocal (theoretical mesh) material present, so of course the average size is very much smaller than he has indicated. The field beyond 200 mesh (ordinary screen) has been so little explored that it would seem advisable to limit calculations on efficiencies to the plus 200-mesh sizes.

If the results of Mr. Franke's sizing analysis in Table II are plotted on logarithmic paper instead of on the crushing-surface diagram, the last few points will be found in a straight line for both machines, with this difference: the Hardinge line is steeper than the Chilean line, as shown in Fig. 2. I have found similar straight lines as a result of plotting other results, indicating a law by means of which the minus 200-mesh material may be studied. It will be sufficient to state here that the straight line indicates a hyperbola for the screen analysis plotted reciprocals against weights or percentages as in the crushing-surface diagram, and further, that the steeper line on the logarithmic plotting indicates the more efficient work.

In spite of the commercial success of the Hardinge mill, and the increased economic results accomplished by its introduction into concentrating mills, I wish to criticize the statements that, as in this paper, are so frequently made as to the value of the cone, its segregating action on the pulp, and the graduation of forces, intensities of energy, or inertia so that each particle gets just the right blow. I have never seen any published results of screen analyses of material taken at different points along the cone, and I do not think screen analyses taken at these points will bear out the claims made for this feature.

Analyzing the mill on the assumption that the greatest diameter is

to produce the greatest effect in crushing, we find that the weight of crushing pebbles is proportional to the square of the diameter (machine half full); that the energy per unit pebble weight is something nearer the square than the first power of the diameter; and that the velocity with which the ore or pulp being crushed passes through the mill is inversely proportional to the square of the diameter. The result

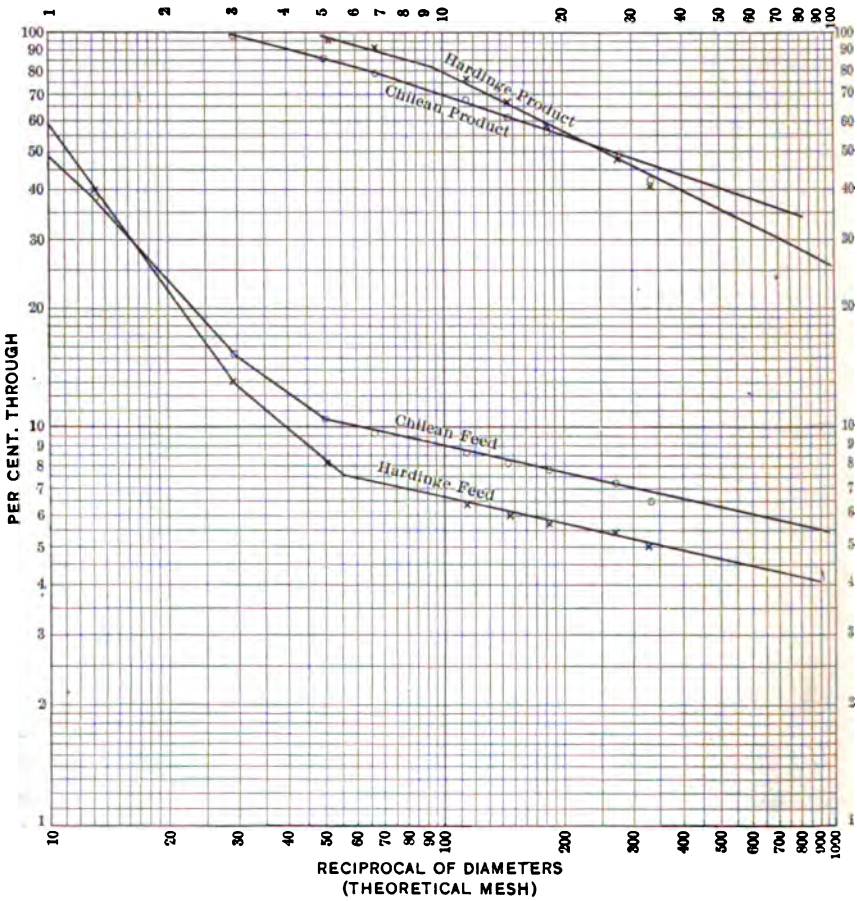

FIG. 2.—LOGARITHMIC PLOTTING OF CRUSHING EFFICIENCIES OF HARDINGE AND CHILEAN MILLS.

is that the energy applied per pound of pulp at various points along the cone is inversely proportional to about the sixth power of the diameter. This means that half way toward the apex of the cone, only 1/64 as much work is done as at the cylindrical portion, while three-fourths of the way toward the apex, only 1/4000 is done. This means that the energy applied along the cone is so small that the force exerted by

the falling or rolling pebbles is not sufficient to break the coarser particles, with the result that the work in the cone is largely done on the fines! This is as logical as the generally accepted explanation.

But the Hardinge mill is not run at such speeds that the effect of the large diameter is obtained; it runs at such a speed (750 ft. per minute peripheral speed for the 8-ft. diameter size, according to Mr. Hardinge[1]) that centrifugal force at the periphery is about 1.2 times that of gravity and therefore at least one layer of pebbles in the

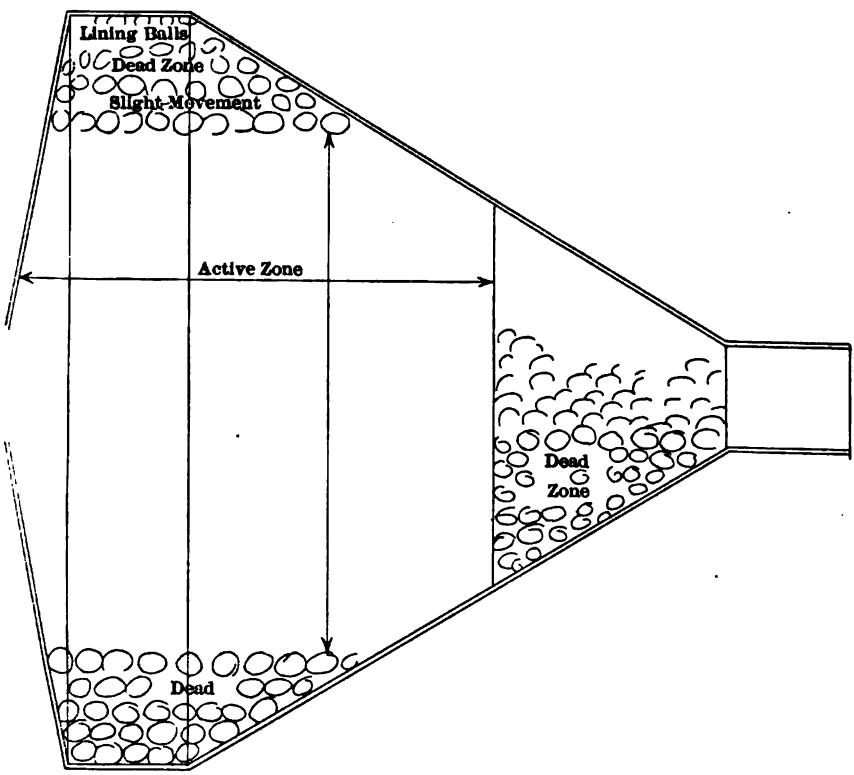

FIG. 3.—ACTIVE AND DEAD ZONES IN THE HARDINGE MILL.

periphery is useless except for the purposes of lining. For at least a foot in, the possible fall of the pebbles is so slight as to be valueless for crushing. The result is, neglecting part of the apex of the cone where the energy is too small to be effective, the Hardinge mill resolves itself automatically into a short tube mill, the 8-ft. size producing about the same effect as a 5 by 5 or 6 by 6 ft. tube mill, as in Fig. 3.

[1] *Trans.*, xlv., 201 (1913).

Perhaps I am like the sailor's mother who could credit his story about the mermaids, but refused to believe what he told her about the flying fish. The segregation of the pebbles is entirely reasonable, the survival of the most energetic, but how can the fines separate themselves from the coarse in the turmoil taking place within the crushing zone? The particle has got to go where it is knocked, the agitation is too great for it to follow any laws of classification, and it with the

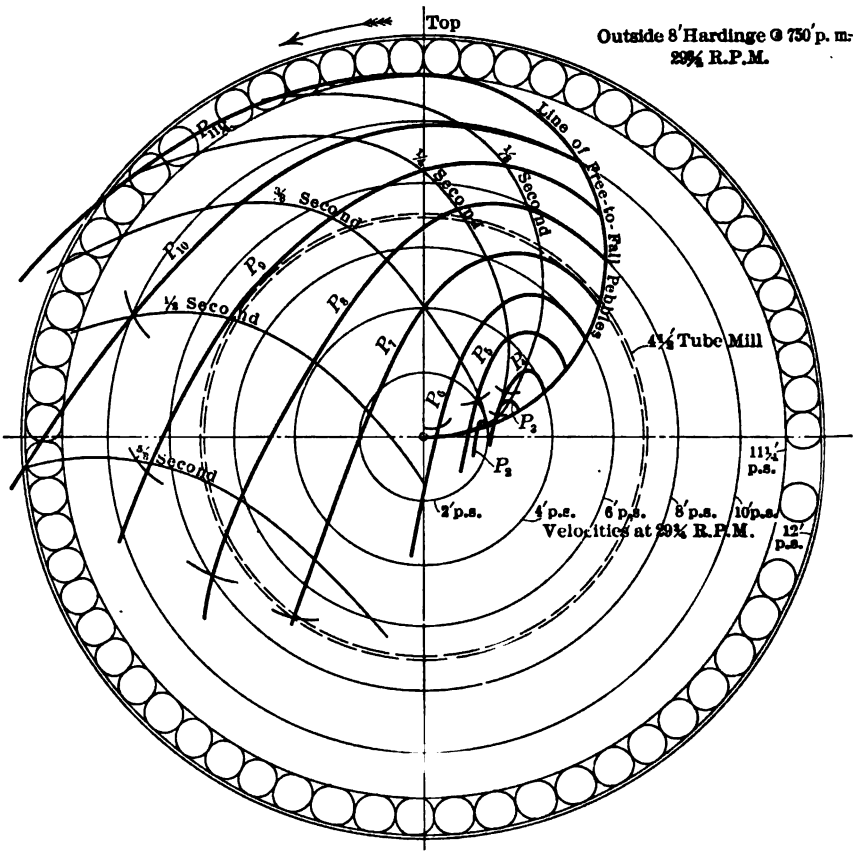

FIG. 4.—DIAGRAMMATIC REPRESENTATION OF THE ACTION IN A TUBE MILL.

others passes through by displacement and chance, perhaps getting through without being hit at all; or again, a single particle in the final pulp may be the result of perhaps a hundred blows.

In Figs. 4 and 5 are plotted some graphical results of calculation of what goes on inside a tube mill, particularly of the Hardinge type. The concentric circles in Fig. 4 represent planes through the cone. The velocity of each of these circles, based on 29.75 rev. per minute,

is indicated in the lower right-hand quadrant. Applying the principles of mechanics, it will be found that pebbles will become free to fall when reaching the half-circle drawn in the upper right-hand quadrant, going up, and their paths from that time on will without interference follow the paths P_2, P_5, P_8, etc. Centrifugal force is too great on the outer ring of pebbles to let them move. Supposing pebbles to leave this half-circle along each of the concentric circles at the same time, lines of equal interval of time have been drawn so that one may judge velocities. I have not attempted to locate the landing place of

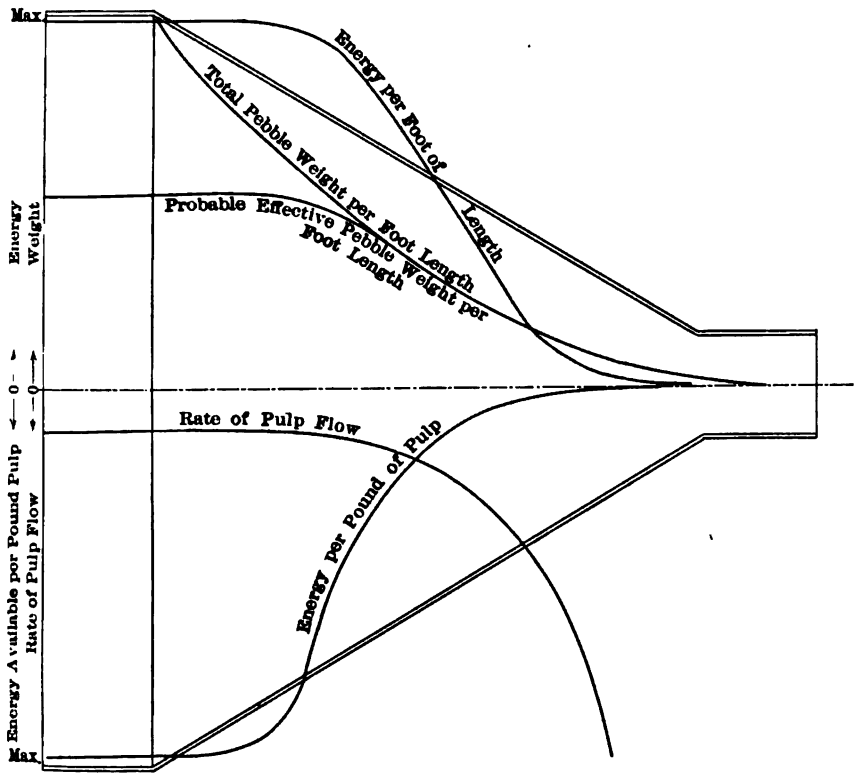

FIG. 5.—CURVES OF ENERGY, PEBBLE WEIGHT, AND FLOW OF PULP.

the pebbles very accurately, although I have shown possible landing places by curved lines across the path lines. When these landing places are located properly the resultant velocity can be determined graphically, energy then being proportional to the square of the velocity.

In Fig. 5 are plotted away from the horizontal axis of the mill, weights and energy available per foot of length, for different positions

along the length of the mill, and below, the rate of flow of pulp through the mill, and the energy per pound of pulp imparted by the action of the pebbles. The curves are plotted to relative units, not absolute.

I predict that the fine-crushing machine of the future concentrating mill will be a short tube mill, followed by an efficient sizer to remove more of the fine material than is done at present, and followed by a second short tube mill. And by short tube mill I mean short, 1 or 2 ft. long, and perhaps of large diameter, the pulp traveling through it rapidly so that the fines are not subjected to repeated crushing.

The great advantage of the Hardinge mill is in its simplicity, reliability, and low operating charges, as Mr. Franke has so clearly shown, and most operators are interested most in these features. His comparison of efficiencies is one of the first real quantitative comparisons made in this country. It is my opinion that the day is coming when such comparisons of efficiency will be made from day to day in our milling plants, just as in the power plant indicator cards are taken and worked up at frequent intervals.

The Use of the Microscope in Mining Engineering.

BY FREDERICK W. APGAR, JAMAICA, N. Y.

(Butte Meeting, August, 1918.)

THE valuable results that have followed the application in recent years of microscopic methods of research to problems of ore genesis have been significant, but possibly the recognition of their practical importance is not as general as might be, and while, as a scientific method of investigating ores and rocks, the advantage derived through the use of the microscope is unquestioned, its utility as an aid in solving mining problems arising during actual every-day mine examination and operation may not be fully appreciated.

Perhaps the most logical method of showing some of these practical applications of the microscope will be to consider first the prospect stage of a mining venture, then the uses to which the instrument may be put during the operation of a working and developed mine, and, finally, to discuss some of the problems of a metallurgical nature that may arise during milling and smelting the ore.

The valuation of mining property as a preliminary to acquiring ownership or determining the advisability of investing time and money in an enterprise is one of the most important and delicate problems that come before the mining engineer. The many and variable factors that have to be taken into account, as, for example, the geology of the district, availability of supplies, probable mining costs, transportation facilities, and the quality of ore, require a careful balancing of costs against recoverable values.

Primarily, however, a study of the local geology is of first importance, since, in a prospect with little development work, the question of character and probable quantity of the ore and its value can only be approximated with any degree of accuracy by a thorough investigation of geologic conditions, and a general knowledge of ore deposits. Unless these are favorable and pay ore reasonably certain, other factors need not be considered. The character of the outcrop, evidences of mineralizing agencies in the adjacent rocks, and geological structure, are the data of chief significance.

Now, an early question which will present itself is: What sort of a deposit have we to deal with? Is it a fissure vein, for instance; is it a replacement, or is it possibly a contact-metamorphic deposit?

At first thought it might seem as though the broad general structural characteristics would serve to differentiate these classes, and usually an idea may indeed be gained in this way which will be proved by subsequent investigation to be correct. Not always, however. Let us take, for example, the first two types, fissure veins and replacement deposits. Normally these have few points in common. The former has relatively well-defined walls, and comparative regularity of thickness and character; the latter appears in ramifying masses and irregular mineralized areas throughout the containing rock.

Fissure veins are formed when mineral-bearing waters or gases traverse a pre-existing opening in the rock where some dynamic disturbance has caused fracturing and faulting. The channels for underground circulation so formed are gradually filled by mineral matter deposited from the solutions.

Replacement deposits, on the other hand, are formed by chemical interchange, or metasomatism. No open space for deposition exists except such minor cracks or fractures as are necessary to permit the solutions to penetrate the rock. The process is really one of substitution. Certain of the rock constituents are dissolved and, at the same time, metallic sulphides or foreign minerals are deposited. The reaction is simultaneous, molecule by molecule, solution of one and precipitation of the other, until finally the original grain is wholly replaced by a new constituent.

Now consider a certain type of replacement ore body often met with where rocks have been subjected to intense dynamic stress which has produced fractures along more than a single plane. The effects of the disturbance are distributed through a series of narrow parallel cracks, closely spaced in the zone of maximum movement and gradually separated by wider and wider intervals toward the outer limits. Under favorable conditions a complete replacement of the narrower tabular sheets of rocks toward the center of the zone might develop a solid mass of ore in which the original fissuring would preserve a structure closely resembling true crustification, while, toward the outer limits, a plate or wall of barren rock would separate adjacent areas of mineralization. In cases of this kind it might easily happen that errors of interpretation would result in misleading conclusions regarding genesis, and the presence of valuable ore might remain unsuspected.

Crustification, or banding, is one of the typical features of mineral

deposition in pre-existing openings, but the type of deposit just described differs little in its general aspect from a true fissure vein.

And now, having indicated the problem, let us inquire how the microscope aids in its solution:

By study and investigation of many various types of replacement ore bodies geologists have detected certain common peculiarities, and have come to recognize certain criteria as being characteristic of metasomatic conditions. It is through observations of this sort, some structural and some microscopic, that questions of origin and genesis may be determined.

On examining a normal igneous rock in thin section some of the component minerals will be found in well-developed crystal form; in other words, they are idiomorphic toward others which show only an irregular outline.

In order that a pyrogenetic mineral may develop crystallographic faces on all sides it must have formed freely while suspended in a liquid of approximately its own specific gravity and without interference from neighboring crystals. Therefore the well-bounded minerals are believed to be early crystallizations from the magma. Iron oxides, apatite, and titanite, for example, are often idiomorphic.

But when we examine a rock, either igneous, metamorphic, or sedimentary, that has been mineralized, and find that the new mineral, introduced long after the rock was formed, has complete crystal outline, it is believed to indicate metasomotic replacement, since this theory of molecular interchange seems the only reasonable explanation. Had the new mineral started growth in the interstices between grains and forced them apart, the fact would be demonstrated by strain shadows developed in the adjacent minerals; or had the crystal formed in an open cavity, it would have been imperfect at its point of attachment to the walls. This evidence then, complete perfect crystal development of the new mineral in a rock, furnishes one important criterion of replacement. Its value, however, varies with the kind of rock in which the complete crystals occur, and other qualifying factors must be determined before reasonably safe conclusions can be reached.

When dealing with igneous rocks it must first be ascertained with certainty that the mineral is really secondary, introduced after consolidation, and crystalline form, alone, can be used as a criterion only with those minerals not known to form under igneous conditions.

With other more typical ore minerals the problem is somewhat involved, since, under certain conditions, though rarely, they may be original. But if it is found that the mineral is thickly disseminated

in the neighborhood of a crack and is not prominent in the normal rock, the fact of its secondary nature is established, and its relations to other grains may then be utilized to determine replacement; or, if the mineral cuts or intersects a quartz phenocryst in acid lavas, the criterion is of value, since quartz is here an early crystallization.

Metamorphic rocks present some difficulty, because even the original nature of the rock itself is obscure in many cases, and, unless the secondary minerals were introduced after the change and transformation were effected, very few indications will be found by which their origin may be determined.

But, if the new crystals intersect typical metamorphic minerals, or cut across the schistosity, and are entirely free from strain and fracture, it is reasonably certain that deformation of the rock was produced prior to mineralization, and the usual criteria may then be applied.

With the sedimentaries the problem is simpler. Most ore minerals are too soft and friable to resist the extreme mechanical attrition during the formation of the sands and rock, and it is sufficient to prove only that the mineral has not grown by forcing the layers or bands of rock apart, but has really formed at the expense of the rock material.

Another criterion of value lies in the preservation of original rock structures. A dolomitic limestone, for instance, may be replaced by silica and still preserve the minute, rhombic, dolomite crystals in their original form; yet polarized light will show that they are now merely a quartz aggregate.

Still another microscopic criterion that is a rather definite indication of replacement is the presence of perfect pyrite crystals or of quartz crystals, surrounded by a mass of the same or other ore minerals. The inclosed crystals may or may not be due to replacement, but the surrounding mass proves the gradual substitution of ore for rock. It must be demonstrated, however, that the deposit is not a magmatic segregation.

Suppose now we take up another problem connected with mining in the prospect stage or in early development. To vary the treatment, this discussion will apply more particularly to the fissure-vein type of deposit. Let us assume that a shaft has been sunk on the vein and that conditions have been exposed somewhat below the actual outcrop. A question of vital importance to the success of the mine, and one often submitted to the petrologist for an opinion, is the probability of extension with depth. What sort of conditions or what mineralogical changes may be expected at lower levels?

The factors to be determined and the questions which depend for their solution upon observations of mineralogic relationship and structural characteristics of the vein, are, first, the nature of the primary mineralization, and, second, the probable extent of enrichment through secondary processes.

Primary ore, while reasonably certain to extend to great depth, is not always of commercial grade, and pay ore is more likely to result from a reworking of the deposit by later solutions. Descending surface waters penetrate the vein and cause changes in the upper zone through oxidation and solution, with redeposition as secondary sulphides at lower levels.

The original formation of a vein is by no means the end of the operation; alteration of some kind is always going on. It is doubtful if any marked change could take place, however, without the presence of water, and, unless aqueous solutions have free access, this change is very slow. It is evident, then, that the extent of alteration depends largely upon the character of the vein material as regards permeability to solutions. Another factor of importance is the mineralogic nature of the gangue. For example, a gangue prominently calcic retards secondary enrichment, probably through neutralizing the active organic or mineral acids in solution.

Microscopic examination reveals all this, sometimes clearly, sometimes less definitely; but, even if an original mineral be wholly removed, traces often remain, or characteristic alteration products are produced by which the original may be identified. Permeability of vein and wall rock is determined by evidence of brecciation, fracture, or distortion of the constituents.

Figs. 1 to 5 illustrate the mineralogic changes that may be found in a vein from the surface downward. The sections were made from samples from the Otter vein, Patagonia district, Ariz.

Most minerals are known to be capable of formation under many different agencies, but there are a few which, by their very presence in an ore deposit, serve to indicate certain definite physical conditions, and, where these are present, conclusions as to genesis are simplified.

Tourmaline, for example, is not known to form from aqueous solutions in the ordinary way, but is a typical pneumatolytic mineral which has been formed from vapors and under high temperature. Garnet is another, but it is particularly indicative of contact metamorphism.

Ores of this type, formed by deposition from gases and vapors, usually above the critical temperature and pressure of water given

off by a rock magma during solidification under deep-seated conditions, are rarely of economic importance, and, where their original character is still preserved, with primary sulphides at or near the surface, it is rather useless to look deeper for profitable ore.

In a study of any class of ore deposit an examination of the associated rocks is always necessary. Wherever mineralization has been extensive enough to form profitable deposits the rocks will be found considerably altered, not by ordinary surface change, weathering and oxidation, but through reactions occasioned by the mineral-bearing solutions themselves. Where rocks are fresh, mineral deposits will not, as a rule, be found worth developing.

Figs. 6 and 7 are micro-sections illustrating rock alteration.

In discussing the field of usefulness of the microscope during active mining operations it will be evident that the problems are of a somewhat different nature from those we have been considering. Questions of genesis and origin are no longer of real economic importance. The ore is exposed to some depth, and has been explored along the various levels and has been blocked out. Its quantity can be estimated with some accuracy and its metallic content ascertained by systematic sampling and assay; and since the zone of secondary enrichment may have been passed through, the primary mineralization may be exposed for study.

The success with which microscopic methods may be used to aid in solving the new problems is, however, just as definite, but in this case the work is more one of identification than one of interpretation. The structural geology of the district and of the mine become important factors.

The most economical system of stoping, the proper location for shafts and tunnels, the distance between levels and other purely engineering problems depend upon geologic structure, and upon the nature of ore and rock formations.

These factors are best determined by a careful survey of accessible workings and the compilation of accurate mine maps showing all geologic features. A certain dike, fault, or ore body, for example, may be plotted and correlated through similarity of microscopic characteristics with another exposure on a different level. Its strike and dip can then be computed, and its probable location at a desired point determined.

With these data the miner is enabled to reach the particular formation with the least possible excavation; or avoid it, should this be necessary. But, if the identification be faulty and general appearance alone be depended upon, it may happen that two separate and distinct

FIG. 1.—VEIN QUARTZ NEAR OUTCROP, SHOWING FRACTURE IN VEIN INDICATING DYNAMIC STRESS, RECEMENTED BY SECONDARY QUARTZ AND HEMATITE. × 35.

FIG. 2.—VEIN COMPOSITION AT DEPTH OF 20 FT. REMNANTS OF PRIMARY CHALCOPYRITE SURROUNDED BY SECONDARY HEMATITE. × 35.

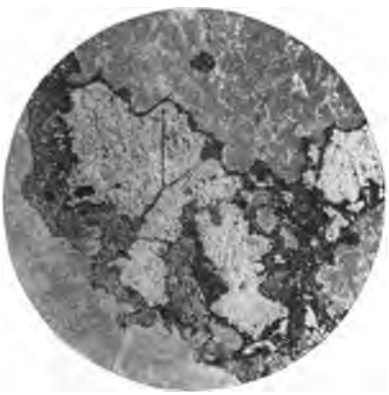

FIG. 3.—VEIN MATTER AT DEPTH OF 40 FT. PRIMARY CHALCOPYRITE PROMINENT, SURROUNDED BY RIM OF OXIDES. × 35.

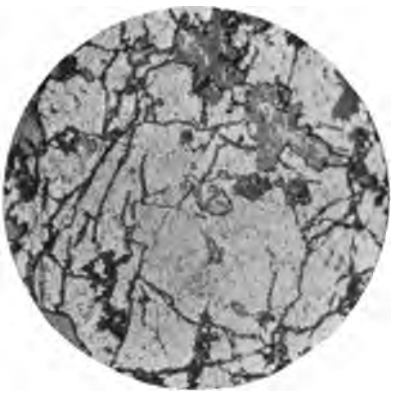

FIG. 4.—VEIN AT 50 FT. BENEATH THE SURFACE. SHOWS ABUNDANCE OF CHALCOPYRITE GREATLY FRACTURED, WITH SLIGHT ENVELOPE OF OXIDES. × 35.

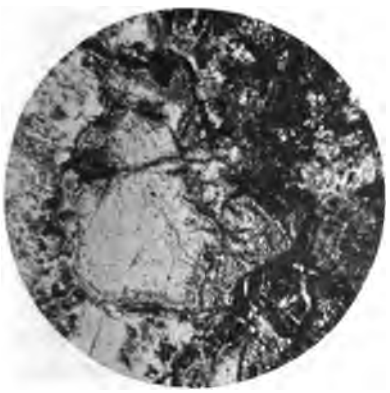

FIG. 5.—VEIN AT DEPTH OF 60 FT. PRIMARY CHALCOPYRITE SURROUNDED BY SECONDARY SULPHIDE, COVELLITE, AND AN OUTER RIM OF OXIDES; EVIDENTLY THE BEGINNING OF SECONDARY SULPHIDE ZONE. × 35.

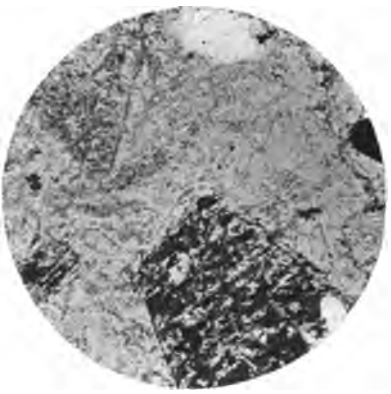

FIG. 6.—QUARTZ-PORPHYRY, GROUNDMASS DEVITRIFIED, BIOTITE CHANGED TO CHLORITE AND FELDSPARS KAOLINIZED. × 35.

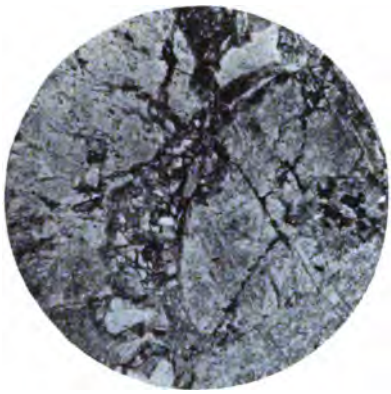

FIG. 7.—SYENITE SHOWING MICROSCOPIC CRUSHED ZONE. × 35.

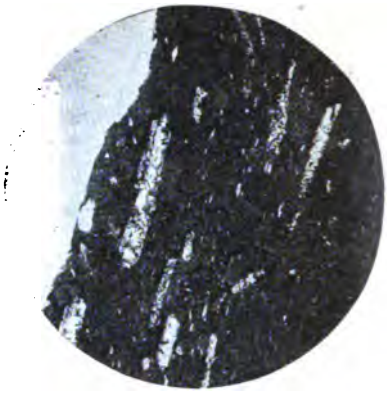

FIG. 9.—MICRO-SECTION OF A 2-IN. STRINGER AS SHOWN IN FIG. 8. FELDSPARS SHOW IDIOMORPHIC DEVELOPMENT, THE GROUND-MASS MERELY A BASIC GLASS; FLOW STRUCTURE MARKED. × 35.

FIG. 10.—MICRO-SECTION FROM A 20-FT. DIKE OF TYPICAL CAMPTONITE INTRUSIVE IN THE FRANKLIN LIMESTONE AT FRANKLIN FURNACE, N. J. AUGITE, BIOTITE, AND TITANITE IN A FELDSPAR AGGREGATE. × 35.

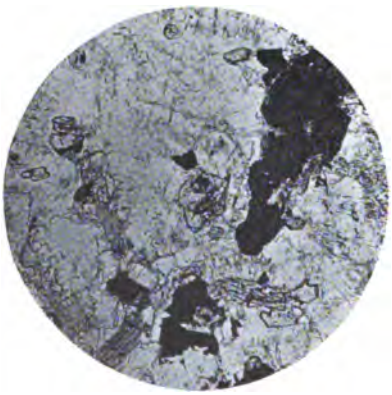

FIG. 11.—SECTION OF MICRO-DIORITE FROM SOUTHERN ARIZONA. HORNBLENDE, AUGITE, APATITE, AND MAGNETITE IN A FELDSPAR GROUND-MASS. × 35.

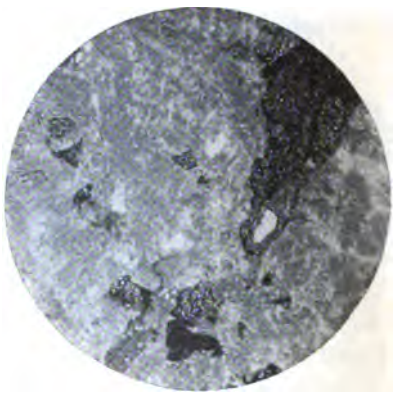

FIG. 12.—SAME AREA AS FIG. 11 BY SURFACE ILLUMINATION, SHOWING CRYSTAL OF NATIVE SILVER IN THE MAGNETITE. × 35.

features will be assumed to be identical. Location on this basis may prove to be in error by hundreds of feet. These problems become particularly important in regions where the structure is complex.

Figs. 8, 9, and 10 illustrate changes in rock texture produced through differences in the rate of cooling.

The identification of rocks and ores, then, is one of the fields of usefulness for the microscope in the operation of producing and developed mines, and it was the accuracy with which rocks could be identified and correlated by petrographic methods that led to one of the first practical applications of it in an actual mining problem.

FIG. 8.—STRINGERS OR OFFSHOOTS FROM THE INTRUSIVE MASS PENETRATING CRACKS IN THE LIMESTONE ADJACENT TO THE MAIN DIKE.

The particular question involved related to the identity of certain formations in the Eureka district, Nevada, and to the definition of the terms "vein" and "lode" as used in the U. S. Mining Statutes.

A body of mineralized limestone in Ruby Hill, near Eureka, had been located and mining operations conducted for some time. Unfortunately, a dispute arose. One company, which had followed down a more or less connected vein within the limestone, claimed ownership of a certain mass of ore on the 5th level, by virtue of the law of extra-lateral rights; the other company contended that the limestone itself was a lode as defined by the statute, and therefore

that they were entitled to that portion of the ore located on their side of a compromise line between the two claims.

During litigation it developed that a definite foot-wall of quartzite formed one boundary of the mineralized zone and that a shale and limestone formed a definite hanging-wall. The rock embraced between the two differed in its characteristics from all other limestone in the vicinity. It was extensively altered; crushed, brecciated, and recemented by calcareous matter and silver-lead ores. No traces of stratification remained.

These facts, demonstrated by microscopic methods, helped to establish that the mineralized Prospect Mountain limestone itself constituted a lode or vein of ore in place.

Besides the usual results to be derived from this sort of work, quite unexpected facts of profitable import are sometimes brought to light. A case that came to notice not long ago serves to illustrate the point:

Having had occasion to determine the nature of a mineral occurrence in southern Arizona, a microscopic investigation of the ore was undertaken. Samples of the neighboring rocks also were taken, to help in the solution. One of these proved to be a diorite, of normal appearance at first sight, but, on more careful examination, it was discovered that throughout the rock were some peculiar opaque white crystals, perfectly developed, and generally surrounded by magnetite. They were identified as native silver. An assay of the rock, after careful sampling, gave an average silver content of 7.5 oz. per ton. The silver value might easily have been overlooked except for its accidental discovery in thin sections. (See Figs. 11 and 12.)

The last phase of the subject which will be considered relates to metallurgical treatment of the ore after reaching the surface. In many cases the success of a mine depends on the solution of this very problem, how can the ore be most economically treated to extract its values. Here again the microscope may profitably be utilized to assist in the solution.

The nature of the work differs from either of the two previous classes. The important things to be determined relate to the texture of the ore, the nature of the mineral aggregate, the size of grain; in short, to the answer to the question how, and not why, are the minerals related to one another.

For example, let us consider a recent milling problem at a Canadian gold mine. Upon microscopic examination of the ore it was found that most of the gold was closely associated with pyrite, as an inclusion or intergrowth, in a finely divided state. An appreciable proportion, however, occurred in the quartz gangue separate from

other minerals. It was justifiable to anticipate that, with this proportion, amalgamation would accomplish a good recovery, but that, with the other and principal part of the gold, amalgamation would prove ineffective without extremely fine grinding. Hence a combination with the cyanide method suggested itself as a solution of the problem.

Actual mill tests along this line proved the value of these observations. A preliminary study might easily have saved a considerable amount of money spent in unprofitable experiments.

Or take the case of disseminated iron in schists and gneisses, a class of ores that is coming into prominence. Their value and importance lie wholly in the ease of concentration through magnetic separation. But if, instead of the normal magnetite, part of the iron is present as martite, a mineral which is only slightly magnetic and yet which resembles magnetite closely in general appearance, it may readily be seen how the ore might prove a very unprofitable thing to handle.

Polished sections studied with vertical illumination would reveal the mineral character and would suggest the difficulty of concentration, notwithstanding the fact that a chemical analysis might show a favorable percentage of iron.

When deciding upon methods of concentration for any ore, microscopic examination will, in general, indicate which process will be most likely to give the best results. If two or more components are found to be very closely associated and minutely intergrown, chemical treatment, as a rule, will prove most effective, since fine grinding usually introduces difficulties; while an ore with a comparatively coarse-grained fabric will prove more susceptible to mechanical separation.

One recent development in the preparation of ores for the market, the sintering of fine concentrates, flue dust, and other by-products to render them amenable to furnace treatment, is worthy of notice. Many devices have been invented to accomplish this result, and, when comparing the qualities of the sintered product from the various processes to determine the most available treatment for a certain class of material, microscopic characteristics will be found of no little value in arriving at conclusions.

In this way may be determined, for instance, whether the material is sufficiently porous to permit the ready access of furnace gases, or whether the cohesiveness of the product is due to the formation of refractory silicates which would retard reducing action.

The study of mattes, slags, and speiss is another field, but one in which the problems are within the province of the metallurgist rather

than the mining engineer; nevertheless it is an unusual problem that may not be at least simplified by microscopic investigation.

In conclusion, it may be of interest to note that petrographic methods were first applied less than 60 years ago, and, when we realize the great development of the science and its wide application to various lines of research since Dr. Sorby in 1858 published his first paper on the microscopic structure of crystals and of rocks, the possibilities of future development may be the better appreciated.

Discussion.

L. C. Graton, Cambridge, Mass.:—I am perhaps one of the few present who are exceedingly interested in the details of microscopic work, but I am sure we have all enjoyed very much the general features of this paper. I will not attempt to express my own feeling in regard to it beyond saying that it seems to me that this kind of microscopic investigation and study has been overlooked and slighted and that its possibilities are very great.

There are one or two details in Mr. Apgar's paper which impressed me because they were novel to me: one of these was the development of secondary galena with dull luster. None of the galena which I have seen has seemed to be secondary and I have never seen any, whether secondary or primary, that did not have a bright luster.

Kirby Thomas, New York, N. Y. (communication to the Secretary *):—The present-day mining engineer is expected to "see into the ground farther than the point of the pick" and he is justified in making free claim to his ability to do so. Mr. Apgar has recorded the development of the application to the work of the engineer of a very useful but tardily recognized instrument—the petrographic microscope, an instrument which is helping to shatter the proverbial "point of the pick" limitation—and he has shown its use and its possibilities very clearly. Some further field examples of the use of the microscope in engineering problems may add to the appreciation of Mr. Apgar's scholarly paper. In exploration by diamond drills in the Sudbury, Ontario, nickel district it is often of great importance to determine when the drill has passed out of the norite and into the foot-wall formation. Structural calculations may give a clue to the expected point of contact, but the variations are considerable, and since to stop the drill before reaching the contact is futile, and to continue beyond it is needless, it becomes of theoretic and economic importance to determine positively the lithological nature of the core

* Received Sept. 22, 1913.

at critical stages in the work. In some places the foot-wall is an earlier norite, only distinguishable from the nickel norite (Sudburyite) by means of an examination of the rock slides with a petrographic microscope. Again, certain diabase dikes which cut the norite and the foot-wall formations cannot be easily or with certainty distinguished megascopically from the norite. The determination, only possible with the high-power microscope, that the often highly altered rock gangue associated with outlying pyrrhotite masses in the Sudbury district is a phase of the nickel norite, is of evident importance in view of the fact of the invariable association of the norite and the nickeliferous pyrrhotite. From these random examples from actual working notes, it is evident that the microscopic laboratory is often quite as essential to the engineer in charge of explorations as is the assay laboratory to the metallurgist or to the miner.

Another point of application of the microscope should be considered. Often it is of no great importance, from a commercial point of view, that the exact species of the associated rocks of a mineral deposit should be determined, but the engineer owes a recognition to the science on which his profession is founded which should restrain him from incorrect, careless, or indefinite rock nomenclature in his reports and records. A few "standby" rock names, frequently wrongly used in mining literature, often cover obvious ignorance or indicate culpable carelessness. It is not to be expected that the busy field engineer can offhand "read" all the multitudinous rock species, but he can obtain a slide of the important rock formations he describes at a very insignificant cost, and likewise can have a lithological determination made by a specialist for a nominal charge, if desired.

Method of Testing Draeger Oxygen Helmets at the Copper Queen Mine.

BY C. A. MITKE, BISBEE, ARIZ.

(Butte Meeting, August, 1913.)

1. *Character of Gases which Caused Helmets to Get Out of Order.*

DURING September, 1911, the fire area in the Lowell mine continually increased and gases resulting from the fire came through the upcast shaft. These gases contained such a large percentage of sulphur dioxide that the men were unable to go up and down on cages, and, within a comparatively short time, repairs in the shaft were found to be necessary.

Since the fire was in sulphide ores and extended into certain timbered sections, the gases contained varying amounts of SO_2, CO_2, and perhaps, at times, CO. The temperature in the shaft was generally about 97°, and the relative humidity from 97 to 99.5 per cent. The corrosive action of the fumes became apparent when the cables had to be changed more often than usual, the heads of lag screws in the guides of the shaft disappeared, and the cages needed frequent repairs. It also became necessary to repair two compartments of the shaft by putting in 150 ft. of new guides between the 600 and 800 levels and lag screws in nearly all the old guides from the surface to the 800 level.

2. *Ordinary Methods of Testing Helmets which were Unsatisfactory.*

These conditions required all work to be done by men wearing oxygen helmets. The Draeger type was tried, and was used until all repairs were completed. At first the helmets were tested for air circulation and air tightness and the injector was tested by water gauge according to the methods recommended by the Draeger Oxygen Apparatus Co. Six arrangements, covering these different tests, are illustrated in the catalogue and circulars of the company.

After the above tests had been made on the oxygen apparatus, the men still complained about leaky helmets. Small quantities of gas entered the helmets and could not be detected by these methods. The

tests will not reveal any small leaks in the helmet or breathing bag, but test only the strong parts of the apparatus which seldom get out of order and not the weaker parts, such as the inflation tube, the handle of the cleaning sponge, and the breathing bag, which frequently needed repairs.

Aside from these objections, it was not practicable to spend a large amount of time in making a number of tests when the oxygen helmet

FIG. 1.—PRESSURE TEST OF DRAEGER OXYGEN HELMETS.

apparatus was used by both the day and the night shift men. It was therefore desirable to find some device which would test both the weak and the strong parts of the apparatus within 2 min.

3. *The Method Used.*

The following method was used at the Copper Queen mine and was found to fulfill the necessary conditions. Fastened to the lid of each box containing a complete set of apparatus was a card bearing the

determined pressure and suction records, similar to the following example:

Box,	No. 1.	Check,	10.5 cm.
Helmet,	No. 5,000.	Pressure tube,	4⅛ inches water gauge.
Apparatus,	No. 5,500.	Suction tube,	4 inches water gauge.

The testing device consists of a wooden block, *A*, Fig. 1, which fits into the helmet, taking the place of a man's face, and is provided with a water gauge on the side next the operator, for recording total pres-

FIG. 2.—SUCTION TEST OF DRAEGER OXYGEN HELMETS.

sure and suction. The device is mounted on a stand, *B*. There is an adjustable connection between block *A* and stand *B*, so that the helmet can be fitted accurately to the block.

To make the pressure test, first connect the helmet to the apparatus, then lift it over block *A* and fit it on, as shown in Fig. 1. Inflate the tube inside the helmet by pressing the bulb. Remove the potash cartridge. Open the upper valve by pressing the thumb against the check spring and turn on the oxygen. Within 10 sec. the breath-

ing bag should be filled up and the water gauge should show a pressure of 4 in. (reading for helmet in box No. 1). The quantity is also determined if the breathing bag is filled in this time and has the required pressure.

To make the suction test, put back the potash cartridge, release the tube from the oxygen cylinder, and place the hand firmly on the opening of the breathing bag, as shown in Fig. 2. Turn on the oxygen and after 10 sec. read the water gauge, which should, in this case, indicate 4 in. If there are small leaks anywhere in the entire apparatus it will be evident from the low pressure or suction reading of the water gauge.

Generally the readings found in testing the helmets by the methods described above will be much lower than those determined by attaching the water gauge directly to the oxygen tube from the injector, because of minor leaks or escape through the safety on the breathing bag.

The test is conclusive and indicates all leaks in the weaker parts of the apparatus, such as the inflation tube, the handle of the cleaning sponge, the breathing bag, and all connections. The total time required for making the complete tests is about 2 min. This method of testing the entire apparatus each day was satisfactory in every respect.

Preparation of Ore Containing Zinc for the Recovery of Other Metals Such as Silver, Gold, Copper, and Lead by the Elimination and Subsequent Recovery of the Zinc as a Chemically Pure Zinc Product.

BY S. E. BRETHERTON, SAN FRANCISCO, CAL.

(Butte Meeting, August, 1913.)

THIS title introduces the subject I wish to describe to my fellow members, very few of whom, I hope, have ever had as much trouble with the smelting of ore containing much zinc, either in the lead blast furnace or the matting furnace, as circumstances have forced upon me: First, for several years in Leadville, Colo., after the sulphide ores containing zinc had been developed with greater depth and were sold to the interests with which I was connected for smelting in the blast furnace for the recovery of the lead, silver, and gold; then, in New Mexico and Arizona, where the principal source of ore supply for our custom smelter was from miners who sent most of their product in the form of concentrates, which we briquetted and smelted in the blast furnace, producing a matte containing copper, silver, and gold, for which we were paid; together with some zinc and lead, which was penalized if in excess of a maximum base set by the refinery to which we sold our copper matte; last, but not least, my experience in Shasta county, Cal., where for the sake of recovering $13 per ton value in copper, silver, and gold, I have smelted ore containing as high as 40 per cent. zinc (the last 18,951 tons smelted averaged $15\frac{3}{10}$ per cent. zinc) which we not only lost, but which it cost us money to lose.

After such an experience, and knowing that, instead of being worth only $13 per ton, such 15 per cent. zinc ore should be worth about $30, including zinc at the present market value, the question of recovering the zinc in a profitable manner from such ore became very interesting to me. During several years' time I sent samples, of from 5 to 1,200 lb. each, to be tested by the different best-known zinc-recovery plants, and experimented with several processes proposed by others for the treatment of such ore, with but little encouragement, until I

tried leaching with ammonia and carbon dioxide. I referred to this process and to my objections to other processes in the *Mining and Scientific Press* of April 12, 1913. The discouraging feature of this process was that, after using it at the Hoboken works, near Antwerp, for several years, it was reported as being abandoned, although it was claimed that the loss of ammonia was suprisingly small. Since we have worked this process on a somewhat larger scale than laboratory tests, I am not surprised at any trouble they may have encountered when leaching zinciferous oxides left after cupellation; in fact, to get the proper roast is the most delicate part of the operation.

My first attempt to leach with ammonia and carbon dioxide (NH_3 and CO_2) was made upon flue dust containing 30 per cent. zinc, just as it came from the dust chamber without any preparation, with the result that an extraction of 72 to 78 per cent. of the zinc was obtained. This I considered good for the reason that some of the zinc in the flue dust is there carried over mechanically as fine particles of insoluble ZnS. I then had some ore roasted in a custom assay office, and attempted to leach it, but with only 56 per cent. zinc extraction. The trouble in this test was that the ore had been roasted at too high a temperature, so that some of it was partly sintered or fused. Knowing the cause of this poor extraction, I was not discouraged, but discontinued my experiments because my time was too fully occupied with other matters. However, I suggested to some persons who had a large quantity of flue dust rich in zinc that they try out the ammonia process, which they did with very good results as to the zinc extraction, but the zinc oxide product was comparatively worthless, due to the arsenic content which had been originally in the siliceous ore used in the blast furnaces for flux; and due to concentrating the arsenic from these ores into the flue dust and a further concentration when it was extracted with the zinc in the form of oxides. This and more recent experiences have proved to us that, while the ammonia process is suitable—and I feel safe in saying the most suitable process—for the treatment of the zinc ore of Shasta county, Cal., where I have not found more than traces of arsenic in such ore, the process is not suitable for fused products or ore containing much arsenic or zinc silicates, the latter being insoluble.

The following results were reported by our Metallurgical Chemist, F. L. Wilson: First, by what we call the bottle test, that is, taking 100 g. (about 3½ oz.) of the finely ground, roasted ore from the Afterthought mines of Shasta county, Cal., and agitating in an ordinary 6-lb. acid bottle on rollers for 20 hr. for leaching and then for 4 hr. for washing. The results of these tests are shown in Table I.

TABLE I.—*Preliminary Bottle Tests.*

Roast.	Contents. Per Cent.		Extractions, Per Cent.			
			Solution.		Residue.	
	Zn.	Cu.	Zn.	Cu.	Zn.	Cu.
Sulphatizing. Lime added 20 minutes before finish,	20.80	2.12	83.00	49.10	84.60	43.90
Sulphatizing. Lime added at beginning,	20.04	2.71	95.00	60.33	83.40	56.10
Sulphatizing. Gypsum added at beginning,	19.04	2.35	85.68	62.77	83.14	51.35
Sulphatizing. 2 hr. roast,	21.38	3.00	88.30	58.66	83.55	62.40
Sulphatizing. 3 hr. roast,	35.98	0.93	100.0	83.2	87.52	85.37
Sulphatizing. 3 hr. roast,	36.09	3.27	95.32	70.27	91.44	67.89
Sulphatizing. 3 hr. roast,	33.40	2.31	92.60	83.4	92.10	80.90
Sulphatizing. 3 hr. roast,	36.86	1.28	93.60	52.0	94.40	88.30
Sulphatizing. 3 hr. roast,	19.70	2.67	93.80	60.9	89.13	71.16
Sulphatizing. 3 hr. roast,	21.0	2.58	86.38	53.13	81.33	30.00

Second, by a continuation of the tests with 25- to 50-lb. lots, concluding with tests made under pressure in 8 hours' time for leaching, precipitating copper and washing, instead of 24 hr. The data are given in Table II.

TABLE II.—*Tests on 25- to 50-lb. Lots.*

Roast.	Contents. Per Cent.		Time Hr.	Extraction-Residue Assay.		
	Zn.	Cu.		Zn.	Cu.	
Sulphatizing. 3-hr. roast,	21.3	2.99	24	82.16	50.83	.....
Sulphatizing. 3-hr. roast,	21.3	2.99	24	82.4	53.54	.....
Sulphatizing. 3-hr. roast,	21.7	2.77	24	83.73	57.76	.....
Sulphatizing. 3-hr. roast,	21.7	2.77	24	81.84	50.0	.....
Sulphatizing. 3-hr. roast,	21.5	2.40	8	81.8	.....	Pressure.
Sulphatizing. 3-hr. roast,	22.12	.....	8	80.0	.....	Pressure. Improper roast.
Sulphatizing. 3-hr. roast,	21.5	2.43	8	79.4	53.7	Pressure. Improper roast.
Sulphatizing. 3-hr. roast,	21.8	.....	8	87.46	.....	Pressure. Correct roast.

In Table I., over the two columns showing zinc and copper extraction is marked solution, then residue; this is simply to show that it was by determining the zinc in the residue that the results were finally obtained; assaying the solutions was a quicker method and showed a higher percentage of extraction, but the results were not so reliable. The copper extraction was not as large, but in our case, where we intend to smelt the residue for the remaining copper with the silver and gold, the less copper extracted the better. Where the copper extraction also is sought, more time is necessary for leaching.

In order that the members of this Institute will more readily understand this process, with its advantages, I will briefly describe it: The

ore is first crushed to less than walnut size and ground, either wet or dry, in a Hardinge ball mill, to about 30 mesh; roasted in a Wedge type of roasting furnace at comparatively low temperature to about 7 per cent. sulphur, the object being to oxidize the zinc and form as much zinc sulphate as possible. (This crushing and roasting would have to be done for any method of treatment; in fact the high-grade concentrates have to be roasted for retorting purposes until all the sulphur possible is eliminated.) After roasting, the ore is ground in an ordinary pebble mill to 200 mesh—or perhaps I should say slimed. The slimed ore is now agitated in a solution containing preferably about 9 per cent. ammonia and 9 per cent. carbon dioxide, as stated by Schnabel in his hand-book of metallurgy. After sufficient agitation has been given the pulp for proper extraction, it is allowed to settle and the solution drawn off by decanting or filtering, or both; the pulp is properly washed to avoid mechanical loss of ammonia. The residue is smelted by the ordinary reverberatory type of matting furnace; in some cases cyanidation was adopted successfully where copper does not interfere, since the sliming and sweetening will certainly have been accomplished and only 50 to 60 per cent. of the weight of the original ore remains to be treated, depending on the original zinc and sulphur contents.

It is not necessary to go further into the treatment of the residue in this article, except to state that in washing we found it necessary to wash, first, with a solution containing ammonia and carbon dioxide to avoid precipitating some of the zinc as a basic carbonate before it was washed out; then wash out the ammonia with water, steam, boiled solution, or air. The first wash solution, rich in ammonia and carbon dioxide, containing some zinc, can go direct to the agitation tanks for leaching purposes; the second wash water can be brought up to strength with ammonia and carbon dioxide, which has been boiled over from the still; the other wash waters can be used over again for washing, and the last wash waters can go to the pebble mill. Enough fresh wash water will have to be added to each fresh charge of ore to make up for the mechanical loss carried off in the residue, and no more.

I will now refer back to the solution containing zinc and some copper, as it comes from the agitation tanks. To precipitate the copper from this we pump it through a long upright tank containing sheet zinc, either old scrap purchased from the junk shop, or made by ourselves locally. The copper is allowed to settle, drawn off, filtered, washed, and thrown into the matting furnace with the residue. After the solution is free of its copper, it is forced into the still—a battery

of conical-bottomed tanks—where the free ammonia and carbon dioxide are distilled off into absorbers and a basic zinc carbonate allowed to settle, which is drawn off and filtered. Very little washing is necessary for this zinc carbonate for the reason that any free or combined ammonia and carbon dioxide is recovered from the Wedge muffle furnace when the zinc is calcined to zinc oxide. We find this zinc oxide almost chemically pure and ready to market.

In order to make our own sheet zinc for precipitating copper and take advantage of the special high price of such pure spelter, we intend to convert part of our zinc into metal direct from the basic zinc carbonate, thus avoiding the expense of calcining. The carbon dioxide as liberated is conveyed back to the absorbing tanks for future use, and any ammonia that may be present is taken up with the H_2SO_4, to be recovered later by the addition of lime. The great difficulties of precipitating zinc, especially pure zinc, from an acid solution, which allows other impurities to go down with the zinc, we have overcome in the following manner: Dissolve the zinc carbonate or zinc oxide into solution of proper density, then maintain this standard solution by feeding automatically the granular precipitate of basic zinc carbonate fast enough to supply the zinc as precipitated and neutralize the SO_3 (sulphur trioxide) as liberated. The fact that our basic zinc carbonate contains no impurities to be precipitated enables us to make a very pure spelter.

We find that the ore from the Afterthought properties, Shasta county, California, roasts and leaches very well. Also from another ore, containing 2.1 per cent. copper, 6.5 per cent. lead, and 10 per cent. zinc, which was sent us from Sinaloa, Mexico, we were able to get an extraction of 73 to 80 per cent. of the zinc, most of the lead remaining in the residue.

I believe the expense of installing and operating a plant for this process will be less than with the ordinary methods of treating even such zinc ores as those suitable for concentration before roasting and smelting in the retort furnace. The cost of the crushing and roasting plant would be the same in each case. We have the advantage in not requiring such a "sweet" roast. Then it is a question of tanks, pumps, filter presses, and steam boiling-out apparatus in our case, as against the small capacity of clay retorts, special clay, and expensive fuel required with the retort furnaces. In our case we have practically all the silver, gold, and copper from the original ore in the residue and only about 50 per cent. of the original ore to be smelted, whereas, with the retort furnaces, there is such a loss of the precious metals by volatilization and the cinder from the retorts is so badly

mixed with ash from the coal used, broken retorts, etc., that it is often too low in value to pay the smelting charge for the recovery of these metals.

In addition, we are also able to treat ores that so far have not proved suitable to other processes, and we have a higher recovery of the zinc from the original ore, viz.: 90 to 95 per cent. from a 35 per cent. zinc ore; 85 to 90 per cent. from a 25 per cent. ore; 80 to 85 per cent. from a 20 per cent. ore, and from 73 to 80 per cent. from a 10.4 per cent. zinc sulphide ore containing some lead and copper. Iron and wooden covered tanks answer the desired purposes, but it is necessary to avoid the use of copper, brass, or zinc in the construction of this plant.

Since we first started to experiment with this process for the recovery of zinc and preparation of the ore for further treatment, we have found that, instead of requiring nearly 24 hr. to leach by agitation, filter, and wash, we are now able to obtain equally as good or better extraction in less than 8 hr. by agitating under pressure, and we use less solution. In this connection it may be well to state that several patents have been applied for in the United States and foreign countries. The cost of operation can be estimated as being the same as preparing the ore for cyaniding, plus the cost of roasting in our case. The cost of leaching is about the same as cyaniding, except that less time is required. The steam for boiling out the ammonia would be extra, but this can be furnished by the waste heat of the reverberatory matting furnace, as they are now generating steam for power purposes at the large smelting plants in Montana and elsewhere. The loss of chemicals is about the same as for cyaniding. In our case it is ammonia, which we find to be about 1 lb. less per ton of ore treated, and lime, 20 to 50 lb. of which are used. The loss of carbon dioxide is not considered, as it is obtained when we make the burnt lime; in fact, what little burnt lime we use is afterwards utilized in the matting furnaces as flux. The pure acid we would use for making metallic zinc, should we decide to do so, instead of marketing the zinc oxide, is used over again continuously, less some loss as sulphate of lime when recovering the fixed ammonia. The cost of smelting the residue in the matting furnace is comparatively low, on account of the elimination of the zinc and sulphur; in fact, we have to add a little raw sulphide ore to furnish sufficient sulphur for matting purposes, and we then smelt siliceous gold ore to flux the iron which has been concentrated by the removal of the zinc. By having this roasted residue and fine ore, we are able to adopt the reverberatory type of matting furnace, using only the cheaper fuel oil for matting purposes and avoiding the use of either coke or coal.

It seems strange to me that those persons who used ammonia and carbon dioxide for the removal of zinc ozide from fused products in the metallurgy of silver, where impurities conflicting with the process must have been encountered, did not adopt this process for the extraction of zinc from ores for the recovery of that metal and for preparing the ore for the recovery of other metals. I mention fused products for the reason that we attempted to leach the zinc from slags containing a high percentage of zinc with no satisfactory results, and also found that ore roasted at an extremely high temperature did not leach well; in fact, we have had to depart in detail from many of the ideas suggested to us by Schnabel, Lunge, and others. The process may have been tried on ore not suitable, or if suitable, the experiments may not have been given proper attention.

Discussion.

S. E. Bretherton, San Francisco, Cal.:—Since preparing my paper, our chemist, F. L. Wilson, has been experimenting with Leadville, Colo., oxidized zinc ore and Leadville zinc sulphides containing lead, operating upon 40-lb. lots at a time. His results on Leadville sulphides are as follows: The raw ore contained a trace of gold, 5.04 oz. of silver, 19.1 per cent. of zinc, and 7.24 per cent. of lead. After roasting the ore contained a trace of gold, 5.72 oz. of silver, 0.75 per cent. of copper, 21.7 per cent. of zinc, and 8.29 per cent. of lead. The loss in weight by leaching was 38.8 per cent. The extraction was 42.8 per cent. of the copper, 80.6 per cent. of the zinc, 7.1 per cent. of the lead, 0.64 per cent. of the silver, and no gold. The little silver that is extracted would be thrown down with the copper when precipitating it from the zinc and both thrown back into the residue to be smelted. There would be no trouble in smelting the residue with nearly all the sulphur eliminated and containing less than 7 per cent. of zinc. On zinc carbonates containing silicates the results are not satisfactory.

H. O. Hofman, Boston, Mass. (communication to the Secretary*):—The paper by Mr. Bretherton upon the application of ammonia and carbonic acid to the recovery of zinc as oxide from roasted sulphide ore is of interest to me, as the first installation of the Schnabel process for the extraction of zinc from a mixture of zinc oxide, lead oxide (with some cupric oxide), and pellets of enriched lead, produced in steaming liquated zinc-silver-lead crust of the Parkes process, was made at Lautenthal, Harz mountains, where I was chemist. In that capacity I carried out numerous experiments on the Schnabel process,

* Received Aug. 15, 1913.

was active in the erecting of the plant, and later had charge of it. After the Lautenthal plant had been in operation for some time a similar one was built at Hoboken-les-Anvers, but both have been abandoned, as the wet process could not compete with the distillation of the liquated zinc-silver-lead crust, now used universally in the Parkes process.

The experience of Mr. Bretherton in not being able to extract a satisfactory percentage of zinc from an ore which had been partly sintered during the preparatory roasting is not surprising, as it is one which is frequently encountered even when an ore has been only overheated. Mr. Laist called attention to this fact in his paper[1] on the lixiviation of concentration tailings at Anaconda.

The analysis of the material treated at Lautenthal as quoted by Schnabel[2] gives: Pb, 30.065; PbO, 36.87; Zn, 1.9; ZnO, 23.24; Cu, 1.24; Ag, 1.855; Bi_2O_3, 0.44; Sb_2O_3, 0.57; Fe_2O_3, 3.82 per cent. There is no siliceous gangue to cause sintering, hence the zinc oxide, after having been heated to a light cherry red, say 850° C., remained readily soluble in ammonium sesquicarbonate.

Some of the tests upon the solubility of zinc oxide in ammonia and ammonium sesquicarbonate which I carried out have been published by Schnabel[3]. I give them in Tables I and II, as they may be of service in the working out of the proposed process.

TABLE I.—*Solubility of Zinc Oxide in Solutions of Ammonia.*

100 g. solution with g. NH_3	20	15	10	9	8	7	6	5	4	3
Dissolve at 14° C. in 24 hr. g. ZnO	1.78	1.75	1.51	1.34	1.12	0.91	0.76	0.56	0.45	0.25

The solubility of zinc oxide in caustic ammonia is too small to be of any practical value.

TABLE II.—*Solubility of Zinc Oxide in Solutions of Ammonium Carbonate.*

100 g. Solvent Containing, g.		Dissolve in 24 hr. at Room Temperature g. ZnO.	100 g. Solvent Containing, g.		Dissolve in 24 hr. at Room Temperature g. ZnO.
NH_3	CO_2		NH_3.	CO_2.	
6.49	2.70	3.60	6.17	7.94	9.85
6.44	3.36	6.45	6.13	8.61	9.95
6.34	5.07	8.65	6.05	10.07	10.65
6.25	6.02	9.95	6.03	10.47	9.95
6.28	6.06	9.75	5.91	12.63	7.95
6.19	7.59	10.05	5.90	12.81	7.35

[1] *Trans.*, xlvi., 362 (1913).

[2] Schnabel, C—Louis H.: *Handbook of Metallurgy*, vol. i, p. 677 (1905).

[3] *Zeitschrift für das Berg- Hütten- und Salinenwesen im preussischen Staate*, vol. xxviii, pp. 279 and 280 (1880).

The solutions were prepared by passing carbon dioxide through ammonia liquor with 6.664 per cent. of NH_3 for different periods of time; the CO_2 contents were then determined. The table shows that for 6 per cent. ammonia liquor the greatest dissolving power for zinc oxide lies between 6 and 10 per cent. CO_2; 1 g. of NH_3 will dissolve about 1.5 g. of ZnO. Lengthening the time for solution from 24 to 36 hr. showed little increase in the amount of ZnO dissolved; raising the temperature to 30° C. had a more favorable effect. In the plant the standard solvent contained 9 per cent. of NH_3 and 9 per cent. of CO_2.

The manner in which the Schnabel process was carried out at Lautenthal is described on pp. 679-680 of Schnabel's treatise already quoted.

The ore was leached in a horizontal boiler-iron drum with revolving shaft provided with arms. In fact, every apparatus in the mill was a closed iron vessel strong enough to withstand considerable pressure. The vessels were provided with bottom pipes and valves for liquors and similar devices for gases.

The leached residue was washed in a filter press, which was ammonia-tight, and was there freed from ammonia; whether a similar result will be obtained with an ore containing a gangue only practical experience can tell, as the gangue is liable to hold back ammonia more tenaciously than a mixture of lead, lead oxide, and zinc oxide. A large Merrill press can be made ammonia-tight, as was the small Dehne press used at Lautenthal.

The precipitation of the copper was easily accomplished, only care had to be taken to relieve the pressure of the hydrogen without wasting ammonia. With an ore not carrying metallic zinc this feature probably need not be considered. The removal and washing of the precipitated copper is another important factor.

The distillation of the ammonia-zinc solution freed from copper was a brilliant success, the liquor showing only a trace of ammonia.

The calcination of the basic carbonate in a muffle furnace was accompanied by difficulties, as at a certain temperature the powdery charge begins to run as soon as it is touched with a rabble. The recovery of the carbon dioxide from the basic zinc carbonate was given up at the plant, and fresh gas prepared from burning coke.

The process outlined by Mr. Bretherton has possibilities, but many difficulties of a mechanical nature will have to be overcome before it can become a commercial success. However, the handling of large quantities of material with ammonia in closed vessels, *e. g.*, in the Solvay process for the manufacture of sodium carbonate, is not uncommon at present, so that there is a prospect in the end of a favorable result.

The Laws of Jointing.

BY BLAMEY STEVENS, TEMASCALTEPEC, MEXICO.

(Butte Meeting, August, 1913.)

THE following paper aims to make a full explanation of the phenomena of rock jointing.

It may be unnecessary to give any general description of what are termed *joints* in rocks, but Professor Geikie's clear and faithful generalizations are so illuminating that no excuse need be given for introducing the following and other quotations into this paper:

"Almost all rocks are traversed by vertical or highly inclined divisional planes termed *joints*. These have been regarded as due in some way to contraction during consolidation (fissures of retreat); and this is no doubt their origin in innumerable cases. But, on the other hand, their frequent regularity and persistence across materials of very varying texture suggest rather the effects of internal pressure and movement within the crust."[1]

General Explanation.—In this paper I show that the phenomenon of jointing may to a large extent be accounted for by the pressure of the water contained in the pores of the rock.

To understand this, each constituent grain of the porous material may be considered to be separately compressed by the water which submerges it. As all the grains are held together by cohesion, the whole mass is contracted by this pressure.

In an indefinitely extended mass, however, there must be a limit to the possible contraction without rupture, because the material is restrained from moving freely by other material around its boundaries. The mass of rock thus becomes broken up.

This phenomenon is known as "shrinkage" jointing. The term "shrinkage" is an apt one, if it be understood that the shrinkage of the rock is often brought about by change of stress.

Stresses.—The compressive component of water stress on the individual grains is evidently the same as that of the water with which it is in contact. This is so, both before and after the jointing is formed. Before the jointing is formed there is, however, a tensile stress in one or more directions which keeps the material from contracting.

At the moment of the formation of joints this stress is equal to the cohesive compressive stress. This is more commonly known to engineers as the tensile breaking stress.

[1] *Text Book of Geology*, 3d ed., p. 318 (1893).

Table I. shows this tensile breaking stress for several rocks and the corresponding water head which will produce it.

TABLE I.—*Tensile Strength of Rocks and Equivalent Depths.*[2]

Kind of Stone.	(1) Tensile Breaking Stress.	(2) Equivalent Water Head.	(3) Equivalent Rock In Water Head.	(4) Equivalent Rock Head.
	Pounds per Sq. In.	Feet.	Feet.	Feet.
Granites.				
Very hard, coarse grained St. Gotthard, obliqne to bed,	1,940	4,770	2,980	1,790
Very hard, coarse grained St. Gotthard, parallel to bed,	1,309	3,010	2,010	1,200
Very hard, striped grained St. Gotthard, perpendicular to bed,	2,773	6,380	4,250	2,550
Very hard, medium fine grained, from Cham,	1,991	4,580	3,060	1,830
Limestones.				
Muschelkalk, from Würzburg,	680	2,260	1,510	900
Best quality,	1,252	2,880	1,920	1,150
Poorest quality,	939	2,160	1,440	860
Dolomites.				
From the white Frankenjura, Poppenheim,	2,560	5,900	3,930	2,360

NOTE: Column 2 gives the minimum depth of formation of secondary vertical shrinkage joints.
Column 3 gives the minimum depth of formation of primary vertical shrinkage joints.

Causes of Water Pressure.—The most evident cause of water pressure existing in rocks is its natural head, as explained in a previous paper.[3] This exists in what is known as the subterranean sea. The tension-producing effect of the natural water head existing in rocks of the subterranean sea is, however, more or less counterbalanced by rock head stresses. The proof of this, which involves a little mathematics, is explained in the Appendix.

There are, however, other large bodies of interstitial water, called subterranean lakes, where the water pressure is greater than the natural head would be at that depth, and the pressure may occasionally be as great as that due to the whole weight of the overlying rock, or say 2.5 times what the water head would be for the same depth.

The assumption that bodies of water can exist under a greater pressure than that due to the natural head may not seem reasonable. We must, however, remember that gas, which is hundreds of times less viscous, is imprisoned by beds of shale. This fact goes to show that, in general, beds of shale are very much more impervious and

[2] Lanza, *Applied Mechanics*, 5th ed., p. 718 (1891 .
[3] The Laws of Igneous Emanation Pressure, *Trans.*, xliii., 167 (1912).

compact when buried deep in the earth than they are on the surface or when exposed by mining excavations.

The pressure may be brought about by the natural settling of stratified rocks which always occurs. The pore space is thus considerably reduced, and the water, imprisoned by the strata of impervious material, finally attains some pressure less than the weight of the overlying rocks.

By certain calculations given in the Appendix, and made on the assumption that the elastic stress ratios of glass apply to rock, the water pressures which will rupture the various rocks are found, and the corresponding minimum depths of formation of joints are given in the second and third columns of Table I.

In making these calculations it is presumed that the whole rock head is balanced by water pressure. This is not always the case, and perhaps seldom so, but the figures so found give the *minimum* depth of formation of shrinkage joints.

Column 3 is only applicable to primary vertical shrinkage joints; that is to say, to joints in the direction which is first ruptured.

A considerable increase of water pressure is necessary to cause jointing at right angles to this, as is shown in the Appendix and the second column of Table I.

Shear Joints.—After a certain depth is reached and the excess of water pressure released, by the formation of joints, etc., the simple vertical compression of the rock, partly buoyed up by the water, remains.

This pressure (which is approximately represented in water head by 1.5 H) becomes at a certain depth great enough to crush rocks with open shrinkage joints. The immediate cause of crushing fracture is, of course, shearing.

The corresponding depth of submersion in the subterranean sea required to produce this stress will be found in the third column of Table II. This table is for the same kind of rocks as Table I.

It is to be noted that the depth required for shear jointing is very much greater than for primary and secondary vertical joints. This explains their absence in the less altered sedimentary rocks.

The angle[4] between the shear and contraction jointing and the horizon is more or less the same as it would be with a fault. It is determined, however, to some extent, by the size of the block in which it occurs, its relation to other blocks, which have or have not been similarly fractured, and by other local factors.

[4] See Laws of Fissures, *Trans.*, xl., 475 (1909).

TABLE II.—*Compressive Strength of Rocks and Equivalent Depths.*

Kind of Stone.	(1) Crushing Strength.	(2) Equivalent Water Head.	(3) Equivalent Rock in Water Head.	(4) Equivalent Rock Head.
	Pounds per Sq. In.	Feet.	Feet.	Feet.
Granites.				
Very hard, coarse grained St. Gotthard, perpendicular to bed,	11,740	27,000	18,000	10,800
Very hard, coarse grained St. Gotthard, parallel to bed,	12,660	29,100	19,400	11,600
Very hard, striped grained St. Gotthard, perpendicular to bed,	15,650	36,000	24,000	14,400
Very hard, medium fine grained, from Cham,	22,190	51,000	34,000	20,400
Limestones.				
Muschelkalk, from Würzburg,	6,260	14,400	9,600	5,800
Best quality,	6,900 to 8,390	15,900 to 19,300	10,600 to 12,900	6,400 to 7,700
Poorest quality,	3,560	8,200	5,400	3,300
Dolomites.				
From the white Frankenjura, Poppenheim,	16,780 to 18,490	38,600 to 42,500	25,700 to 28,300	15,400 to 17,000
Sandstones.				
Very variable,	2,500 to 20,000	5,700 to 46,000	3,800 to 30,600	2,300 to 18,400

NOTE: Column 3 gives the minimum depth of formation of sheared jointing.

Joints in Stratified Rocks.—Geikie says of these:

"To the presence of joints some of the most familiar features of rock-scenery are due. Joints vary in the angles at which they cut the planes of bedding, in the sharpness of their definition, in the regularity of their perpendicular and horizontal course, in their lateral persistence, in number, and in the directions of their intersection. As a rule, they are most sharply defined in proportion to the fineness of grain of the rock. In limestones and close-grained shales, for example, they often occur so clean-cut as to be invisible until revealed by fracture or by the slow disintegrating effects of the weather. The rock splits up along these concealed lines of division, whether the agent of demolition be the hammer or frost. In coarse-textured rocks, on the other hand, joints are more apt to show themselves as irregular sinuous rents.

"As a rule, they run perpendicular, or approximately so, to the planes of bedding, and descend vertically at not very unequal distances, so that the portions of rock between them, when seen in profile, appear marked off into so many wall-like masses. But this symmetry often gives place to a more or less tortuous course with lateral joints in various random directions, more especially where the different strata vary considerably in lithological characters. A single joint may be traced for many yards, sometimes, it is said, for many miles, more particularly when the rock is fine-grained, as in limestone. But where the texture is coarse and unequal, the joints, though abundant, run into each other in such a way that no one in particular can be identified for more than a limited distance. The number of joints in a mass of stratified rock varies within wide limits. Among strata which have undergone little disturbance the joints may be separated from each other by intervals of several yards. But in other cases where terrestrial movement has been con-

siderable, the rocks are so jointed as to have acquired therefrom a fissile character that has nearly or wholly obliterated their tendency to split along the lines of bedding.

"An important feature in the joints of stratified rocks is the direction in which they intersect each other. In general they have two dominant trends, one coincident, on the whole, with the direction in which the strata are inclined from the horizon, and the other running transversely at a righ angle or nearly so. The former set is known as *dip-joints*, because they run with the *dip* or inclination of the rocks; the latter is termed *strike-joints*, inasmuch as they conform to the *strike* or general outcrop. It is owing to the existence of this double series of joints that ordinary quarrying operations can be carried on. Large quadrangular blocks can be wedged off, which would be shattered if exposed to the risks of blasting."

* * * * * * * * * *

"Ordinary household [soft] coal presents a remarkably well-developed system of joints. A block of such coal may be observed to be traversed by fine laminæ, the surfaces of many of which are soft and soil the fingers. These are the planes of stratification. Perpendicular to them run divisional planes, which cut each other at right angles or nearly so, and thus divide the mineral into cubical fragments. One of these sets of joints makes clean sharply defined surfaces, and is known as the *face, slyne, cleat,* or *bord;* the other has rougher, less regular surfaces, and is known as the *end.* The face remains persistent over wide areas; it serves to define the direction of the roadways in coal-mines, which must run with it."[5]

Without having to repeat the reasoning given in former papers,[6] it will be clear that one set of parallel, vertical joints is first formed in a direction perpendicular to the least rock stress, then when the water pressure increases enough the other set is formed at right angles to the first set.

The "face" joints in coal, described by Geikie, evidently belong to the first-formed set and the "end" joints to the second set. The "strike" joints and "dip" joints have a relation to the directions of least rock pressure, for it is to be noted that a sedimentary rock is usually made to dip by gentle folding; and folding is caused by a compressive rock stress.

In many folded rocks, the main, or "master," joints are observed to be perpendicular to the axes of the folds.

Where there has been no particular direction of least stress it is evident that "more or less tortuous courses with lateral joints in various random directions" may be formed.

When the water pressure exceeds that due to the overlying rock, it is also evident that horizontal or inclined shrinkage joints may be found.

Rents.—The formation of joints in rock under pressure no doubt often leads to the draining out of part of the water which has been contained in the rock.

This water makes its exit from the rock mass under the pressure

[5] *Text Book of Geology*, 3d ed., p. 524 (1893).

[6] *Ibid.*, and The Laws of Intrusion, *Trans.*, xli., 650 (1910).

DH + C, which is the same as for emanation and intrusive magma in equipressure rocks.[7]

Water at this excessive pressure is therefore capable of breaking a passage for itself in the rocks above it and of acting in other ways like emanations.

Shearing Joints.—When a brittle rock alternates with a plastic one (as an alternation of sandstone and shale) the brittle one is often fractured, while the plastic one escapes. The master joints, transversing the brittle rock, rapidly become obliterated after they reach its margin.

A critical examination of the shale often shows that its plastic properties are due to the ease with which it shears.

Joints in Massive Rocks.—Geikie says of these:

"While in stratified rocks the divisional planes consist of lines of bedding and of joint, cutting each other usually at a high, if not a right, angle, in massive (igneous) rocks they include joints only; and as these do not, as a rule, present the same parallelism as lines of bedding, unstratified rocks, even though as full of joints, have not the regularity of arrangement of stratified formations. Some massive rocks indeed may have one system of divisional planes so largely developed as to acquire a bedded or fissile character. This structure, characteristically shown by phonolites, may also be detected among ancient porphyries. Most massive rocks are traversed by two intersecting sets of chief or 'master' joints, whereby the rock is divided into long quadrangular, rhomboidal, or even polygonal columns. A third set may usually be noticed cutting across the columns and articulating them into segments, though generally less continuous and dominant than the others. When these last-named cross-joints are absent or feebly developed, columns many feet in length can be quarried out entire. Such monoliths have been from early times employed in the construction of obelisks and pillars.

"In large masses of granite, an outward inclination of the natural divisional planes of the rock may sometimes be observed, as if the granite were really a rudely bedded mass, having a dip towards and under the strata which rest upon its flanks. It is not a foliated arrangement of the constituent minerals analogous to the foliation of gneiss, for it can be traced in perfectly amorphous and thoroughly crystalline granite, but is undoubtedly a form of jointing by reason of which the rock weathers into large blocks piled one upon another like a kind of rude cyclopean masonry. In the quarrying of granite, the workmen recognise that the rock splits into blocks much more easily in one direction, though externally there is no trace of any structure which could give rise to this tendency."[8]

Igneous rocks, especially the more basic ones, contract on crystallization. In dikes and lava flows the consolidation proceeds from the sides towards the center and a prismatic structure is so formed perpendicular to the walls.

The more massive igneous rocks are usually cooled at great depths, and consolidation takes place much more slowly. Under these conditions the solidifying rock adjusts itself to the contraction taking place, and the jointing from this cause is often either absent, irregular, or not at all prominently displayed.

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[7] Laws of Igneous Emanation Pressure, *Trans.*, xliii., 167 (1912).

[8] *Text Book of Geology*, 3d ed., p. 527 (1893).

Joints in Schistose Rocks.—Geikie says of these:

"The schists likewise possess their joints, which approximate in character to those among the massive igneous rocks, but they are on the whole less distinct and continuous, while their effect in dividing the rock into oblong masses is considerably modified by the transverse lines of foliation. These lines play somewhat the same part as those of stratification among the stratified rocks, though with less definiteness and precision. The jointing of the more massive foliated rocks, such as the coarser varieties of gneiss, approaches most closely to that of granite; in the finely fissile schists, on the other hand, it is rather linked with that of sedimentary formations. Upon these differences much of the characteristic variety of outline presented by cliffs and crests of foliated rocks depends."[9]

Generally, it might be said that metamorphic rocks have been buried at greater depths than the less altered sedimentaries. They should, therefore, be more jointed.

But there are several modifying factors. Having been once well jointed and perhaps faulted, some rocks can no longer retain any considerable excess of water pressure. It is rare to find any but fresh water in highly metamorphic rocks, while sedimentaries usually contain salt water in depth.

The most pronounced joints are usually those which are vertical and perpendicular to the foliation.[10] These are the same as the primary joints of the sedimentary rocks.

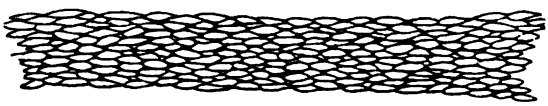

FIG. 1.—CLEAVAGE STRUCTURE OF SLATE.

Schistose and slaty rocks necessarily result from great horizontal pressure and plastic yielding. The individual grains of the material are thereby flattened as in Fig. 1. It is evident that a large portion of the boundaries of these grains are parallel to one directional plane. This is called the plane of cleavage or of fissility and is naturally a direction of small cohesion. Along it the material will easily split. Perpendicular to it the rock has considerable strength, the flattened grains being bonded together in such a manner as to offer much area of frictional resistance to one another. When any attempt is made to separate them by a pull in this perpendicular direction, a great area of fracture is presented. When there is no imposed stress perpendicular to the planes of foliation, rocks break very irregularly across the grain. The clean breaks of much of the jointing of these rocks tells us that there must have been considerable stress perpendicular to the planes of foliation.

[9] *Text Book of Geology*, 3d ed., p. 531 (1893).

[10] The arkose and slate series of Latouche Island, Alaska, is a good example which happens to have come to my notice.

Many rocks other than slate show a slaty cleavage which may easily be mistaken for jointing. Its general strike, however, is at right angles to the great horizontal pressure to which the rocks of the region have been subjected. There is often good evidence as to this. The plane of cleavage is not, properly speaking, necessarily a plane of jointing, but jointing planes are formed at various angles to it.

Irregular Jointing.—Many metamorphic rocks show much irregular jointing. This may be explained by variation in structure. An important cause is the occurrence of internal (uneven) strains which have their effect after the great earth pressures are released. Such joints are usually curved or radial.

Geikie says of these:

"Among mountain-valleys, in railway-tunnels through hilly regions, or elsewhere among rocks subjected to much lateral pressure, or where owing to the removal of material by running water, and the consequent formation of cavities, subsidence is in progress, sounds as of explosions are occasionally heard. In many instances, these noises are the result of relief from great lateral compression, the rocks having for ages been in a state of strain, from which as denudation advances, or as artificial excavations are made, they are relieved. This relief takes place, not always uniformly, but sometimes cumulatively by successive shocks or snaps. Mr. W. H. Niles of Boston has described a number of interesting cases where the effects of such expansion could be seen in quarries; large blocks of rock being rent and crushed into fragments and smaller pieces being even discharged with explosion into the air. More recently Mr. A. Strahan has called attention to the occurrence of slickensided surfaces in the lead-mines of Derbyshire which on being struck or even scratched with a miner's pick break off with explosive violence, and he suggests that the spars and ores along those surfaces are in 'a state of molecular strain resembling that of the Rupert's Drop or of toughened glass, and that this condition of strain is the result of the earth movements which produced the slickensides.'

"If such is the state of strain in which some rocks exist even at the surface or at no great distance beneath it, we can realize that at great depths, where escape from strain is for long periods impossible, and the compression of the masses must be enormous, any sudden relief from this strain may well give rise to an earthquake shock. A continued condition of strain must also influence the solvent power of water permeating the rocks." [11]

Following the reasoning used for fissures [12] and intrusions,[12] the straightness or crookedness of a joint is no doubt largely affected by the value of stress components, other than that to which it is approximately perpendicular.

An irregular rock structure may also be the cause of irregular jointing. Such rocks as tuffs, conglomerates, etc., may be very variously jointed. Thin beds are usually less evenly jointed than thick ones.

It is seldom, however, that there is not one obvious plane towards which many of the principal joints approximate. Sometimes there may be a linear direction which is common to planes which are otherwise apparently irregularly placed. Such, for example, as the vertical, or the perpendicular to the walls of a dike or vein.

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[11] *Text Book of Geology*, 3d ed., p. 311 (1893).

[12] Ibid.

Size and Number of Joints.—The distance between joints depends on the amount of strain which the surrounding rocks will allow of. A thick bed of rock will have a much larger spacing of the joints than occurs in a thin bed. Thus thick beds of limestone often have joints very widely spaced, while thin beds of the same material have joints very close together.

Effect of Metasomatism.—Sometimes water and solutes contained in it are absorbed by minerals, with consequent swelling, Serpentinization is an example of such a case. The effect is just the opposite to that of jointing: viz., to make a compact joint-free rock.

The effect of mineral alteration is, however, often a shrinkage of volume. Thus the alteration of limestone to dolomite. The effect in these cases would always be to form joints, were it not that the highly charged waters deposit material in the cracks and so re-weld them.

Prismatic Jointing.—Geikie says of this:

"The most characteristic structure, however, among volcanic rocks is the prismatic, or, as it is incorrectly termed, 'basaltic.' Where this arrangement occurs, as it so commonly does in basalt, the mass is divided into tolerably regular pentagonal, hexagonal, or irregularly polygonal prisms or columns, set close together at a right angle to the main cooling surfaces. These prisms vary from 1 inch or even less to 18 or more inches in diameter, and range up to 100 or even 150 feet in height. Many excellent and well-known examples of columnar structure are exhibited on the coast-cliffs of the Tertiary volcanic region of Antrim and the west coast of Scotland, as in the Giant's Causeway and Fingal's Cave. In many cases no sharp line can be drawn between a columnar basalt and the beds above and below, which show no similar structure, but into which the prismatic mass seems to pass." [13]

Hexagonal jointing is formed by the operation of laws of an entirely different kind from those determining other kinds of joints. I have noticed that, in drying, silt starts with the opening of almost innumerable extremely small cracks, as shown in Fig. 2. These apparently form in haphazard directions, but each one is very straight, is widest in the middle and closes gradually towards the ends. It is in fact impossible to say exactly where a cracklet begins or ends, but this cannot, of course, be shown in an illustration.

As the surface of the mud continues to dry these cracklets extend and broaden themselves until one runs into another as in Fig. 3. The point of junction then becomes considerably enlarged and the three directions of radiation are extended rapidly. But other cracklets for a considerable radius around this center cease to develop.

The radial arms do not now follow straight courses, but as they cut other cracklets, tend to take their course, as shown in Fig. 4.

It is to be noticed, however, that the arms tend to take mean courses in such a way that angles of 120° are extended between them.

[13] *Text Book of Geology*, 3d ed., p. 529 (1893).

In the meantime other similar centers of fracture have been forming in just the same way, and these are all more or less the same distance apart.

Finally, the arms of each of these centers intersect with those of an adjoining center and a very rough irregular hexagonal pattern of the now much-enlarged cracks is formed.

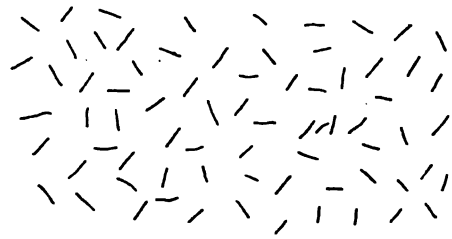

FIG. 2.—FIRST STAGE IN CRACKING OF MUD.

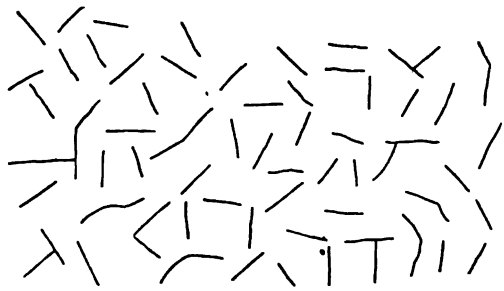

FIG. 3.—SECOND STAGE IN CRACKING OF MUD.

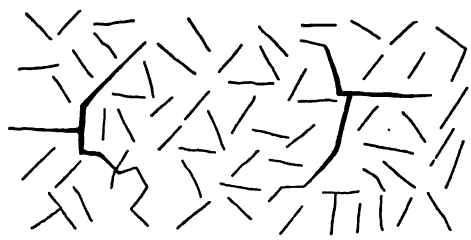

FIG. 4.—THIRD STAGE IN CRACKING OF MUD.

It was evident from the silt cliffs[14] of the vicinity that these cracks become more straight and the polygons of more even size and perfection as the silt dries down. The jointing so formed is perfectly vertical, and consequently the faces stand out like masonry towers for 50 ft. or more in height, though the material can be cut with the finger nails, or crushed into powder in the hand. Crystals of a dry salt were found in the interstices, indicating clearly that the mass

[14] In vicinity of Okanagon lake, B. C.

must have dried by evaporation, and leaving no doubt as to the cause of jointing.

The jointing of cooling lavas may, no doubt, be explained in the same way, although we cannot witness it so clearly. It is not necessary to consider any center of cooling, as is usually done. General centers of fracture are formed automatically by the evolution of cracklets. The larger of these survive and develop because they relieve the stress to such an extent as to prevent the further development of the smaller cracklets.

Spherical Jointing.—Geikie says of this:

"In many prismatic massive rocks (basalt, diorite, &c.), segments of the prisms weather into spheroids, in which successive weathered rings forms crusts like the concentric coats of an onion." [15]

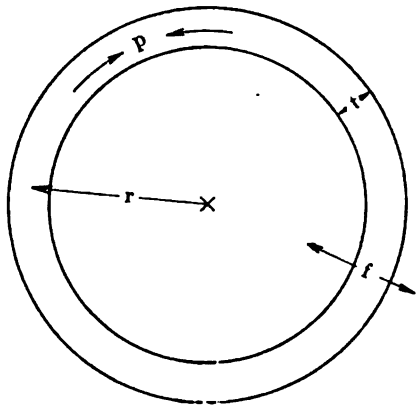

FIG. 5.—FORMATION OF SPHERICAL JOINTS.

Spherical jointing usually forms near the surface. The ground has previously been cut up into blocks by other forms of jointing. By the agency of the atmosphere the rock becomes altered on each side of the joints and in doing so tends to expand. The corners are thus chipped off so as to form new joints bordering a roughly spherical mass of rock. This in turn is altered at and near its surface, which expands and causes a shell compression p in the surface of the sphere. Radial tensions f are thus developed.

The ratio of this radial tension to the shell compression is evidently $\frac{f}{p} = \frac{2t}{r}$, where t is the thickness of the shell and r the radius of the sphere. (See Fig. 5.)

As alteration proceeds t increases until f becomes equal to the tensile breaking stress. The shell then becomes separated by a spherical

[15] *Text Book of Geology*, 3d ed., p. 348 (1893).

joint from the smaller remaining sphere inside. This process proceeds until alteration is complete.

Some Misconceptions.—An experiment of Daubrée in the formation of cracks, by the twisting of a glass rod, is often used to illustrate torsion jointing. As a matter of fact, the cracks so formed are contraction joints, being formed at right angles to the direction of direct tension. The lines of starting of the tension cracks on the two sides of the plate or rod are necessarily at right angles to one another.

It is to be noted that shear jointing proper is not formed in the planes of greatest tangential stress, but in the planes of greatest ratio of tangential to direct stress.

The assumption of the former hypothesis[16] leads to the conclusion that shear joints should be at right angles to one another, which is not usually the case. The ordinary shrinkage joints may make right angles with one another, but there is no reasonable way of considering these as shear joints.

Needed Experimental Work.—Probably the most necessary experimental work connected with the Laws of Jointing is the determination of the coefficients of elasticity of rocks.

To do this it would only be necessary to take long drill cores of the various rocks and note the deflections when they are used as beams and when they are put under a torque. All the elastic coefficients and stress ratios could be calculated from these easily obtained observations.

Appendix.

General Elastic Properties of Rocks.—The elastic properties of rocks appear to have been little studied. In engineering books only special phases are treated of, and these are not all of those required in geological problems.

The general elastic properties of any homogeneous isotropic substance are determined by two constants. These are most scientifically taken as the coefficient of volumetric elasticity, κ, and the coefficient of rigidity, μ. The values of κ and μ have been determined for very few substances. Of these, glass is the only one which we may consider as approaching the average rock. The elastic values for glass are:[17]

$$\mu = 2{,}900{,}000 \text{ lb. per square inch.}$$
$$\kappa = 5{,}800{,}000 \text{ lb. per square inch.}$$

If a fluid pressure f is applied to a unit cube, Fig. 6, it will be diminished in bulk by $\frac{f}{\kappa}$.

[16] G. F. Becker, *Bulletin of the Geological Society of America*, vol. iv., p. 48 (1892). Also, Torsional Theory of Joints, *Trans.*, xxiv., 130 (1894).

[17] *Encyclopedia Americana*, article Elasticity.

The corresponding diminution of each unit dimension of the cube is $\frac{f}{3\kappa}$.

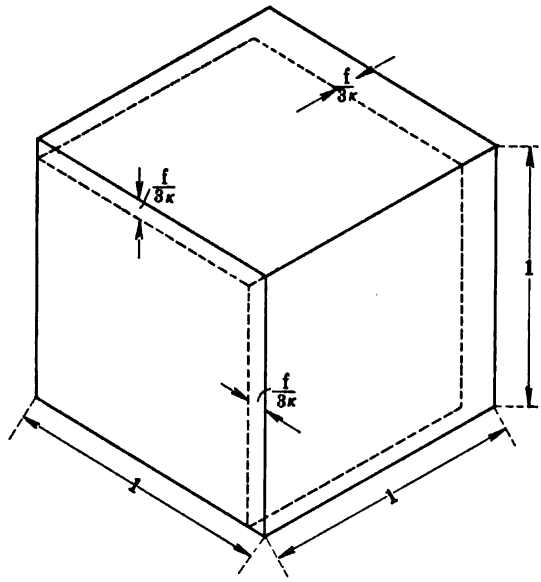

FIG. 6.—STRAIN PRODUCED BY FLUID COMPRESSIVE STRESS.

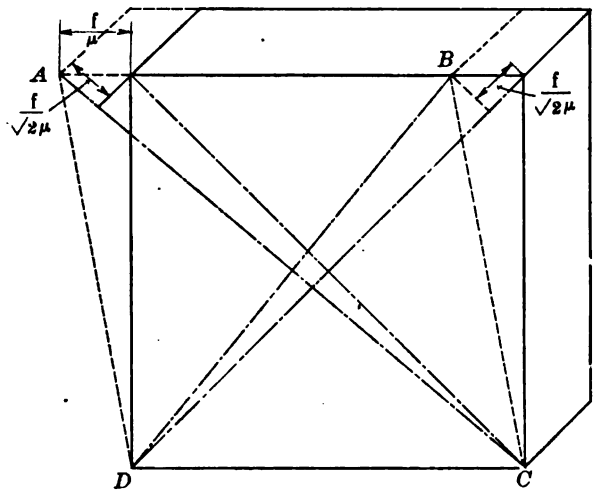

FIG. 7.—STRAIN PRODUCED BY TORSIONAL STRESS.

The value of 3κ, the linear component of volumetric elasticity, is, for glass, 17,400,000 lb. per square inch.

If a shear is applied to a unit cube, Fig. 7, it will be distorted as shown, by $\frac{f}{\mu}$. This is equivalent to compressing it along the diago-

nal BD by an amount $\frac{f}{\sqrt{2\mu}}$ and expanding it by an equal amount along the diagonal AC. The proportional compression and expansion along the direction of these diagonals are respectively obtained by dividing the deflection by the length of the diagonals. As these lengths are approximately $\sqrt{2}$ in each case, the proportional compressions and expansions are $\frac{f}{\sqrt{2\mu}} \times \frac{1}{\sqrt{2}} = \frac{f}{2\mu}$.

From an inspection of the stress relations in Fig. 8 it will also be seen that a shearing stress f on the unit diagonal is equivalent to two compressive forces $\frac{f}{\sqrt{2}}$ and two expansive forces $\frac{f}{\sqrt{2}}$ on the sides of

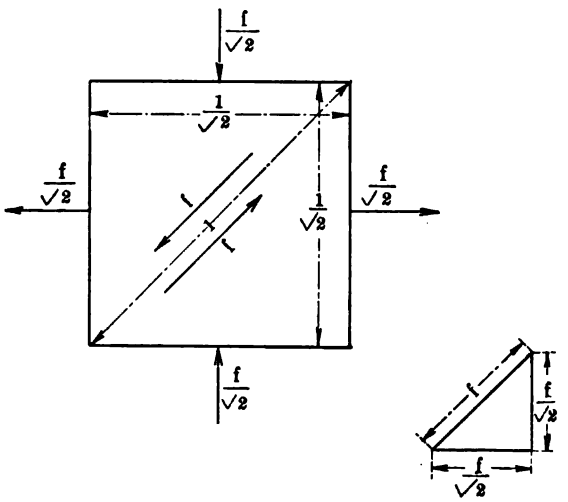

FIG. 8.—RELATION OF TORSIONAL TO DIRECT STRESSES.

the cube. As the sides of the cube on which these forces act is $\frac{1}{\sqrt{2}}$ the corresponding compressive and expanding stresses are f in each case.

The value of 2μ, the linear component of rigidity, is, for glass, 5,800,000 lb. per square inch.

Stress in One Direction Only.—The "modulus of elasticity," used in engineering problems, is a combination of κ and μ. In finding it a direct stress is applied in one direction only.

In the diagram Fig. 9 presume a fluid compressive force f is first applied equally in all directions on the unit cube shown. The dimi-

nution of each single dimension is $\frac{f}{3\kappa}$. Now apply shears such that there are no resultant forces on the cube in the y and z directions. This will be equivalent to expanding the cube by $\frac{f}{2\mu}$ in each of these directions and compressing it in the x direction by equal amounts. The stress in the x direction is then 3f and the strain $\frac{f}{3\kappa} + \frac{f}{\mu}$. The ratio of these is the "modulus of elasticity,"

$$E = \frac{3f}{\frac{f}{3\kappa} + \frac{f}{\mu}} = \frac{9\kappa\mu}{3\kappa + \mu} \quad . \quad . \quad . \quad (1)$$

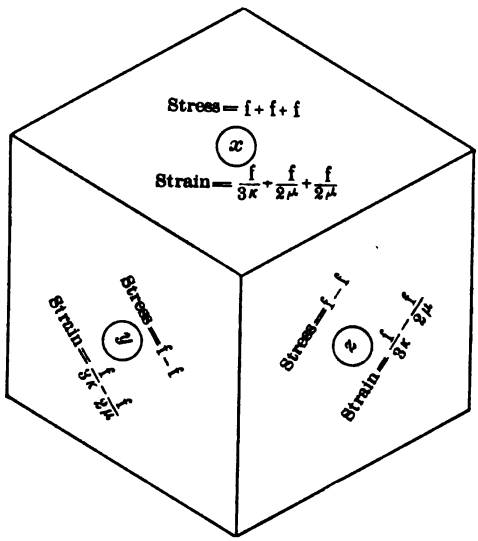

FIG. 9.—STRAINS PRODUCED BY A DIRECT STRESS.

Using the coefficients of μ and κ, given for glass, the value of E is, approximately, E = 7,450,000.

The strain in each direction at right angles to the direction of stress is $\frac{f}{3\kappa} - \frac{f}{2\mu}$. By dividing the x stress, 3f, by this strain we get the coefficient

$$B = \frac{-3f}{\frac{f}{2\mu} - \frac{f}{3\kappa}} = \frac{-18\kappa\mu}{3\kappa - 2\mu}, \quad . \quad . \quad . \quad (2)$$

For glass, therefore, $B = -26{,}100{,}000$. The ratio $\frac{E}{-B}$ is called Poisson's ratio and is given by

$$\frac{E}{-B} = \frac{3\kappa - 2\mu}{6\kappa + 2\mu}, \quad . \quad . \quad . \quad . \quad (3)$$

For glass, therefore, $\frac{E}{-B} = 0.286$.

Strain in One Direction Only.—Another case is when there is only a single strain, say that in the direction x, Fig. 10. Presume again that a compressive stress f is applied and creates a strain $\frac{f}{3\kappa}$ on each face of the unit cube. This strain can be counteracted by one component of a shearing strain of the same

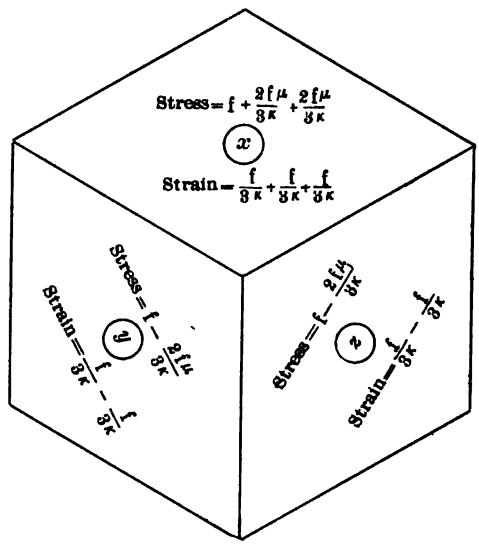

FIG. 10.—STRESSES PRODUCED BY A DIRECT STRAIN.

amount and opposite sign. The shearing stress required for this is obtained by multiplying the strain $\frac{f}{3\kappa}$ by 2μ: viz., $\frac{2f\mu}{3\kappa}$. Reasoning as before, the coefficient of elasticity sought is therefore

$$A = \frac{f + \frac{4f\mu}{3\kappa}}{\frac{f}{3\kappa} + \frac{2f}{3\kappa}} = \kappa + \frac{4}{3}\mu, \quad . \quad . \quad . \quad (4)$$

For glass, therefore, $A = 9{,}700{,}000$.

The stress at right angles to the direction of strain is $f - \frac{2f\mu}{3\kappa}$. The ratio of this stress to the stress in the direction of the strain is therefore:

$$i = \frac{1 - \frac{2\mu}{3\kappa}}{1 + \frac{4\mu}{3\kappa}} = \frac{3\kappa - 2\mu}{3\kappa + 4\mu}, \quad . \quad . \quad . \quad (5)$$

For glass, therefore, $i = 0.4$.

No Stress in One Direction and No Strain in Another Direction.—Another case of geological strain is that of an applied stress in one direction, no strain in a second direction and no increment of force in the third direction.

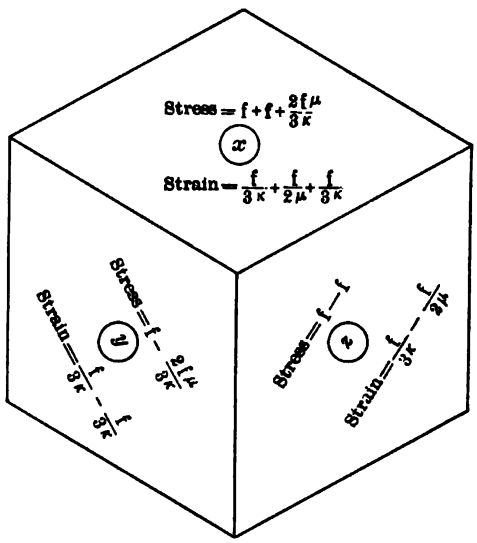

FIG. 11.—A COMMON GEOLOGICAL STRAIN SYSTEM.

Fig. 11 shows how such a system may be made up of a unit bulk strain, a shear from y to x and another from z to x. The resultant coefficient in the direction x will be

$$G = \frac{2 + \frac{2\mu}{3\kappa}}{\frac{2}{3\kappa} + \frac{1}{2\mu}}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

For glass, therefore, $G = 8{,}100{,}000$.

The ratio of collateral to lateral stress is

$$j = \frac{3\kappa - 2\mu}{6\kappa + 2\mu}, \qquad (7)$$

This is the same in value as Poisson's ratio $\frac{E}{-B}$ (equation 3).

For glass, therefore, $j = 0.286$.

Stresses Producing Primary Vertical Shrinkage Joints.—The effect of a vertical rock stress may now be calculated. If H be the depth, the vertical rock pressure in water head will be, approximately, $H \times 2.5$. If this head produces no horizontal strains, the horizontal stresses necessary to balance it will be $i \times H \times 2.5$ ($= H$ for glass). So that this horizontal stress would be balanced by the water pressure, and no tension produced. This result may be somewhat modified if the elastic ratios of rocks differ from those of glass. It is also to be noted that the water pressure acts on the solid rock material, while the rock pressure acts on the porous collection of grains, whose elastic coefficient must be somewhat less.

The extreme result of water pressure is attained when the water pressure becomes equal to the whole rock pressure. It cannot very well become greater than this, as horizontal joints would be formed along those planes where there is little cohesion and these would open indefinitely.

When the pressure is equal to the rock head, the vertical stress on the rock is $H \times 2.5$ and the resultant horizontal compressive stress is $i \times H \times 2.5 = H$ (for glass).

The tension stress produced horizontally by the water pressure is $H \times 2.5$, so that the net horizontal tension of the rock before rupture is the balance of 1.5 H (for glass.)

When the water pressure is equal to the rock head this pressure tends to contract the rock and the depth producing jointing under these conditions may be directly read from the third column of Table I. As the fluid pressure is no doubt usually less than that due to the whole weight of rock, the column must simply be taken as showing the least possible depth of joint formation in stratified rocks.

Stresses Producing Secondary Vertical Shrinkage Joints.—The first set of joints formed may be presumed to be perpendicular to the y axis. (See Fig. 12.) The stress in each of the two directions x and y, reckoned as water head, is 2.5 H. Each of these creates a stress along z of $2.5\ H\ j = 0.715$ H (for glass). The resultant compressive stress along z due to these two is therefore 1.43 H (for glass). The tensile stress produced along z by the water pressure is 2.5 H, so

that the net horizontal tension along z is $(2.5 - 1.43)\ H = H$ (approx. for glass).

The minimum depth necessary to produce jointing under these conditions may be directly read from the second column of Table I.

The imperfect or partly formed nature of many secondary jointing systems is thus explained by the fact that considerably more depth is required for their development than for the primary joints.

Effect of Earthquakes.—Prof. W. O. Crosby has speculated on the influence of earthquakes in producing joints.[18]

Earthquakes of the largest known violence have within the solid ground an amplitude of vibration (a) of about 2 in. and a complete

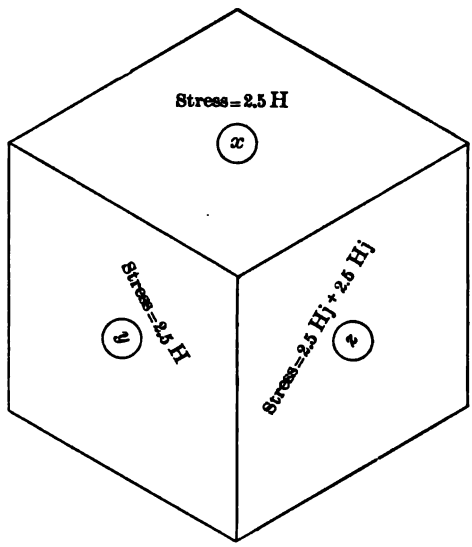

FIG. 12.—STRESSES PRODUCING SECONDARY JOINTS (MAXIMUM).

period (t) of 0.5 sec. The velocity (v) is about 4,000 ft. per second. The following formula gives the resultant stress: $h = \frac{2\pi av}{gt}$, where h is the maximum pressure produced in rock head. Space is not taken here to show how this formula is obtained because it is easily derived from those given in books on earthquakes.

The greatest difference of stress from the normal, produced by an earthquake, is thus seen to be equivalent to 261 ft. of rock head. This is a small amount relatively to the joint-forming stresses and therefore earthquakes cannot be considered as a factor in jointing.

[18] *Proceedings of the Boston Society of Natural History*, vol. xxii., p. 72 (1882–83).

Discussion.

F. L. Grammer, Leesburg, Va. (communication to the Secretary*):—Mr. Stevens refers to an explanation of parallel jointing by W. O. Crosby, which I noted when reading one of his excellent text books.

It seemed to me earthquakes were not a satisfactory explanation, and I looked up several authorities; but I never realized how small the usual amplitudes were until Mr. Stevens presented his paper.

Several letters to the U. S. Geodetic Survey showed me there were very few records of tidal movements of earth crust in the United States. Altitude and latitude would be influencing factors; but this diurnal movement must exceed earthquake amplitudes, besides being more nearly universal. My inquiries brought the answer that the major directions of jointing had not been extensively tabulated.

If major directions of jointing were recorded—with notes as to angle with mountain chain, or North Star, and distance between joints, with nature of rock and maximum and minimum tidal upheaval—some interesting relations might be made manifest.

Perhaps European tidal movements of solid crust have been more closely studied and their influence on joint direction and frequency in different classes of rocks commented on.

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* Received Aug. 8, 1913.

Valuation of Coal Land.

BY H. M. CHANCE, PHILADELPHIA, PA.

(Butte Meeting, August, 1913.)

ADEQUATE treatment of the difficulties surrounding the valuation of mineral lands requires that agreement be first reached defining value as understood for the purpose of appraisal. To define value as the price of a product is not sufficient, and does not assist us in using the factors that cause variations in values and in relative values. The real value of any product depends primarily upon its relation to the necessities, comforts and pleasures of mankind. As the necessities, comforts and pleasures of the civilized races differ from those of more primitive man, the value of products varies with the degree of civilization and the environment of the consumer. Coal has no value to the savages of warm climates, and arrow-head flint none to the residents of New York. It has long been recognized that what has been termed "intrinsic value" must be limited to materials that are essential to existence—air, water, food.

But "intrinsic value" cannot be measured in monetary terms. The intrinsic value of a bushel of wheat is due to the fact that it will support the life of an individual for a certain definite time, but this fact does not enable us to measure or to express that value in monetary terms. It has often been proposed to adopt as a measure of the value of any product the quantity of labor necessary to produce it, but this method is of limited application, and will not apply to what may be termed "intrinsic values."

For many reasons it might be well, while assenting to the fact that any material may be intrinsically valuable, to abandon the use of the term "intrinsic value." One of these reasons is the tendency to confuse the idea of intrinsic value with a value that is conferred by economic conditions, that is purely artificial and that may be permanent or evanescent. Intrinsic value must from its nature be permanent and unchangeable.

Possibly for present purposes the value of any material may be considered as having a dual origin: First, that due to economic

conditions, whereby, demand having made the material desirable, it comes to have value wherever found; and second, that due to the labor expended in producing the material. The term labor is here used in the broadest sense to include labor of all kinds engaged in production (capital being regarded as accumulated reserve labor), in preparation for use, and in transportation to the consumer.

The first subdivision of value defined above is that with which we have to deal and concerns value conferred upon a material by the relation of supply to demand. If the supply be abundant or the demand be small, the material will have no definable value. Thus deposits of limestone in countries where limestone is the country rock have no definable value.

In attempting to determine the value of coal or mineral lands, it therefore seems desirable to dismiss the idea of inherent or intrinsic value; and to confine consideration solely to those economic conditions that justify valuation expressed in terms of a monetary unit.

As these values are due to economic conditions which are not permanent, but are subject to fluctuation and change, the values will likewise rise or fall in correspondence with such changes, and we can thus never hope or expect to do more than reach a result which for a time will fairly represent existing value.

Under the conditions of modern civilization coal is necessary to the continuance of human life. It has thus become valuable. To the savage it has no value. As it becomes more necessary its value increases. It is not a substance inherently valuable as necessary to the continued existence of the race. Food has such value, clothing has such value, and outside tropical and semi-tropical countries fuel of some sort has such value. The savage never used coal and civilized races have developed and maintained highly organized civilizations in countries where it was practically unknown, so that its value must be predicated entirely upon those requirements of modern civilization which have made it a necessity.

We are not able to determine the intrinsic value of coal. The value of products which are not necessities, and which can be produced in unlimited quantities, is fixed by the cost of production, but this law does not hold good for products which are necessary to the continued existence of civilized life, and since coal has become a necessity, and as it is a substance of which the readily accessible supply is not unlimited, its value increases as the demand for it grows.

Hence we find throughout the world that the value of coal land is constantly increasing, and the rate of increase is more rapid than the

increase in the value of farm land, or land containing limestone, building stone, or other similar useful materials.

This rapid increase in value, often marked by great relative disparities in current values in different localities, has stimulated study of the subject with a view to the development of systematic methods for determining value. The subject has attracted the attention not only of coal operators, but of investors, financiers, and political economists, and has become important to those charged with the raising of revenues for defraying the expenses of government, for national, state, county, and township requirements.

It has also become a matter of great, if not of vital, importance to the nation, as affecting the sale, or other disposal, of government lands containing coal.

Methods used for determining the price at which it is deemed expedient to sell coal lands owned by the United States may not be applicable to appraisals of value upon which the financing of coal operations is to be based, and methods suitable for either of these purposes may not properly be applicable to valuations made for the levying of taxes.

The value of coal may or may not be a factor in fixing the price at which coal lands, or coal-mining rights, are offered for sale by the United States. In the past it was deemed desirable te encourage the opening of coal mines by offering the lands (subject to certain conditions easy of fulfillment by an intending purchaser) at the nominal prices of $10 and $20 per acre, the higher price being placed upon such lands as were within 20 miles of an existing railroad, and further, to secure such development as well as to secure the construction of railroads through the public domain, large grants of coal lands were given as a bonus for the construction of such roads. In certain localities it may still be good policy for the government to encourage or stimulate the development and working of coal lands by offering them at relatively low prices to those who will develop and work them, but with few exceptions this policy is no longer necessary to insure the opening of mines.

It will perhaps be conceded without argument that the price paid by the operator for coal land or for coal-mining rights must be repaid to him through the sale of coal. The coal operator must fix a selling price for his product sufficient to include the cost of mining, the price paid for the coal land, and interest on the amount invested in equipment and working capital, for if his product be sold for less than such price, the operation will be unprofitable and will soon be abandoned. As it is thus clear that the consumer must repay to the

coal operator the price paid for the land, the sale of coal land at "coal land prices" by the government becomes in fact merely a method of raising money by taxing the consumer. The wisdom of this method of raising revenue is open to argument.

For some years we have heard the advocates of conservation exploiting their pet hobby. One of the principal, and perhaps one of the most popular, slogans of these enthusiasts has been the conservation of coal. As commonly understood this means the retention by the government of most of its coal lands, presumably for the benefit of future generations.

If we have good reason to fear that the coal supply will be insufficient for those who will follow us, it is indeed a beautiful sentiment that requires us to preserve it for posterity, even if such preservation requires self-denial and imposes some hardships upon the present generation. It is, however, not clear that the present generation is willing to impose upon itself such economies or self-denial as will bring about this result. If such a policy be actually adopted and carried out, the preservation of the supply would follow whether the lands containing coal were owned by the government or by individuals or corporations, for whatever fuel the people require must be mined and delivered to them, and this is quite independent of any question of ownership.

True conservation points rather to the efficient and economic use of our fuels (rather than to parsimony in their distribution), to better mining methods and to improvements in power-generating machinery, to the end that more of the coal be recovered in mining and that more of the heat units generated by combustion be transformed into energy and utilized in doing useful work.

The established policy of the government is, however, now well defined as a plan to utilize its coal lands by selling them at as high a price as the coal operator can be induced to pay. As already pointed out, the effect of this policy is to increase the cost of coal to the consumer, and this in turn tends to promote that form of conservation (which many are now preaching) which curtails the consumption by decreasing the ability of the consumer to purchase as much as he would like to use. Those who advocate this policy justify it by the argument that it is the only way in which the government can secure for itself the unearned increment in value due to the steadily rising prices of coal land. This may readily be granted, but it must also be admitted that in adopting this policy the government is itself doing exactly what conservationists condemn and object to when practiced by the individual owner of coal land. In both cases the profit repre-

senting the unearned increment due to the enhanced value of coal lands is paid by the consumer.

The owner of coal or mineral property rightly estimates its value as equal to the price for which it can be sold. Whether the property be worked under lease, the coal or mineral being paid for as it is extracted, or the property be sold outright and paid for at the time of sale, or in deferred payments extending over a prolonged period, is immaterial—the principle of valuation is the same in both cases. If payment be made in deferred payments, or by royalties extending over a long term of years, the net amounts to be paid, or estimated to be paid after deducting taxes and other expenses, are discounted and the computed present money value of the combined payments is taken as the value of the property. If such property is being worked by the owner, that does not alter the principle of valuation, and the operating profit—which is the owner's reward for working capital invested, business risks assumed, and for his own services—should not be credited to the property and used as a measure for determining its value.

In determinations of value based upon future revenue, the rate of interest used in calculating the present value is a most important item. When there is no doubt concerning the quantity and quality of coal or other mineral present in any property, the interest rate may reasonably be taken at the lowest current rate for stable investment securities, because the owner is reasonably secure against loss and the invested capital is subjected to a minimum risk. In fact, many capitalists are quite willing to permit investments of this kind to remain undisturbed for years, yielding no revenue, provided they show an increase in value equal to 2 or 3 per cent. annually.

On the other hand, it is almost impossible to secure funds for financing coal-land purchases or coal-mining operations by the issuance of bonds bearing less than 6 per cent. interest, and such bonds are often sold at a discount, or accompanied by stock or other bonus in order to effect their sale.

If we can agree upon an interest rate to use in computing the present value of deferred payments (and prospective royalties); if we can fix the present current royalty value per ton, either by existing contracts or by contracts possible of execution; if we can determine the quantity and quality of workable and recoverable coal or mineral; and if we can agree upon a rate of working that will insure the mining of all the deposit in a fixed time, we will then have the principal elements necessary for calculating the present value of the property. In addition to these it is of course necessary to know the cost of

looking after the property, taxes and miscellaneous expenses which are to be deducted from the revenue upon which the calculation of value is based. The essential factors are then:

1. Rate of interest.
2. Royalty per ton.
3. Quantity of merchantable coal (recoverable coal).
4. Rate at which coal will be mined.
5. Taxes and miscellaneous expenses.

This method is that currently used in commercial appraisals of coal properties made for guidance in purchasing or in selling, or as a basis upon which the property may be used as security for the issuance of bonds secured by mortgage.

It is subject to serious criticism on several grounds. It is apparent that for complete appreciation of the result reached the engineer should state the interest rate, royalty, rate of mining, time necessary to exhaust and total quantity of recoverable merchantable coal used in making the appraisal, because what may appear to be small differences in any of these factors may cause great discordance in the values as estimated by different engineers. The method is objectionable because it assumes a fixed royalty value per ton extending over the whole period necessary to exhaust. The history of coal mining development almost without exception shows a tendency to gradual increase in royalties, and while failure to recognize this tendency may be thought to constitute a factor of safety against overvaluation, it must not be forgotten that a decline in future royalty rates is not impossible.

For these and other reasons, it does not seem entirely safe to accept valuations made by this method unless they are substantially in harmony with prices at which similar property has been or can be sold; but in making this statement I do not intend to imply that current sales values should be accepted in preference to values calculated by this method, for the method frequently enables us to determine that coal lands in any locality are selling at less than their real value and that their purchase can therefore be recommended, and on the other hand it may often enable the owner to avoid the mistake of selling at a price materially below the real value.

It is, however, the custom of engineers to check valuation estimates by comparison with actual sales prices whenever this can be done.

But one of the five elements entering into such computation of value is fixed by nature—namely, the quantity of merchantable coal: all of the other factors vary with changing economic conditions. The rate of interest is fixed by world-wide financial conditions, modified

by local economic conditions; the tax rate is affected principally by local conditions, but indirectly may be influenced by State or national requirements; the rate at which the property can be worked is often entirely within the control of the owner, or owner and operator combined, and the royalty per ton, while affected by general economic conditions, is more likely to be controlled by local conditions.

Hence we may state that of the five factors which enter into such method of fixing value, one (quantity) is fixed by nature; one (rate of interest) varies with world-wide and also local financial conditions; one (taxes) varies with local governmental revenue requirements; one (royalty) varies with general economic conditions and is controlled by local competition; one (time to exhaust property) is largely within the control of the owner if he has the means and ability to secure rapid working. The problem to be solved is therefore one including one fixed quantity and four variables.

While the object of this paper is to discuss methods adapted to commercial valuations, a review of the methods used by the U. S. government may assist us in getting a broader view of the subject, especially in relation to the influence of facts that affect the commercial salability of the coal and the cost of mining. In the preceding review these have not been dwelt upon because the effect of thickness, quality, depth, etc., upon the value is measured by the royalty per ton, a thin coal, a poor coal, or a coal deep beneath the surface commanding correspondingly small royalty.

The method now used by the government for fixing the selling price of coal lands is outlined and discussed at length in a recent *Bulletin*[1] of the Geological Survey. The procedure involves the use of a system that has been invented to cover almost every conceivable condition, and to include most, if not all, of the variable physical factors that affect the value of coal land. In the order of their importance these are thickness, depth beneath the surface, and quality. Great ingenuity has been shown by Messrs. Mendenhall, Ashley, and Lesser in reducing to mathematical expression some of the schemes devised for simplifying the calculations by which the relative values of coal differing widely in thickness, quality, and depth are determined. It is of course evident that most such values are necessarily based upon arbitrarily assumed units, and the valuations are therefore not of the character required for commercial and financial purposes. This does not, however, constitute a valid or forceful criticism, because the government may ask any price for its lands, being under no obligation to sell at the true value or at any predetermined price.

[1] *Bulletin* No. 537 (1913).

Some of these assumed units are as follows:

Land containing coal having a thermal value of less than 8,000 B.t.u. on an air-dried, unweathered sample is assumed to have no value as coal land and is classified as non-coal territory.

Coal less than 14 in. in thickness is considered worthless, and coal of that thickness is likewise classified as of no value when it lies more than 500 ft. beneath the surface or when its heat value is less than 12,000 B.t.u., thus:

Heat Value. B.t.u.	Minimum Workable Thickness. Inches.	Heat Value. B.t.u.	Minimum Workable Thickness. Inches.	Heat Value. B.t.u.	Minimum Workable Thickness. Inches.
15,000 to 12,000, . . .	14	9,600 to 9,500, . . .	23	8,800 to 8,700, . . .	31
12,000 to 11,000, . . .	15	9,500 to 9,400, . . .	24	8,700 to 8,600, . . .	32
11,000 to 10,500, . . .	16	9,400 to 9,300, . . .	25	8,600 to 8,500, . . .	33
10,500 to 10,250, . . .	17	9,300 to 9,200, . . .	26	8,500 to 8,400, . . .	34
10,250 to 10,000, . . .	18	9,200 to 9,100, . . .	27	8,400 to 8,300, . . .	35
10,000 to 9,900, . . .	19	9,100 to 9,000, . . .	28	8,300 to 8,200, . . .	36
9,900 to 9,800, . . .	20	9,000 to 8,900, . . .	29	8,200 to 8,100, . . .	37
9,800 to 9,700, . . .	21	8,900 to 8,800, . . .	30	8,100 to 8,000, . . .	38
9,700 to 9,600, . . .	22				

It is assumed that a coal 6 ft. thick can be mined to the greatest depth at which any coal of given quality can be considered workable. The following formula, deduced from the Survey rules, illustrates the method adopted for calculating the depth to which a coal of any thickness less than 6 ft. and yielding any heat value between 8,000 and 15,000 B.t.u. may be considered workable.

$$\text{MWD} = \frac{\text{t} - \text{mwt}}{72 - \text{mwt}} \times \left(\frac{\text{B.t.u.}}{3} - 500\right) + 500$$

Where MWD is the required maximum workable depth in feet, t is the thickness of the coal in inches, mwt is the minimum workable thickness (as given in the above table) in inches; B.t.u. is the heat value of the coal in question, in British thermal units.

This formula is based on the assumption that the maximum depth to which any coal can be worked can be found by dividing the heat value by 3; thus a 15,000-B.t.u. coal is considered workable if 6 ft. or more in thickness to a depth of 5,000 ft., a 9,000-B.t.u. coal to a depth of 3,000 ft., etc.

The above formula, for coal showing 9,800 B.t.u., gives a maximum workable depth for a thickness of 48 in. of 1,989 ft., as follows:

$$\text{D} = \frac{48 - 20}{72 - 20} \times \left(\frac{9{,}800}{3} - 500\right) + 500 = 1{,}989 \text{ ft.}$$

while the same coal if 6 ft. thick would be considered workable to a depth of 3,266 ft., although a 4-ft. coal with 15,000 B.t.u. would be

classed as unworkable at any depth greater than 3,138 ft. This brings us directly to the issue underlying any such system of valuation, for it must fail if its results do not coincide with the experience of operators and financiers in the actual working of coal properties. It is patent that under exceptional circumstances only could a coal 6 ft. thick yielding 9,800 B.t.u. be workable to the same depth as a coal 4 ft. thick yielding 15,000 B.t.u. In many cases the coal of lower heat value would be absolutely unsalable and hence valueless, while that with the higher heat value would be in demand at the highest market price, so that an increase in thickness from 4 to 6 ft. cannot be thought to compensate for the difference between 9,800 and 15,000 B.t.u. It is of course true that the total heat units which can be obtained from all of the coal contained in any given area of two such coal beds will be nearly equal, that is, 6 ft. of 9,800-B.t.u. coal will yield nearly as much available heat as 4 ft. of 15,000-B.t.u. coal, but to put the two on an equal commercial basis it would be necessary to mine and deliver to the consumer 1.5 tons of the poorer coal to compete with one ton of the better coal, and, if the better coal were available, it is probable that the consumer would not accept the poorer coal unless he could buy about two tons of it for the price of one ton of the better coal. However, disregarding entirely the commercial phase of the question and considering the matter solely from an operative standpoint, it is evident, other things being equal, that it is impossible to mine 1.5 tons of coal from a 6-ft. bed at the same cost as one ton from a 4-ft. bed; but even if this were possible it would not justify the placing of the two coals upon a plane of equality as to workability, because the inferior coal while being produced at the same cost would have a value less than two-thirds that of the better coal.

After a consideration of many matters affecting the varying value of coal in the ground, the statement is made that one cent per ton of recoverable coal was finally fixed as a unit to apply to coal having a heat value of 12,500 B.t.u., existing at a depth of 500 ft. or less, and having a thickness of from 6 to 10 ft., allowances being made to increase or diminish this value for higher or lower heat values and for decrease in thickness and increase in depth.

Combining the stated rules and formula we find that the value, corrected for differences in thickness and heat values, may be shown by the following expression:

$$V_1 = \frac{\text{B.t.u.}}{12{,}500} \left(4T + T^2\right)$$

in which T is the thickness of the coal in feet;

B.t.u. is the heat value in British thermal units; and

V_1 is the value of the land in dollars per acre; while the value as finally corrected for depth is expressed

$$V = \frac{MWD - D}{MWD - 500} \times \frac{B.t.u.}{12,500} \left(4T + T^2 \right)$$

in which MWB is the maximum workable depth and

D is the depth of the coal whose value is sought.

The development of the rules which have made it possible to express the mode of treating the subject by algebraic formulæ is both interesting and instructive, and while in the opinion of the writer there is little prospect of working out a means for adapting them to the requirements of the engineer engaged in commercial appraisals they show the possibilities of analytical study of the subject better than anything that has been published.

It is hardly possible to discuss the subject intelligently without explaining more fully the principles adopted by the Survey as governing factors, and I therefore will abstract from the *Bulletin* above referred to some of the more important statements, some of which I believe are not entirely in accord with the experience of operators and investors. These latter are indicated by an interrogation mark inserted thus (?).

"Workability of coal . . . depends on factors of two types . . . the first type,—quality, thickness, depth,—is intrinsic . . . the second type,— . . . transportation and markets,—is extrinsic. . . . A new railroad may make a . . . district or may break it. . . . In a new coal field . . . only the most accessible, thickest and best coal can be worked at a profit. . . . It must be considered workable if its value . . . exceeds the cost of extraction. . . . More coal is unworkable because it is too thin than from any other cause. . . . The price paid for coal in the ground should be recovered by the investor during the early years of mining, when the cost is low, . . . and should be refunded within the first 20 years of the life of the mine. . . . If the royalty rate is 10 cents a ton, the value of coal per ton in the ground when a mine is opened is at least 5 cents (?) . . . 10 cents a ton is not far from the average royalty paid under private leases . . . 2½ cents a ton is a fair sale price for unmined coal that is to be mined immediately, (?) where 10 cents per ton is the prevailing royalty. The government valuations cannot take account of changes in competition, markets, transportation facilities, or freight rates, or other factors that affect the profit. (?) . . . The base price of government coal land is fixed at one cent per ton of 12,500 B.t.u. . . . equiv-

alent to one-tenth the assumed average royalty . . . the royalty is based on the profit. . . . Whatever affects profits affects the royalty. . . . The depth to which any coal can be mined is assumed to be directly proportional (?) to the B.t.u. value of the coal and inversely proportional to the cost of mining (?) for different thicknesses. . . . The value of coal is inversely proportional (?) to the cost of mining (see table on page 84)."

This last view is doubtless the result of abstract reasoning, it being held that if one coal can be worked at half the cost of mining another coal it must be just twice as valuable, because with the same labor we can produce from it a product of just twice the value. This is doubtless another example of the confusion in reasoning that arises from a belief in an inherent intrinsic value. If we are to reach sound conclusions it is essential to remember that the values we are dealing with are those that are measurable by the profits the material can be made to yield, and that the profit does not bear a fixed relation to the cost of production. In the example just cited it is easily conceivable that the profit in working the more cheaply mined coal might be ten times as great as in working the other coal, in which event it would be ten times as valuable instead of only twice as valuable. The adoption of such a principle or rule for determining the effect of the cost of mining upon the value of coal cannot be accepted as correct in principle. It certainly would be impossible of application in making commercial valuations. If two coals are of equal quality and will sell for the same price per ton any excess in the cost of mining one over the cost of mining the other will of course reduce the profit per ton by a sum exactly equal to such excess, and as the profit on coal is usually not more than 15 or 20 per cent. of the cost of production, large differences in the cost of mining, such as come from variation in thickness, depth, etc., must inevitably destroy all possibility of profitably working the more expensive coal. Such coal cannot be held to have any definable or appreciable value until the competition of more cheaply mined coal is eliminated by its exhaustion.

If the value is not inversely proportional to the cost of mining, it does not seem reasonable to estimate the depth to which a coal can be worked by a rule that fixes the depth as in such inverse proportion, and it is difficult to find any other reason that would justify such a broad statement. It does not by any means follow that because a coal can be mined for half the cost of working another coal that it can be worked to twice the depth. The similar assumption as to the relation between the heat value of any coal and the depth to which it can be worked does not appear to have sufficient force to be adopted

as a principle of general application. The interrogation marks in the above abstracts concerning the fixing of values at 5 and 2.5 cents per ton are intended merely to indicate the personal opinion of the writer that such values are much higher than any figures that can be used as representing general averages. Thousands of acres of coal are constantly being sold at prices ranging from 0.5 to 1.5 cents per ton, coal that is easily accessible to existing railroads, while for coal that is some distance from transportation prices commonly range from 0.25 to 1 cent per ton. These figures apply to large areas in our Eastern coal fields and are intended to illustrate prices of land containing coal of fair grade and of fair workable thickness. High-grade coal and coal of more than average thickness bring better prices, but the figures to which exception is taken are reached only in the case of the higher-grade coal in districts where it has become evident that the supply of such coal is or will be insufficient to meet the demand.

While it may be impossible to derive any formula that will be of real use in simplifying the commercial valuation of coal land, an algebraic expression of the principles involved may be of assistance in giving a clearer insight into the relations of the variables and constants with which we have to deal than can be gained by discussion alone.

The simplest form of an equation illustrating current methods of appraisal is,

$$V = c\ d\ P\ r\ T \quad . \quad . \quad . \quad . \quad . \quad . \quad (A)$$

where V is the value in cents per acre;

P is the profit in cents per ton;

T is the thickness of the coal in feet;

r is the recovery in tons per acre per foot in thickness—this may be taken at 1,000 to 1,300;

c is a coefficient whose value depends upon the time required to exhaust the property, interest rate adopted, etc., and will usually have a value between 0.1 and 0.5;

d is a coefficient to represent the proportion of total profit that is to be credited to the coal (the balance being credited to working capital, etc.), and can usually be taken at 0.3 to 0.6.

A 4-ft. coal yielding a total profit of 20 cents per ton, 0.4 of which is credited to the coal, on 1,200 tons per foot-acre recovery, with 20 years to exhaust, 5 per cent. being the interest rate adopted, with a

profit margin to the purchaser of about 100 per cent., may be used to illustrate, thus

$$V = 0.3 \times 0.4 \times 20 \times 1{,}200 \times 4 = \$115.20.$$

If no profit to purchaser be provided for, the value of c is about 0.65 and the value per acre will be $249.

Equation (*A*) shows at once that the value per acre increases directly in proportion with each of its factors. As r and T are fixed, and the variations of c and d in any given case will be confined within small limits, the important variable is P.

As this quantity, P (the profit), is the difference between the sale price realized per ton and the cost of production, which may vary widely, slight increases or decreases in the cost of production, in the sale price, or in both, may reduce its value to zero or increase it several times. Hence it is evident that the value per acre depends principally upon this one item, all others sinking into insignificance beside it.

The sale price is affected principally by the quality of the coal.

The cost of mining is affected principally by the thickness of the coal, and to lesser degree by the physical conditions, slate partings and depth beneath the surface.

To enter upon a discussion of the increase in mining cost due to physical conditions, underground and at the surface, to slate and other refuse in the coal bed, and to depth beneath the surface, would extend this discussion far beyond reasonable limits and it is therefore proposed to consider only the principal cause (thickness) of large differences in mining costs, and the principal cause (quality) of variations in sale price.

The term mining cost or cost of mining is used to include selling commissions, taxes, and all other costs incident to extracting and delivering coal to the consumer, excepting only freight and interest on capital invested.

For the purpose of illustration let us assume that in the markets where the coal is to be sold, coal of average grade, say 13,000 B.t.u. heat value, commands an average sale price (ASP) of $2.70 per ton, the freight rate being $1.50 and the net price $1.20 per ton. If the cost of mining (CM), including a selling commission of 10 cents, is $1 per ton, the profit (P of the above formula) is 20 cents.

But if the coal be of better or poorer grade, the actual sale price (SP) will not be equal to the current average sale price for coals of 13,000 B.t.u. value, but will be decreased or increased thus

$$SP = ASP \frac{B.t.u.}{13{,}000}, \quad . \quad . \quad . \quad . \quad (B)$$

and if the coal has a heat value of 14,300 B.t.u. the sale price realized should be about

$$SP = \$2.70 \frac{14,300}{13,000} = \$2.97 \text{ per ton,}$$

or 27 cents more than the average current sale price, thus increasing the profit from 20 cents to 47 cents. We thus have a condition that increases the value per acre by 135 per cent., the value of V in the preceding example being $115.20, which is increased to

$$V = 0.3 \times 0.4 \times 47 \times 1,200 \times 4 = \$270.72.$$

This example has been worked out to show graphically the predominating importance of the quality of the coal. The reason for this predominating importance is not generally understood. It is found in the fact that the operator having a coal worth 10 per cent. more than average competitive coals, collects this 10 per cent. not only upon the cost of mining but upon the freight also. The larger the freight rate the greater his profit. He also collects the additional 10 per cent. on the profit which he would derive from the sale of a 13,000-B.t.u. coal, for the normal sale price (SP) is equal to the cost of mining (CM) plus freight plus profit (P), thus

$$SP = CM + \text{freight} + P, \text{ and as } SP = ASP \frac{\text{B.t.u.}}{13,000}, \quad . \quad (C)$$

$$P = ASP \frac{\text{B.t.u.}}{13,000} - CM - \text{freight}, \quad . \quad . \quad . \quad . \quad (D)$$

and while the selling price is increased 10 per cent. in the above example from $2.70 to $2.97, the net price is increased from $1.20 to $1.47, an increase of 22.5 per cent., while the profit rises from 20 to 47 cents, an increase of 135 per cent. This ability of the owner of high-grade coal to collect premiums on the freight paid, whereby the higher the freight the greater his profit, is the reason high-grade coals naturally seek far distant rather than nearby markets, for in the nearby market the premium is correspondingly less.

This phase of the commercial end of the business, especially since the purchase of coal on a B.t.u. basis has extended to large consumers, is accountable for the very large prices at which high-grade coal lands are now sold, and the general appraisal of such lands in a class by themselves at valuations that are very much larger than can be justified by the average profits of the business.

It has already been stated that any increase in the cost of mining reduces the profit by exactly the amount of such increase. Increase

in cost is therefore extremely dangerous, for if we have a normal profit amounting to 15 to 25 per cent. of the cost of mining, very small changes in physical condition or decrease in the thickness of the coal may increase the cost by an amount equal to or greater than the normal profit. Increase in cost from poor management or other remediable condition (for the reason that such cause is remediable and should be remedied) is not considered as a factor affecting valuations.

Sufficient has already been written concerning the rapid increase in cost which attends each, even small, decrease in thickness. The critical point seems to be at about 4 ft., coal of 4 ft. being workable at very slight increase over the cost of mining a coal 5 ft. thick, and a bed 5 ft. thick being workable at almost the same cost as a coal 6 ft. thick; 6 ft. commonly being thought the ideal thickness, giving head room for men, mules, motors, etc., and requiring less timber or lighter timber than beds of greater thickness.

As the thickness decreases below 4 ft. the rise in mining cost is rapid, the cost in a 3-ft. coal often being from 50 to 80 per cent. higher than the cost of mining coal 4 ft. thick, and the increase in cost in coals less than 3 ft. thick is still more rapid.

In districts where coal 4 ft. thick is available in quantity, this difference in mining cost, unless accompanied by more than compensating difference in quality, absolutely prevents the working of coal 3 ft. thick. Thin coals existing under such conditions can be given no assignable present value except by assuming that in a given number of years the thicker coal will be exhausted and mining of the thinner coal will become possible. In most cases the time required to exhaust the thicker coal is so long that the prospective value of the thinner coal, when discounted to a present money value basis, shrinks to small proportions, becoming quite unimportant as compared with the value conferred upon the property by the thicker coal. For this reason it has become quite common to base commercial appraisals upon the thicker and better coals and to ignore the relatively thinner and poorer coals.

The algebraic formulæ have been introduced by the writer for the purpose of graphic illustration and without intending to suggest their use in the actual appraisal of coal lands, because it seems impossible to devise any equation to include the effect of variations in all the factors that tend to increase or decrease costs. The influence of depth, thickness, irregularities, partings, dip, roof, floor, gas (fire-damp), spontaneous combustion, dust, swamps, faults, floods, water supply and other local physical conditions cannot be represented by algebraic symbols.

Equation (*A*) is merely one expression of the method in rather common use for valuing mineral properties and has repeatedly been elaborated in technical literature in this and other countries. In the opinion of the writer it is about as far as we can go in attempting to formulate general rules for coal appraisals. It may be said of coal, as of metalliferous properties, that no two are exactly alike. Each property must be treated separately, its peculiarities studied, its coals measured, sampled, analyzed, and their heat value determined before it is possible to attempt an appraisal of value. In making this examination careful study should be made of all those conditions that may favorably or unfavorably affect the cost of mining. The average mining cost in the district in which the property is located will be the standard by which the property should be judged. If its mining cost be less than the average cost in the district, and the coal is of a quality equal to the average of the district, the profit will be correspondingly larger than average profits of other operations, and *vice versa*. In the same way great care should be taken to make sure of the quality of the coal, not only of its quality as it exists in the ground, but of its quality as mined and shipped to market. If these features are investigated with proper care it is usually quite possible to reach conclusions as to the value of any property that will stand the test of time.

In estimating profits the competition of coals mined in the same district and shipped to the same markets is not especially important, as it is this competition that normally fixes the average profit at from 15 to 25 per cent. of the actual net cost of mining, but it is important to look closely into the competitive relations of coal from other districts, this being especially true when appraising values in an undeveloped region. The coal of any district must be considered not only with reference to nearby coals but also should be compared with those of other districts that may sell their product in the same markets.

As an illustration of the importance of these considerations the successful working of relatively thin coals, often of rather poor quality, in Kansas and Michigan which justify values often approximating or exceeding $100 per acre may be quoted. Coals of like thickness and quality existing in the principal mining district of Pennsylvania or West Virginia would be considered valueless.

Enhancement in Value.

In the preceding discussion no reference has been made to the effect upon present values of future enhancement in value. The prices at

which city, suburban and farming property, thought to be in an improving position, is sold are usually higher than the present value as measured by net earnings. Central property in large cities often sells at prices upon which the rentals yield net earnings of 2 or 3 (or less) per cent. Investors in such property realize that in paying the price asked, they are anticipating the enhancement in value for 5 or 10 more years. Only the most stable kind of real estate is valued upon an investment basis, that is, upon its normal average net earnings.

Although it often may be desirable in valuing coal lands to make allowances for future enhancement upon present values, this can rarely be done. In the case of non-mineral real estate, the price that similar property commands usually includes full allowances for future changes in value, and this may also be true of coal land, but in many coal districts transfers of property are few and infrequent, the properties sold may not be similar to the lands in question, the real sale price may be concealed or misstated in the conveyance, so that it may be difficult if not impossible to determine current sale prices, or the relation of such prices to the value based on earnings.

This question has often arisen in controversies over the assessment of coal land for taxation, without, however, developing a rule or law of general applicability by which such controversies can be settled.

That coal land tends steadily to enhance in value cannot be doubted. Innumerable instances of such increase in value might be quoted, while depreciation in value has occurred in few districts and such depreciation has usually been the aftermath of an unwarranted inflation or has normally followed the mining out of the best coal. In some districts coal land has fallen in value through the competition of newly-opened coal fields supplying cheaper or better coal. It may, however, safely be assumed that the value of coal land grows steadily, keeping pace with the increasing demand for coal, and there seems to be no reason to doubt the continuance of this tendency for as long a period as the consumption continues to increase.

Royalties.

This tendency is well illustrated by the increase in current royalties. In districts approaching exhaustion this increase is rapid and reaches large proportions, especially in localities producing coal of high grade. In the high-grade steam-coal districts along the eastern edge of the Alleghany mountains in Pennsylvania and Maryland, coal originally leased at 6, 8, and 10 cents per ton, 30 years ago, has frequently been re-leased or sub-let at royalties of 12, 15, 18, or 20

cents per ton, and such later leases, made at the higher figures, have often been sold for a larger bonus per acre. In the anthracite districts the enhancement in value has long been recognized, and a portion of this increase is sometimes secured for the owner of the land by making the royalty a percentage of the price at which the coal is marketed.

Legal Principles Affecting Values.

As the value determined by the engineer may be used for other purposes than that for which the appraisal was made, the legal principles affecting valuations should not be overlooked.

By decisions in many parts of the country, constituting precedents not likely to be reversed, the principle seems well established that for the purpose of taxation all property (including coal land, or the coal separately as a portion of the real estate) must be assessed equally and equitably at its fair market value, or at a definite percentage of such market value. Many decisions have defined the market value as the price that can be obtained after the property has been publicly offered and advertised, but not necessarily the price realized at a forced sale at which the property is offered without upset price or limit. When the value cannot be determined in this way, other methods of fixing values may be held to be valid, but the tendency seems to be to enforce the same method in fixing coal values as that used for other classes of real estate. Methods based on the foot-acre, or any other plan depending upon the quantity of coal or upon the royalty, or other income, have not fared well as a basis for taxation. The situation in all States is not the same, differences in the State constitutions and laws governing taxation naturally causing differences in the decisions, but the general tendency is as already stated. As, however, there are constantly increasing difficulties in attempting to apply these principles to the valuation of coal lands, changes in the tax laws, even if they involve amendment to constitutions, are not at all improbable, especially in States in which coal mining is an important industry.

Issues of bonds secured by mortgages upon coal land or upon coal-mining rights, or issues of stock (as full-paid) upon coal or mining rights, should be based upon real values. If the assumed value be fictitious, or overestimated, the directors and officers of the company making such issue may in some cases be held personally responsible, although there may have been no intent to mislead or defraud, or the holders of stock—supposedly full-paid—may be held to be liable for the debts of the company to the extent of such unpaid value.

Appraisals of value may be needed by those intending to purchase

coal land, by owners contemplating its sale, by capitalists engaged in, or planning, its development, by brokers or middlemen aiming to make a profit by effecting its sale, by owners wishing to make it the basis of a bond or stock issue, by assessors for taxation, or by executors or heirs for the purpose of subdivision or allotment. As an appraisal made for any of these purposes may at some time be used for some other purpose, the appraisal should be accompanied by a complete statement of the facts, the method used in making the valuation, the object for which it was made, and the personal opinion of the engineer as to the relative importance, value, etc., of the known facts, not only as an explanation of the appraisal, but to guard against its inadvertent or intentional misuse.

Value and Sale Prices.

When the value indicated by current sale prices is greater than that computed from the net income the property can be expected to yield, the difference may be due to over-optimism or ignorance on the part of the purchasers, or it may represent value predicated upon future prospects for higher prices. Whether such future enhancement should be included in fixing values is not entirely clear. Decisions of the courts that valuations be made at actual sale prices require assessments for taxation to include such value, although it does not seem reasonable to compel the payment of taxes on a value that does not and that never may exist. On the other hand, the purchaser of stock in a coal company may be satisfied to have a portion of the par value of the stock represented by such possible future enhancement in value. When recent increase in value has occurred, and surrounding conditions promise a continuance of the tendency, valuations made for the guidance of buyers or sellers, executors and trustees, etc., and similar purposes, can quite properly include a reasonable allowance to represent such probable future increase.

Quantity of Coal.

It is commonly believed that coal has such regularity in thickness and quality over large areas that the quantity of workable coal can accurately be estimated from data furnished by a few openings or borings. This belief is not well founded. Throughout most of the coal fields of this country the coals are subject to variations in both thickness and quality, and reliable estimates can be made only after the area is thoroughly explored, either by shafts, pits, or open-cuts, by boring, or by both. It does not follow that because a coal is of certain thickness and quality, as shown by openings along the out-

crop on two sides of a hill, that the same conditions exist throughout the intervening area. Disaster has overtaken many operations based upon such an assumption.

With the extension of mine workings to distances of from one to four miles from the outcrop, we are gaining knowledge of the variations both in thickness and quality that was not available 10 or 15 years ago, and systematic boring over large areas has in recent years also added largely to our knowledge of these matters. We now know that coal beds are not usually of the same thickness and quality over large areas.

In stating quantities, to distinguish between that which is known, and which is only partly known and that which is surmised, the practice commonly used in valuing metalliferous deposits may conveniently be modified for use in coal-land appraisals, and the terms, positive ore, probable ore, and possible ore, used in metal mining, can be replaced by the following terms, which are suggested as being readily understood by those not familiar with technical mining literature: Developed (workable coal) tonnage; partly developed (workable coal) tonnage; and undeveloped (workable coal) tonnage.

To determine the average yield per acre, and the average thickness for use in making an estimate of value, we must either confine such estimate absolutely to the proved territory, ignoring that which is partly developed, and that which is undeveloped, or we must adopt some plan providing for the recognition of the factors whose value has not been fully ascertained, by including a portion of such partly known values. This may be done by assigning to these other quantities a certain weight to represent, as nearly as possible, the judgment of the engineer as to their relative value. As an example, if the estimates gave: Developed tonnage, 10,000,000 tons; partly developed tonnage, 5,000,000 tons; undeveloped tonnage, 20,000,000 tons, and if we give weights of 1, $\frac{3}{5}$, and $\frac{1}{5}$, respectively, to these three quantities, the tonnage on which the valuation estimate will be made is

		Tons.
Developed tonnage, ...	$10,000,000 \times 1$	10,000,000
Partly developed tonnage, ...	$5,000,000 \times \frac{3}{5}$	3,000,000
Undeveloped tonnge, ...	$20,000,000 \times \frac{1}{5}$	4,000,000
Tonnage to be used for valuation, ...		17,000,000

The same principle can be used in determining average quality and average thickness required for purposes of valuation. The method is open to criticism on the ground that it does not give results that are capable of being checked or proved, but it is not as objectionable as methods which ignore important known facts. The engi-

neer can protect himself from adverse criticism for adopting such a plan by plainly stating the basis upon which the estimate is made or by supplementing the estimate by separate appraisal of the value of the developed coal.

Value as Affected by Ownership.

If the owner of a property be able to secure its rapid exhaustion, either by leasing on royalty, or by installing a mining plant of large capacity, the value of the property will be much greater than if its development be delayed or its working prolonged over a long period. Depending on the period required to take out all the coal, and assuming that the coal is mined at a uniform rate, taking interest rate used for computing the value at 5 per cent., each $100 received in profit or royalty has a present value, depending on the period covered by operation of the property, about as follows:

If Exhausted in	Present Value is About
10 years,	$81.00
20 years,	65.00
30 years,	54.00
40 years,	40.00
50 years,	34.00

If the working be deferred for 30 years and 10 years be then taken to exhaust, the present value falls to about $15, the value declining to very small proportions if the time of working be postponed and the time required to take out all of the coal be extended over a long period.

Cost of Plant and Improvements as Affecting Value.

In the estimates used to illustrate this subdivision of the subject, the cost of miners' houses, store buildings, boarding houses, etc., is excluded because the operator may not find it necessary to erect such buildings and because the net revenue from them is sufficient to cover interest on their cost after full deductions for maintenance, insurance, amortization, etc., so that they do not constitute a charge against the profits of the operation—the investment being self-sustaining.

The cost of mining plant and equipment and its installation and the cost of development and improvement are often of greater importance than the cost of the property upon which the operation is to be started. Mining machinery and equipment and many other improvements must be considered as having an average life of not more than 20 years, and complete amortization of their cost is usually necessary in about that period, for even if a property has a life twice or three times as long, complete renewal, remodeling or rearrangement of the plant and improvements must usually be made after an operation has

reached that age. Whether this renewal be gradual, or involve remodeling to place the operation in competitive condition as compared with other newer mines, is immaterial; the result is the reinvestment in that plant of the sinking fund which was provided for its amortization. Provision must be made not only to cover interest on the cost of the plant, but to create and maintain a sinking fund, as well as interest and sinking fund to cover the cost of the land, before the operator can realize a profit. If the improvements and plant cost as much or more than the coal, the greatest concern of the operator will be to select a property that can be cheaply equipped. Before discussing this in detail it may be useful to consider the effect of the cost of the coal, whether this be paid for as a royalty per ton or by the outright purchase of the property.

Profit and Surplus Profit.

As in all competitive industries the profit is fixed by competition, and in the present case is measured by the difference between the cost of mining and the net sale price. The value of any property is directly affected by conditions that govern the profits of properties producing a similar or competitive product.

If the earnings are not large enough to pay interest and sinking-fund requirements of the capital investment in land and plant, it is evident that the investment in land or in plant or in both was too large; that the plant cost more than the conditions justified, or that the price paid for the land was too high, or that too much land was bought, or that the investment in both was too large.

The value of coal land to the owner is a sum upon which the profit will pay interest and sinking-fund requirements after deducting interest and sinking-fund requirements of the investment in plant and improvements. If the requirements of the latter are relatively large the coal-land value will be correspondingly decreased. In equation (A) the subdivision of the profit between land and plant investment is provided by the coefficient d, the value of which as shown below may vary between wide limits.

Failure to realize the importance of the time necessary to exhaust a property and of cost of the plant, etc., is responsible for many overestimates of the value of coal land. The following tables are introduced to illustrate the necessity for thorough study of each case before an appraisal of value is attempted. Table I. shows how the cost of coal per ton depends not only upon the price paid but also upon the quantity of land purchased, for land prices equivalent to prices of 1, 2, and 3 cents per ton of recoverable coal of workable

quality, charging interest on the purchase price at 6 per cent. per annum and with sinking fund to repay the purchase price when the property is worked out calculated at an interest rate of 5 per cent.

TABLE I.—*Relation of Price Paid and Time to Exhaust to Cost of Coal.*

Years Required to Exhaust Property.	Cost of Coal per Ton.		
	Price Paid, 1 Cent.	Price Paid, 2 Cents.	Price Paid, 3 Cents.
	Cents.	Cents.	Cents.
10	1.39	2.78	4.17
20	1.78	3.56	5.34
30	2.23	4.46	6.69
40	2.75	5.50	8.25
50	3.25	6.50	9.75
60	3.78	7.56	11.34
80	4.89	9.78	14.67
100	6.04	12.09	18.13

Those who are misled into purchasing at 3 cents per ton coal reserves for 100 years by a belief that the coal is cheap at 3 cents per ton because they can make a profit of 10 or 12 cents per ton of coal mined and shipped, are merely saddling upon themselves a loss of 6 or 8 cents per ton. The only remedy for such a condition is the increase in the capacity of the mining plant to mine the coal in 30 or 40 years, thus reducing its cost to about 8 cents per ton or less.

Cost of Plant and Improvements.

When the coal to be worked is of good thickness, 5 ft. or more, is nearly level, with sufficient grade towards the outcrop for good drainage, is soft and easily cut, lies from 25 to 40 ft. above the railroad shipping tracks, is in best demand as run-of-mines coal, is clean and requires no preparation other than care in mining, the plant and improvements will be of the cheapest and most simple type and the investment in plant and mine development may not exceed 50 cents per ton of annual capacity, or, say, $50,000 to $60,000 for a capacity of 120,000 tons (240 days at 500 tons) annually.

If the coal contains slate, requiring hand-picking, if its best market is as screened coal sold in several sizes, if it lies high above the railroad, if it is hard and not easily cut, if it is not self-draining and is not nearly level, the plant and improvements and development work may require an investment of perhaps $1 per ton of annual capacity. The difference is due to the extra cost of plane and equipment, to additional cost of shipping tracks, to cost of tipple with shaking screens and picking table, to larger power plant, more mining machines,

greater cost of driving entries and other mine development work, to expense caused by drainage troubles, etc.

If the coal is relatively thin, requiring more entry work at higher cost per foot, if it lies below water level and must be opened by shaft or slope, if the slack and nut require jigging, if the property lies some distance from a railroad and a branch road must be built to reach it, the capital required may easily be $1.50 and in some cases may reach $2, per ton of annual output.

Table II. shows the cost per ton chargeable to the investment in plant and improvements for these three types of properties, interest being taken at 6 per cent. and amortization calculated at 5 per cent. In this table plant A costs $0.50, B, $1, and C, $2 per ton of annual output.

TABLE II.—*Cost of Plant per Ton of Annual Output.*

Life of Plant.	Plant A.	Plant B.	Plant C.
Years.	Cents.	Cents.	Cents.
10	6.80	13.60	27.20
20	4.44	8.88	17.76
30	3.72	7.44	14.88
40	3.43	6.87	13.75
50	3.27	6.55	13.10

For reasons already given the life of a plant should not be assumed at more than about 20 years and the cost of plant per ton of coal should therefore be taken as 4.44, 8.88, and 17.76 cents for the three types above described. If these differences in cost of plant are unavoidably caused by differences in the coal, then they represent actual differences in the value of the coal per ton, for coal from one property will cost that much less per ton to produce and will therefore be worth (in the ground) 4.44 or 13.32 cents per ton more than that of the other properties, the differences in value between these two properties being 8.88. The differences in value in the case of a coal bed 50 in. thick, yielding 5,000 tons per acre, to be worked out in 20 years, amount respectively to about $145, $435, and $290 per acre. We here have an illustration of one reason for the great disparity in values often shown by actual sales of properties which on casual inspection appear to be of nearly equal value. Even this, however, does not fully exemplify differences in value caused by varying local and physical conditions and trade requirements, for it is almost always found that coals that call for expensive equipment cost more to mine than those that can be cheaply opened. The cheap plant may be just as efficient as the more costly installation, the higher cost of operation of the property costing more to equip not being appreciably reduced by a higher efficiency of the more expensive plant.

Ability to Meet Competition.

By combining the figures of Tables I. and II. we may obtain the interest and sinking-fund requirements covering the cost of both land and plant, these two items including the whole investment excepting the working capital to meet payrolls, current bills, etc. Table III. shows such combined charges based on amortization of plant in 20 years irrespective of the life of the property.

TABLE III.—*Interest and Sinking Fund Requirements (Includes cost of land, plant and improvements.)*

Years to Exhaust Property.	Price of Land 1 Cent per Ton.			Price of Land 2 Cents per Ton.			Price of Land 3 Cents per Ton.		
	Plant A.	Plant B.	Plant C.	Plant A.	Plant B.	Plant C.	Plant A.	Plant B.	Plant C.
	Cents.	Cents.	Cents.	Cents.	Cents.	Cents.	Cents.	Cents.	Cents.
10	8.19	14.99	28.59	9.56	16.36	29.96	10.97	17.77	31.37
20	6.33	10.68	19.58	8.01	12.46	21.36	9.79	14.24	23.14
30	6.68	11.13	20.08	8.91	13.36	22.26	11.14	15.49	24.49
40	7.20	11.65	20.55	9.95	14.40	23.30	12.70	17.15	26.05
50	7.70	12.15	21.05	10.95	15.40	24.30	14.20	18.65	27.55

An operation financed on capital borrowed at 6 per cent. must make net earnings larger than these requirements if there is to be any surplus available as profit to compensate the operator for the risks assumed, for his own time and for the use of the funds employed as working capital.

This table shows how an operator may be handicapped by the purchase of a large acreage of relatively high-priced land requiring the more expensive types of plant, and how almost impossible it will be for him to meet the competition of those who have bought a smaller acreage, or cheaper land, or land that can be developed by a less costly plant.

The higher carrying charges shown by this table are in excess of the average profits of the business, and operations so burdened can survive only when the product commands prices correspondingly higher than those obtained for the competing product or when the coal does not meet that of the less heavily burdened operations in open market competition. Coals of especially high grade, commanding premiums over those of ordinary grade, such as those especially adapted for domestic use, or for coke making, particularly for by-product coke, for use in gas producers, etc., can be made the basis of a successful operation even when the carrying charges equal or exceed the figures of this table, thus justifying valuations of 2, 3, or more cents per ton, or $100, $200 or more per acre for lands containing 5,000 recoverable tons per acre. Coals of average or inferior quality and

thickness, under present conditions, rarely warrant valuations as high as $100 per acre. In most cases the value of such coal is not more, and is often less, than $50 per acre.

On the other hand, coal of especially high grade, accessible to large markets, in thick beds, regular and persistent in both thickness and quality, to be quickly worked, may readily be worth $1,000 or more per acre. Small areas of workable coal even if of poor quality, when close to markets and far distant from better coal, may have surprisingly large value.

Value of Leaseholds.

As the lessee virtually is the owner of the coal, subject to the payment of the royalty, all increase in the value of the coal will accrue to his benefit. In many cases the value of the lessee's interest in the property exceeds that of the owner of the property. The value of a lease may be determined by the profit that can be made during the term of the lease, or by determining the value of the property as a whole and deducting from this the value of the royalty payments. Leaseholds can very properly be made the basis for an issue of stock or bonds and when so used there is no reason why they should not be appraised by the same methods used in fixing the value of lands held in fee.

Use of Formula.

Modifying formula (*A*) by the variables illustrated by Table II. and by formula (*D*), the value per acre may be expressed thus:

$$V = crT\left(ASP\frac{B.t.u}{13{,}000} - CM - f - 0.088\,n\right),$$

in which

V = value of land in dollars per acre.

c = coefficient depending upon the time necessary to exhaust the property and usually will have a value of between 0.1 and 0.5.

r = recovery in tons per acre for each foot in thickness.

T = thickness of coal in feet.

ASP = average selling price of coal of 13,000 B.t.u. value in markets where the coal is to be sold.

$B.t.u.$ = heat value of the coal.

CM = cost of mining.

f = freight.

n = cost of plant and development for each ton of annual capacity based on 6 per cent. interest and amortization in 20 years at 5 per cent.

Possibly future study of the subject may make it possible to derive a formula to express the effect of variations in thickness, depth, and other physical conditions and local conditions upon the cost of mining. The utility of such formula, however, is found principally in facilitating study of the subject, rather than in its actual use in computing results.

If the object of an appraisal is to reach a valuation representing the real value of any property, no fact is trivial or unimportant that has any bearing whatever upon the subject. Few realize the difficulties and intricacies of the issues involved in each such valuation. The object of the writer will have been attained if he has succeeded in showing how some of the problems can be studied and a better understanding gained of the real issues involved in determining coal-land values.

Discussion.

* George H. Ashley,† Washington, D. C.:—Mr. Chance's long practical experience in reporting on coal lands admirably fits him for a discussion of the value of such lands. On behalf of the government geologists, I wish to express my appreciation of Mr. Chance's friendly, constructive criticism of the government method of valuing coal land in the first part of his paper and, at the same time, to reply to some queries he makes.

All admit that coal has no "intrinsic" monetary value, in the sense that a pound of coal of certain chemical and physical characters is worth "intrinsically" so many cents or fractions of a cent. The term has, however, been found very useful as including those natural factors of coal value, among which are its position and location in the earth, that affect its sale price in contrast with the other cost elements involved in the labor and capital necessary in mining, preparing, transporting, and selling the coal (extrinsic factors). Mr. Chance, while recognizing the "dual origin of value," does not offer any better terms.

The Survey agrees with Mr. Chance in his definition of coal conservation, that it means the fuller utilization of the heat contents of the coal, and the more complete recovery of the coal from the ground. Coal land withdrawals have not been made to keep such lands off the market, but to prevent their acquisition as non-coal lands before they could be classified so as to be disposed of under the proper law. Nor has the pricing of the more valuable coal lands above the minimum been to raise money for the Reclamation Fund, to which the money

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† Chairman, Coal Section, Land Classification Board.

goes, but primarily to discourage the alienation, for holding or speculation, of coal lands not intended or needed for development, and, secondarily, to secure to the people the "unearned increment," which they are certain in any case to be charged.

Remembering that government prices of coal average well under 1 c. per ton, which with interest, etc., will hardly come to more than from 1.5 to 2 c. per ton when mined, as compared with the other charges against coal, such as the freight rate, which may run from 50 c. to $5 per ton or more, it is not quite clear how the government prices are going to be a burden on the ultimate consumer, particularly when, considered as a whole, that money has been in the consumer's other pocket or at interest for him. What the consumer objects to are the charges representing unearned increment on the land, unearned interest on watered stock, if such exists, unearned railroad rates where they are excessively high, etc.

Mr. Chance questions a number of the principles adopted by the Survey as governing factors, which he quotes. In view of the writer's belief that the cost of coal lands should be refunded within the first 20 years of mining, regardless of the life of the mine, it would seem from Mr. Chance's table under "Value as affected by ownership," that the value of a ton of coal at the time of opening of the mine is at least one-half the royalty rate. How much that rate should again be cut to allow a margin of safety depends on the local conditions in each case. For the "developed tonnage," to use Mr. Chance's term, one-half would seem to be a fair margin. In the government pricing, a safety margin of four-fifths or more of the estimated value is allowed.

Mr. Chance's question of the statement that "The government valuations cannot take account of changes in competition, markets, transportation facilities, or freight rates, or other factors that affect the profit," suggests that the statement should be modified or explained. Undoubtedly the government could take account of those things, but to do so would involve an enormous increase in the cost, not only of the field investigations, but of the office work in valuing the lands, probably doubling the cost and, with the present meager funds at the Survey's disposal, doubling the time required for the classification and valuation of the coal lands. There is also another side to this question. Coal lands far from railroad transportation today have practically no commercial value except for holding. To be utilized they must be brought into touch with transportation facilities. To sell such lands at their present actual commercial value would mean transferring their holding from the government to private owners, who must hold them at a high rate of interest, as compared

with the government, or it would mean subsidizing the builders of railroads to them. Coal lands near railroads are still too abundant to need any such subsidizing beyond the one-half price reduction now made for lands more than 15 miles from a railroad.

The last questions will be considered together:

"The depth to which any coal can be mined is assumed to be directly proportional (?) to the B. t. u. value of the coal and inversely proportional to the cost of mining (?) for different thickness. . . . . The value of coal is inversely proportional (?) to the cost of mining (see table on page 84)."

The Survey has taken the same ground as Mr. Chance, that the value of coal depends on the profits. No profit, no value; double net profit, double value. The depth to which any coal can be mined is that depth where the increasing cost of extraction extinguishes the profit.

The table referred to (page 84, *Bulletin* No. 537, U. S. Geological Survey) considers only the relative value of the *same* coal of different thicknesses. If a coal has no value unless it can be mined at a profit, to compare two coals of different thicknesses it must be assumed that the thinner coal can be worked at a profit. If a 2-ft. bed of a certain coal can be mined at a profit just vanishing, what will the profit be on a 4-ft. bed of the same coal? Obviously just the difference in the cost of mining between a 4-ft. and a 2-ft. bed. For the selling price, freight rates, etc., being the same, the profits in working the 4-ft. bed under conditions otherwise similar to those of working the 2-ft. bed, will just equal the difference between the cost of working the two beds. If the conditions be such that the minimum thickness for any coal can just be worked without loss, it is found that the profit in working beds of greater thickness approximates an inverse proportion to the cost of mining.

Frankly the facts are these: The study of the cost of mining different thicknesses of the same coal had brought out clearly the great difference in the cost of mining thin coals. As between the same coal in adjoining mines, a difference of thickness would undoubtedly affect the profits and to that extent the value of the coal. Could any definite relation existing between the cost of mining as affected by the thickness be found that could be systematically used in valuing coal lands? The premise given above was the only one that offered any definite answer, and, while it may never be quite correct in comparing the value of two thicknesses of the same coal, it appears to give the closest average solution of all cases.

The preceding statement that the value of coal or the depth to

which it can be mined is proportional to the B. t. u. value is more open to question. It is approximately true from the standpoint of use; that is, that a ton of 15,000-B. t. u. coal contains as many B. t. u.'s as 1.5 tons of 10,000-B. t. u. coal and therefore is worth 1.5 times as much. Actually the first coal is worth a little more than 1.5 times as much as the poorer because of added cost after delivery to the boiler-room of the lower-grade coal, due to increased labor of stoking and removal of ash, increased storage capacity needed, and possibly to furnace difficulties. This may make a difference of say one-tenth to one-half between the two coals. Such a difference, however, may be largely offset by the greater ash content of the higher-grade coal. On the whole it is not far from right, considered from the standpoint of use where no smoke ordinances are involved, but only horse power per dollar of fuel cost.

More and more, large users of coal are studying their coal bills so that, as between two coals with the same losses through the grate bars and out of the chimney per B. t. u. and the same ratio of impurities, they will be rated as follows: A (cost of poorer coal per ton) $\times$ (B. t. u. of better coal divided by B. t. u. of poorer coal) + excess cost of handling and storage = B (equivalent cost of better coal per ton). If B, as so computed, is less than the market price of the better coal delivered, the poorer coal will be used. If B is more, the better will be used.

Leaving out of consideration household, coking, and blacksmith coals, which have special enhancing values and are specially treated in the regulations governing the pricing of government coal lands, the study of coal profits the country over in relation to B. t. u. value is confusing rather than enlightening. In general, such a study shows that coals of all degrees of heat values are being mined and marketed and that all of the profits are not going to the operators in high-grade steam coals. The old law of supply and demand soon levels down any high profit points, so that it must be doubted if the producers of Pittsburg steam coal are getting any larger profit per ton than a large share of the producers of sub-bituminous coal in the West. If the various coals were all grouped together in a small field, equally distant from a common market, either only one at a time would be worked or, if more than one, the profits on the better coals (cost and impurities being the same) would be governed by the B. t. u. value. But under the conditions actually existing each coal has its own field, whose size and limits are determined by the ability of reduced cost of production and B. t. u. value to overcome transportation charges against competing coals in every direction. In this competition the

better-grade coals have the advantage of wider markets, of being able to carry higher operating costs, of being able to meet higher freight rates to common markets, and of being minable at a profit against lower-grade coals in the same field. In the West it is not an infrequent condition to find coals of different ages and different qualities in the same sequence of rocks. So that, notwithstanding the general statements just made, even though all coals were to be ultimately mined at about the same close margin of profit, the better coals can enter the market sooner and are stronger to meet adverse conditions and, therefore, must be considered as having a higher value. That difference in value probably would be better expressed in a relation to B. t. u. that gave greater increased value to the higher-grade coals than that yielded by the simple proportion to the B. t. u. value; such as, for example, that expressed by the formula: price per ton of any coal equals its B. t. u. value divided by (10,000 + (15,000 — B. t. u. value)); but considering that the basing price per ton is the one element of the regulations that, more than any other, would be sought by the public and form the basis of judging the regulations as a whole, it seemed wisest to use the simplest relation possible, one that was readily intelligible without computations or uncertainty. In the case cited by Mr. Chance of a 6-ft. bed of 9,800-B. t. u. coal and a 4-ft. bed of 15,000-B. t. u. coal, it is more than probable that under present-day conditions, if the 9,800-B. t. u. coal is being mined at all, it is situated under such conditions that it sells f. o. b. mine at about the same profit as the 15,000-B. t. u. coal at its mine.

In conclusion, it is evident to any one who has studied this subject at all that it involves a great number of factors in which theory must constantly be checked by the facts of the industry and in which the primary need is still for the collection and assembling of facts. Each constructive paper, such as that by Mr. Chance, adds its quota to the solution of the problems involved. No attempt is made here to review the main part of his paper, which will need more careful study than was possible in the brief time allowed for preparing this review. Such study, it is hoped, will be given in the succeeding months, and I believe that I voice the sentiments of my colleagues in stating that the Geological Survey stands ready to modify at any time its procedure in valuing coal lands when convinced by presentation of facts that its methods are erroneous or can be improved.

Howard N. Eavenson, Gary, W. Va. (communication to the Secretary *):—Mr. Chance is undoubtedly correct in his statement of the

* Received Sept. 25, 1913.

essential factors in fixing the value of coal land, and also in his objection to the use of a fixed royalty value per ton in making the valuation, on account of the appreciation usually found in this value as time demonstrates the worth of any particular coal field. Against this may be stated the argument that custom has usually fixed the royalty value at an average figure for any particular field and that this amount should govern the final valuation, particularly where the work is done for taxation purposes. The custom, now being introduced in some new leases in the bituminous fields, of exacting as royalty a percentage of the selling price of the coal, will eliminate the objection stated to the use of royalty value, as the average amount paid under the new arrangement will undoubtedly give an accurate measure of the value of the coal.

Where the royalty, the quantity of recoverable coal, and the average rate of shipments—say, for the past five years—are known, it seems to the writer that the value of the coal land is the average price which will be paid for it at the royalty given and in the time which will be required to remove it at the known rate of shipment. For a seam from which can be mined 1,300 tons per acre-foot, at a royalty of 10 c. per ton, there will be paid, the year the coal is mined, $130 per acre-foot; if the life of the mine (the amount of recoverable coal divided by the average rate of shipment) is 40 years, the present worth of the price to be paid for the last acre-foot of coal mined, at 6 per cent., is $\frac{\$130}{10.28} = \$12.64$, and the average value of the coal in the mine is $\frac{\$130 + \$12.64}{2} = \$71.32$ per acre-foot. The amount of coal recovered per acre-foot may change, the life of the property may be decreased or increased as the rate of shipment varies, and the interest rate may vary with financial conditions, but the writer believes that this method, when used with the proper factors, will give results which will check very closely with the figures of actual sales.

The writer is in thorough accord with Mr. Chance in his remarks about the assumptions made by the U. S. Geological Survey *Bulletin* on the method used by the government in fixing the selling price of coal land. Competition from other districts has certainly much more to do with the value of coal land than has the cost of mining. As an instance of this, certain coal lands in Alabama and West Virginia may be cited. In both States the coal is to be mined by shafts of about the same depths, the thicknesses of the two seams are practically the same, the West Virginia coal is superior in quality to the Alabama coal, but the former is now considered of no commercial value, while expensive plants are being erected to mine the latter.

R. B. BRINSMADE, Puebla, Puebla, Mexico (communication to the Secretary *):—I find myself differing materially on but few points with Mr. Chance in his comprehensive and scientific treatment of coal-land valuation. On p. 113 he says:

"As it is thus clear that the consumer must repay to the coal operator the price paid for the land, the sale of coal land at 'coal land prices' by the government becomes in fact merely a method of raising money by taxing the consumer."

This view, that a higher price for land means necessarily a higher-priced coal, disregards entirely the effect of marginal land on prices, which I explained in my discussion [1] of Dr. Raymond's recent paper as follows:

The Essential Factors in Fixing Prices.

The rent of land is essentially a residual product, obtained by subtracting wages and interest from the wealth output; and this is true because both labor and capital must be continually supported and renewed or they perish, while land is self-supporting and self-existent.

In each industry there exists "marginal" land; *i. e.*, land whose product leaves no residuum after wages and interest have been paid. This marginal land varies in quality with commercial activity, but it may be described as the leanest land that has to be worked to satisfy the current requirements of the market. Though supply and demand may name the temporary price of a commodity, its average price is fixed by the cost of reproduction on marginal land; hence under normal conditions rent cannot enter into price, for marginal land is rentless. Only in the cases where all available marginal land was monopolized by some owner, who would not allow it to be used at all without the payment of rent, would rent become a factor in determining price.[2]

The rent of mineral land is usually collected as royalty, and, since the government has calculated its new coal-land prices from the basis of royalty value,[3] it is evident from my above quotation that these new prices will not affect the consumer, but will simply mean that the government will retain part of the land value which has hitherto enriched the coal-land speculator.

If the former policy of practically donating our coal resources to the first comer had not been discontinued, all our remaining coal of any value (present or prospective) would soon have become private

* Received Oct. 14, 1913.

[1] Our National Resources and Our Federal Government, *Trans.*, xliv., 633 (1912).

[2] See also J. E. Symes, *Political Economy*, chap. viii. Karl Marx, *Capital*, vol. iii., chap. 44.

[3] *Bulletin No. 537, U. S. Geological Survey* (1913).

property. Then could easily have arisen in the West what has already happened in the East for anthracite and coking coal; *i. e.*, all available marginal land being cornered the price of the output would cease to be competitive (set by cost of reproduction on marginal land) and become monopolistic, or "what the traffic would bear."

The unearned increment that Mr. Chance discusses on p. 114 does not ordinarily affect coal prices, since all true land rent or royalty is individually unearned and arises solely from productive or marketing advantages which valuable coal land possesses beyond those of marginal coal land. The enhanced prices secured for those coals whose marginal lands have been cornered permits, it is true, the collection of a correspondingly larger royalty by the land owner, but this extra monopolistic royalty is no more unearned than the original competitive royalty, the former merely increasing by business artifice the natural difference in productive advantages.

On p. 49 of *Bulletin* No. 537, U. S. Geological Survey, is the statement:

"It would be a long step backward to return to the early policy of using the public lands as a means of Federal revenue, and any lease law enacted should be so framed as to encourage development, prohibit speculation, and add nothing to the cost of the resource to the consumer."

This sentence seems self-contradictory, for any Federal land rent which did not absorb more than the natural competitive royalty on the coal output could not affect the consumer, whatever its aggregate addition to the revenue. Within this limit, the larger the land rent the less left for the speculator's share of the unearned increment and consequently the less hindrance to legitimate development by speculators.

It is an established canon of taxation that taxes on commodities increase their prices,[4] while it is only taxes on the residual factor in production or land rent that cannot be shifted to the consumer. Excluding artificial monopoly and under normal competitive conditions, the only individually-unearned increment is found in land rent. Being *individually* unearned, land rent must be earned *socially*, and as it fluctuates in quantity with industrial activity and the consequent necessary cost of government, it forms the natural source of public revenue for a democratic nation.[5]

Thus "the long step backward of using public lands as a means of Federal revenue" seems a return to sanity from the long night-

[4] Lawson Purdy: *Local Taxation.* J. E. Symes: *Political Economy*, chap. xvi.

[5] Henry George: *Our Land and Land Policy.*

mare of Federal indirect taxation on the consumer, which has been so largely responsible for the reckless extravagance of the Congress and for the creation of so many unearned fortunes for the recipients of Federal land bounties.

Personally, I deem the leasing system more desirable than the sale of Federal coal land even at the present enhanced values. Leasing is more favorable for the small operator, since it saves him any initial investment in land. It better conserves both life and natural resources, for it gives the government a closer control of the method of exploitation. Lastly, it gives ultimately a much larger Federal revenue, without affecting either consumer or working operator, as evidenced by the basis of land-value calculation on pp. 81 and 82 of *Bulletin* No. 537.

This royalty basis, of only 1 c. a ton for a 6-ft. vein of coal of 12,500 B.t.u., is stated to be but "one-tenth the assumed average royalty rate" for privately-owned coal. This is done "in order to give the prospective buyer an ample margin of safety." But is the government so hard up that it need thus sell for cash its coal resources at one-tenth of their leasehold value? The above "ample margin of safety" means undoubtedly, in many cases, an ample margin for speculative profit also. Lands requiring an expensive equipment can be leased in large areas and on long terms to protect the operator, while retaining for the nation their full rental value by arranging for royalty payments on a sliding scale, adjustable to future changes in productive and market conditions.

JOHN B. DILWORTH, Philadelphia, Pa. (communication to the Secretary *):—Mr. Eavenson, in his discussion of Dr. Chance's paper on Valuation of Coal Land, uses a method of determining the average present value of the coal tributary to an active mine based upon the coal being produced at a uniform rate over a given period of years and the assumption that invested capital be credited with 6 per cent. interest compounded annually. Thus, if an acre-foot of coal is worth $130 when mined, the present worth of such a unit to be mined 40 years hence is $\frac{\$130}{10.28} = \$12.64$; *i. e.*, $12.64 invested at 6 per cent. compound interest for 40 years will amount to $130.

He then states that the *average value* of the coal is $\frac{\$130 + 12.64}{2} =$ $71.32. This would seem to indicate that if the total number of acre-feet of coal tributary to the mine were multiplied by $71.32 the result would be the present worth of all the coal.

* Received Jan. 8, 1914.

That this is not the case is shown by the following example: If it be assumed that the coal is worth $100 per acre-foot when mined, that the life is 16 years, and interest compounded annually at 6 per cent., then the total present value of the 16 acre-feet of coal is $1,072 and the average value is $67 per acre-foot. Now this is not the figure obtained by taking the mean of the first and last year values ($\frac{\$100 + 42}{2} = \$71$), but is the present value of the acre-foot to be recovered eight years hence, or at the mid period of the mine's life.

In general, the average present value of such a unit coal area is the present value of the portion mined during the middle period of the life of the operation. In the example given by Mr. Eavenson this would be approximately $40.50 per acre-foot and not $71.32.

HOWARD N. EAVENSON, Gary, W. Va. (communication to the Secretary*):—The writer wishes to thank Mr. Dilworth for calling attention to the error in his calculations. The correct average present value should be $31.71 per acre, and not $40.50, as stated.

The writer does not agree with Mr. Dilworth, however, that "the average present value . . . is the present value of the portion mined during the middle period of the life of the operation," as, while this is the case in the example quoted by him, it does not necessarily hold true for longer periods; and for a life of 40 years, in the example originally given, the average present value will be in the twenty-fifth year instead of the twentieth, and as the life of the operation or interest rate increases the period of average present value will be found still more above the middle period.

The average present value over any period of years may be found from geometrical progression, as follows: Let A = average value, of which $1 is now the present value; a = first term = $1 + interest rate — at end of first year; r = constant ratio = (1 + interest rate); n = number of years; s = sum of terms;

$$s = \frac{a(r^n - 1)}{r - 1} \qquad A = \frac{s}{n} = \frac{\frac{a(r^n - 1)}{r - 1}}{n}$$

Should it be desired to use this method in practice, a set of tables for the average present value for a number of terms could be prepared, which would easily, by inspection, give the information wanted.

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Rock-Drilling Economics.

BY W. L. SAUNDERS, NEW YORK, N. Y.

(Butte Meeting, August, 1913.)

IMPORTANCE OF ROCK DRILLING.

IT has been estimated that the value of the mineral products of the United States is about $2,000,000,000 a year; that about $25,000,000 is expended annually for explosives and that about double this sum is paid in wages where blasting is done. The value of the mineral products does not adequately express the importance of the industry involved in the excavation of rock and ore because of the enormous sums of money expended in tunnel driving, in quarries, and in open-cut excavation generally. We know that the Catskill Aqueduct in the neighborhood of New York, which is now nearing completion, has cost some $165,000,000 and that about one-third of this may be rated as rock excavation. These illustrations are given in order to call attention to the great importance of an industry the extension and development of which in our industrial life depend so much upon the economics of rock drilling.

Reference has been made to the new Catskill Aqueduct. There are three aqueducts leading into New York, each built at periods separated by some 20 years of time. The first one, known as the Old Aqueduct, conveys the waters of the Croton river to New York City in a manner which reminds one of the old Roman aqueducts, the ruins of which are still in evidence. The engineers who laid out these old conduits sought to direct their lines around the hills and into the valleys, at all times avoiding the rock. When it was necessary to cross a river or a deep ravine, a bridge or causeway was constructed. High Bridge, over the Harlem river, is an arched masonry viaduct differing in no essential features from the aqueducts of the Romans. When the second Croton Aqueduct was built, about the year 1890, the art of drilling rock had been so far perfected, and the use of explosives, through the invention of dynamite, had become so general, that this waterway was planned and executed on straight lines, following rather than avoiding the rock. The second Croton Aqueduct is a tunnel through rock, about 32 miles in length; material gneiss, almost as hard as granite. Its construction was made practicable by sinking

shafts about one mile apart, thus providing multiple points of attack and also serving to ventilate the headings. This second aqueduct siphoned the water underneath the Harlem river. This 32-mile tunnel was built in five years. The Catskill Aqueduct, now nearing completion, not only followed the rock wherever it was available, but it passed under several rivers and actually conducts its water underneath the Hudson, where rock was sought and found at a depth of a quarter of a mile below the surface.

Pliny gives us a description of what, so far as we know, was the first long tunnel driven in the history of the world. This tunnel was about 3.5 miles long and from 6 to 10 ft. in diameter. Forty shafts were sunk in order to expedite the work, and it is said that 30,000 men were employed for 11 years in its construction. The fact that this tunnel and most of the ancient tunnels in the vicinity of Rome were driven through lime rock enables us to understand how it was even possible to build them. Limestone is the easiest material to perforate, and it is likely that here the ancient system of "firing" was used. Firing consisted of building a wood fire close to the rock and throwing water against the hot surfaces, causing them to flake. With lime rock the material would very likely be softened so that chisels might be used to advantage.

What is true of aqueducts is also true of railway tunnels and mines. The Hoosac Tunnel in Massachusetts developed, though it did not perfect, the rock drill. It was said after the completion of this work that only the treasury of a State made it possible to drive this tunnel by machinery. Had the mechanical engineer neglected the study of rock-drill economics it is doubtful that any further attempts would have been made to drive hard-rock tunnels by machinery, but the Hoosac was followed by the Musconetcong, driven from portal to portal by machinery. Then we have the Palisades, the Cascade, the Busk, and numerous other large railway tunnels made possible because the rock drill had been simplified and made efficient. In Europe the great Alpine tunnels, known as the St. Gothard, Arlberg, Simplon, and Loetschberg, one of them (the Simplon) being a continuous tunnel without shafts for a distance of 12.25 miles, were made practicable by the efficiency of the rock drill.

We seldom stop to think how important the rock drill has been in our industrial progress. Dr. Raymond has said that it is the foundation of all the work of the mining engineer. Railroads, even in America, might have successfully followed the foot-hills and hand drilling might have been a feeble substitute for the rock drill in leading through impassable barriers, but railway efficiency would have fallen far short of its present status and the cost of transportation by rail would have been greater than it is at present. In this we are able to see that the civil engineer might possibly have survived without rock-drill economics, but not so

with the mining engineer. The history of modern mining engineering is closely related to the history of progress in the art of building a rock drill. Our so-called precious metals would have been indeed precious without this valuable auxiliary. The wealth of the world, especially in its gold, silver, copper, zinc, lead, and tin, would have been enormously curtailed, limited by an inability to increase the output in proportion to the demand, and further limited by the fact that low-grade propositions could not have been worked at all. Examples of this are seen in the Homestake, the Alaska Treadwell, the mines of South Africa, and in most of the important mines of Michigan, which though once high-grade are now low-grade properties.

Evolution of the Rock Drill.

As late as October, 1910, E. M. Weston, of the Transvaal University College, writing on the subject of rock drills, said: "There is much vacant ground for careful and intelligent experiment and invention. Hammer drills are only now being developed, designs are changing every month, difficulties are being overcome and sometimes new ones encountered; nor has the last word been said in the design of piston drills." No better excuse may be given for this paper at this time than the fact, now generally admitted, that in the last two or three years the study of rock drill economics has been pursued so vigorously and so successfully that in design, material, and workmanship the rock drill of to-day is a superior machine, doing more work, standing up to its work longer and better adapted to conditions heretofore unknown than the machines of the past. Lighter in weight, and more easily handled by the operator, it is an aid to the miner in very materially reducing the cost of ore per unit of labor, repairs, and power consumption. There has in fact been an evolution in the rock drill from a piece of steel with a bit on the end of it, struck by the hand of an operator, to a similar piece of steel struck by a power hammer. Between these two extremes we have seen the jumper, the hand-operated drill, and the power-driven machine, which started with the cumbersome rock drill of J. J. Couch, about 1850, followed by the reciprocating machine of Joseph W. Fowle a few years later, Fowle's original idea of the cutting tool being an extension of the piston rod continuing to hold supremacy in many simplified forms up to a very recent period.

It is interesting to follow the evolution of the percussive type of rock drill. We must bear in mind that there are two distinct types of power-driven machines—the reciprocating and the hammer drill. Fowle's first machine was mounted upon wheels and weighed several thousand pounds. The Burleigh drill, which was a development of Fowle's invention, Burleigh having purchased the Fowle patents, was a machine mounted either upon a carriage or a tripod, but in any case it was difficult to get

the weight below a thousand pounds. André in his admirable work on coal mining, published a generation ago, states that a machine rock-drill should be "simple in construction and strong in every part." He follows this with the statement that it should be "as light in weight as can be made consistent with the first condition." The problem in percussive drills has always been to reduce the weight, provided in so doing the strength of the machine is not brought below the breaking point. The struggle to reduce the weight at first met with considerable success when air pressures of from 40 to 60 lb. were in vogue. It was found, however, that it paid in mining work to use high air pressures and this at first interfered with the efforts made by engineers to bring down the weight. There was even a period, some 20 years ago, when some engineers advocated heavy rock drills as best and most efficient in the end. Successful contractors specified large machines, claiming that by their use they were able to increase the air pressure, to get a heavier blow, to save up-keep expenses and to reduce the cost of excavation. Heavy drills invariably called for heavy mountings, and here is where an added difficulty was experienced, because heavy mountings were cumbersome underground, they were in the way, it was necessary to employ more men to handle them and they could not be used at all in narrow places. Here began the struggle for supremacy between the percussive and the hammer types,—a struggle which has recently resulted in the adoption of the hammer type as the most useful drill for general mining purposes.

The Modern Rock Drill.

The mining drill at the present time is a machine which weighs from 60 to 150 lb. It is largely a one-man machine, though under many conditions of work it is still best to add a helper. The percussive or piston type still holds its supremacy for heavy work, even in mines where large stopes are encountered, as, for instance, at the Homestake, but this percussive drill is now a machine which safely withstands pressures of from 100 to 110 lb. and its weight has been considerably reduced because of changes made in both design and material. This type of drill now used in the stopes at the Homestake weighs 137 lb. unmounted, and mounted on column with arm about 375 lb. Its work is chiefly in down-holes. Each part of the machine represents a study in material. The metal itself and the treatment it receives in the shops are both regulated in accordance with the work that each particular part has to perform. All our knowledge of metallurgy is taken advantage of in the construction of the drill. The cast iron is not ordinary cast iron. It resembles it only in that the metal is cast. The composition is made up to suit rock-drill service and the metal is treated with special reference

to the work it is to do. The steel is not common steel, but it is alloyed to suit each particular part. It receives a hot, crude oil bath and it goes through many processes before it is finally coupled up with the other parts into a complete drill. Special metal and special treatment are not confined to the piston or percussive type, but they apply equally to the hammer type. In fact, it was because of the necessity for lighter weight and greater strength in the hammer type that the study of the metallurgy of the rock drill was initiated and carried to a successful issue.

The first great improvement made in the piston or percussive type has been in the metal used, and this has resulted in greater strength, greater durability, and a lighter weight of machine for higher pressures. There has also been a change in design, which in the main has been confined to the valve motion. The chief aim of the designer has been to get greater speed. This means a greater number of blows, and in order to do this valves have been provided which open and close quickly and which have large ports. It is obvious that, other things being equal, a piston type of drill of large piston area will do more work than the same type of drill with a smaller area. The larger machine, which drills more, is handicapped by its weight, and when the net efficiency is figured up it has been found that it sometimes pays to get less drilling capacity with a machine which is more readily handled. Here the question of upkeep is introduced, because generally speaking the heavy type of machine costs less for repairs than the lighter type. The designer, taking all these things into consideration, has sought to increase the diameter of the piston so as to provide the drilling capacity of the heavy type with a machine of considerably less size and weight. This has in a measure been accomplished by the use of superior material and a design which shortens the piston, putting the extra bearing into the front head, where it is lighter. Through the use of a type of valve known as the butterfly a quick opening of large area is provided, thus increasing the number of strokes and thereby bringing up the drilling capacity. This has been done with no increase in air consumption that is not compensated for by increased drilling power.

Air consumption in rock drills is much misunderstood. Assuming that the piston and valves are tight, in other words, where there is no leakage, air consumption is usually dependent upon the number of strokes delivered. It is assumed, of course, that a constant diameter and length of stroke are used and that the pressure is uniform. It is plain that if we are able to utilize air or steam at 100 lb. gauge pressure for the full length of stroke when a percussive drill delivers its blow we are going to get the best results in drilling capacity; that is, we are going to get the hardest blow that is commensurate with the diameter, length of stroke, and pressure of the machine. If this blow is too hard, that is, if it breaks the steel, destroys

the bit, or creates a condition where the drill is unmanageable on its mounting, then we have the alternative of reducing the size of the machine and in this way getting lighter weight, which is always desirable when it is consistent with the other conditions. We all know that the class of rock usually determines whether or not we are striking too hard a blow, but assuming that the class of rock is uniform, or that it is determined and understood, the engineer is obviously justified in providing a drill which will strike the hardest blow that the machine and the rock will stand without destructive consequences. We see, therefore, how important it is to start with a machine which has a valve motion and ports so designed that the full power pressure will follow the piston the full stroke until the blow is delivered. Having this condition, as light a machine should be used as will stand up to the work.

It naturally occurs to one that a quick-opening valve and a large port will bring about greater speed, but the question is asked, is not this condition wasteful in air consumption on the return or back stroke? It will be generally admitted that it pays to get the blow, but why should the same conditions that give us the blow obtain when the piston returns for another stroke? There are two reasons why this is advantageous. One is, that the pull-back on a piston type of rock drill is sometimes of as great importance in the long run as the blow. A weak pull-back reduces the speed of the machine, causing it to come back more slowly than it went forward, and it has the further disadvantage that holes that are not straight, or which are out of round, and where seams and other irregularities are encountered, will act to retard the steel during its reciprocations. This considerably reduces the efficiency of the machine, not only because it cuts down the number of blows delivered, but because it weakens the strength of blow. The steel is held back in its effort to reach the full stroke and a labored blow, with sometimes a shorter stroke, is the result. Now it is quite true that in good, clean, hard rock, without seams, and where holes are drilled to reasonable depths, it might pay to save air on the return stroke. As a matter of fact, this is always done in a piston drill because the diameter of the rod must be subtracted from the piston area. The percussive piston type of drill is naturally a compounded machine which hits a harder blow forward than backward, because it has the full piston area at one end and a reduced area at the other. It is not a difficult matter to regulate this to any degree desired by increasing or decreasing the size of the piston rod or by increasing or decreasing the valve and port areas on one end of the stroke and not on the other. But every attempt to compound a piston type of drill by putting a pressure in front of the piston is a mistake. Just in proportion as a pressure is introduced in the front end it would cushion or restrict the force of blow, and in doing this, as has been previously pointed out, we make it necessary

to increase the weight of the machine in order to get the effective maximum blow. It is, therefore, a very dangerous expedient in the design of piston drills to attempt to compound the stroke. Reduced air consumption is easily effected at the sacrifice of efficiency. Air at 100 lb. is delivered to a percussive drill at a cost that varies from 40c. to $2 per day. To save 25 per cent. of this is all that compounding could reasonably be expected to accomplish, and this at the maximum is only 50c. a day. Experienced engineers will have no difficulty in seeing that there are many ways underground by which this and larger amounts may be lost through a machine which must inevitably be weakened in certain other directions in order to effect a small saving in air economy. Under most conditions of service it pays to conserve labor and upkeep expenses, giving first consideration to those things which cost most. A small reduction in drilling capacity, or a few idle hours, means an expense which will easily run in excess of any possible saving in air.

The hammer type of drill is a natural air saver, and it is in the design and construction of this type that air economies may be effected safely and without sacrifice. The hammer drill is essentially a machine for mines. It represents the evolution of the rock drill from the piece of steel struck by a hammer through the various stages of percussive machines back again to the hammer-driven blow upon the steel, the difference being only that the blow is a rapid power blow. A hammer drill is economical in air consumption because, in the first place, it reciprocates a light plunger which weighs only a few pounds, while with the piston drill not only must the heavy piston be thrown backward and forward at high speed, but it carries with it the steel and bit. In hammer drills the power is utilized, not in overcoming the inertia of a heavy body, but in compensating for that inertia through the high speed of a light body. A heavy mass moving slowly may give the same impact of blow as a light mass moving rapidly. The effective work done at the bottom of the hole is represented by the weight multiplied by the velocity. The same effect may be produced by subtracting from the weight and adding to the velocity, or *vice versa*. In a piston or percussive drill, velocity is limited, while in a hammer drill it is practically unlimited, and here is where the great possibilities of hammer drills have come in.

We must always bear in mind in comparing piston and hammer drills that the piston drill is handicapped by the load of the piston and steel, which has a certain inertia difficult to overcome in our efforts to reach high speed. The stroke is necessarily short, and as the hole gets deeper this handicap of weight is increased by longer steels, so that we are driven to high air pressures in order to get high speed. High air pressures naturally cost more money than low air pressures, and, as has been shown, if we attempt to save air by compounding we limit the capacity of the

drill to force itself through difficult places. In holes at or near the horizontal there is always an added disadvantage in a percussive type of drill, due to the steel dragging in the hole. This will take place even in a clean, straight, round hole. The steel sags, and in sagging, and during the process of rotation, there is considerable friction loss within the hole. All this leads us to high pressures, which is only another expression for greater power. It is safe to say that the piston type of drill has reached its limit when we consider capacity as a limitation when rating efficiency. In down-hole service, especially for deep holes and in soft rocks, piston drills will always be useful. The pumping action of the bit serves to agitate the material at the bottom, especially when mixed with water, and in this way the hole is kept more or less clean under the bit.

The study of rock-drill economics has led the mechanical engineer into the hammer-drill field as one which offered the greater possibilities. The problem was to do more work and with a lighter machine. The next consideration was to accomplish this with a reduced labor and power cost. All of these conditions have been met and, as subsequent figures will show, extraordinary results have been obtained which have materially reduced the cost of excavating rock and ore. We have seen that the process has been one of return to primitive methods. In other words, we have done what is most natural, and that which conforms closest to the laws of Nature is invariably best. The natural way to drill rock is to strike a piece of steel with a hammer. The only reason why the miner does not continue to do things in this way is because he cannot strike a sufficient number of blows. Just in proportion as he uses a heavier hammer does he reduce the number of blows that he is able to maintain and with the lighter hammer he is brought to an absolute limit. It would seem that the first thought of the engineer would have been to follow the old miner by building a rock drill on the hammer type. He may have thought of this, but the problem proved to be a very difficult one. The first difficulty was to get material that would stand up against this rapid-fire system. Then came the question of rotation, which was not easy to accomplish with a machine of the hammer design. Of equal importance was the question of keeping the bottom of the hole clean, because the bit being practically stationary at the bottom, the cuttings from the hole would pack between the edges of the bit and the bottom of the hole and prevent further progress. Jets of steam, of air, and of water were used to discharge the cuttings. Steam has many disadvantages, air is expensive and it creates dust, while water is, in the first place, difficult to introduce into the bottom of the hole, its mixture with parts of the drill results in wear and tear, and to use much water is a disadvantage and an expense in underground work. Up-hole work offers less difficulties for hammer-drill service than any other. The cuttings drop by gravity out of the hole

and the only disadvantage is dust. Horizontal hole work is that which is most difficult, while with down-holes water thrown into the hole always serves a useful purpose. A mixture of air and water has solved most of the problems arising from the use of hammer drills in mines. This is known as the Leyner system. Air is always available and is readily conducted in the hole, using either live or exhaust air. Where this air is commingled with water it discharges the cuttings from the bottom of the hole without blowing them away in the form of dust, but by simply reducing them to a puddle condition with enough water only to create a moderate stream, which discharges the cuttings through the orifice of the hole. This keeps the bit cool, there is enough puff to the air to remove the chips, and a long experience under all conditions of service has demonstrated conclusively that a mixture of air and water is more effective in cleaning the hole than even a large stream of water alone. In fact, air is the ideal thing to use, and it would be used alone were it not for the dust, so that the present system introduces only enough water to lay the dust. In doing this we effect economies by using only a small amount of water mixed with air from the exhaust.

The pneumatic tool, especially the type known as the riveter, illustrates the mechanical principles involved in the hammer drill. It is likely that the perfection of the pneumatic tool led to the perfection of the hammer drill. A riveter combines an extraordinary amount of power. Its efficiency is very high because the hammer speed is high, and the machine is light and easily handled. Its use in steel and iron construction is now universal. Air consumption in a tool of this nature is low in proportion to the work it does, because the thing reciprocated by the air is so light that it is easy to get high speed without sacrificing power to overcome inertia. In other words, there is a closer relation between the air pressure and the speed of hammer, with the resultant effective blow.

A point not to be lost sight of is that the rapid reciprocation of a flying piston, as in a hammer drill, is very much more easily mounted or held by an abutment than where we have a reciprocating action of a heavy weight, as in the case of a piston drill. The reasons for this are obvious. High speed of a light hammer takes the place of slow speed of a heavy hammer. One is like the rapid reciprocations of a hand hammer, the other the ponderous swing of a sledge. So great an effect is obtained by this rapid, light blow that it has been found practicable to reduce the weight to a figure considerably under 100 lb. in a machine which in drilling capacity compares favorably with a piston drill of two or three times the weight. In this light machine, material alone has not enabled us to cut down the weight, but the light rapidly moving piston design, with the quick-opening valve, is of equal importance. It would surprise a drill runner of 10 years ago to learn that hammer drills are used to-day in hard rock, putting

in holes 10 and 15 ft. in depth without even mounting the drill, it being held in the hand of the operator. Of equal importance in tunnel driving is the fact that by the use of hammer drills the equivalent of heavy machines is placed in the headings mounted upon a light horizontal bar, this bar being easily handled by a few men, yet it affords abutment enough to resist the jar, because the jar has been reduced to a minimum. Heavy upright columns, carriages, and other forms of support were made necessary because of the ponderous pulsations of the piston drill. To be able to use a light bar, placed horizontally, is of the greatest importance in tunnel driving because it affords an opportunity to begin drilling quickly after a blast and before the muck has been cleared away from the bottom of the heading. This has been referred to in detail in the paper of D. W. Brunton, Notes on the Laramie Tunnel.[1]

An effort has been made in the foregoing statements to analyze and give reasons for a condition in rock-drill service which is now in practical operation.

A hammer drill is the modern progressive miner. It has practically done away with hand drilling and in doing this it has largely increased the field of service. There is no longer any question about the fact that it pays to use power drills in all classes of mining. It is now entirely a question of adapting the system of mining to these light, efficient, handy perforators. In August, 1912, there was held at Calumet, Mich., what is perhaps the last contest between hand and power drilling. A three-man double-jack team of the best drillers in the copper country was pitted for a money prize against a small hammer drill of the Butterfly type. This machine was operated by one man. The drilling was in a block of Cape Ann granite, 60 in. thick. The three-man drilling team started with a 1¼-in. bit and finished with a ⅞-in. bit. The one man with the Butterfly (this is a light hand hammer drill, of the usual plugger type, weighing about 40 lb.) used a 2-in. starter and finished with a 1¼-in. bit. It required 14 min. for the machine to drill entirely through the block (60 in.), while the hammer-and-drill squad put in a 49-in. hole in the same time. The machine did 20 per cent. more work with one-third the labor cost. As a matter of fact, no such rate of hand drilling as was shown by the three-man squad could be maintained over a working shift, while there is no reason why the machine drill should not keep up its speed indefinitely.

Classification of Rock Drills.

It may be interesting to classify the present types of drills as follows:

1. The plugger drill. This is of the hammer type. It is used in its

[1] *Trans.*, xliii, 99 (1912).

smallest sizes for dressing stone, for trimming, cutting hitches, and for block holes. It is a hand-rotated machine.

2. The Jackhamer. A hammer drill with automatic rotation, used for sinking shafts, for down-hole work in stopes, for quarrying, for drilling in coal, and in rock and ore work wherever down-holes are required. It is held in the hand of the operator and in some mines is used for horizontal work, mounted upon some simple form of support.

3. The stoper. A hammer drill with air feed, usually used without mounting and for up-holes. It has a large field of usefulness, mainly in stopes or rooms and in driving raises.

4. The mounted hammer drill, as exemplified in the Leyner type, used mainly for horizontal, or approximately horizontal, hole drilling, for side stoping, and for driving drifts and tunnels. In this type of drill a combined stream of air and water is discharged through a hollow steel at the bottom of the drill hole.

5. The reciprocating drill, used for heavy down-hole drilling where the stopes are large, and for surface work, drilling deep holes of large diameter.

Fig. 1.

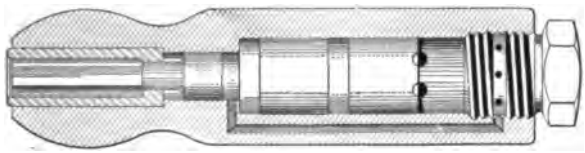

Fig. 2.

FIGS. 1 AND 2.—SIMPLEST FORM OF PLUGGER DRILL.

These stone tools are designed to meet the usual requirements in stone dressing. The several sizes cover a variety of work, from the heaviest cutting to the most delicate tracing. The tool is valveless, hence there is but one moving part and that a hardened piece of steel.

Figs. 1 and 2 are a photograph and a sectional view of the simplest form of plugger drill. Fig. 3 shows the machine in use in dressing stone.

Plugger drills are largely used for pop shooting, breaking up boulders, trimming walls, and for all light work requiring holes not exceeding 5 ft. in depth. Solid or hollow steel may be used. In the latter case the exhaust air is discharged at the bottom of the hole for the purpose of removing the cuttings. This type of machine is equipped with a simple device for

cleaning the holes of rock cuttings which may not have been discharged by the normal process of exhausting in the hole. This device consists of a lever, eccentric in its middle portion and located on the side of the valve chest. A portion of the lever forces a plunger against the valve, stopping the action of both the valve and the hammer piston and thus permitting

FIG. 3.—DRESSING STONE WITH PLUGGER DRILLS.

live air to pass to the bottom of the hole. Where steam is used wooden handles replace the metal ones. Fig. 4 shows the plugger drill for drilling holes, while Fig. 5 is a view showing the drills in operation.

1. The Plugger Drill.

Specifications of Stone-Working Plugger.

Size Symbol	AA.	AB.	AC.	AD.
Weight, pounds and ounces	1—5	2	3—9½	5—1½
Length over all, inches	5½	6½	7¾	8¼
Outside diameter, inches	1⅜	1½	1 11/16	1⅞
Diameter of piston, inches	⅝	¾	1	1¼
Total length of stroke, inches	11/32	⅝	13/16	17/32
Length of stroke to working point, inches	5/16	7/16	9/16	11/32
Air consumption, cubic feet	2	3	4	5.5

Specifications of the Plugger Rock Drill.

	BA.	BC.
Length, inches	22½	24
Cylinder diameter, inches	1½	2
Stroke, inches	3½	4
Steel used (hollow hex.), inches	⅞ or 1	1
Air inlet, inches	½	½
Weight of 23 type, pounds	43	54
Weight of 26 type, pounds	48	65

2. The Jackhamer.

The Jackhamer, Figs. 6 and 7, can be operated either by steam or air, it uses hollow steels, is provided with an automatic rotation and it drills

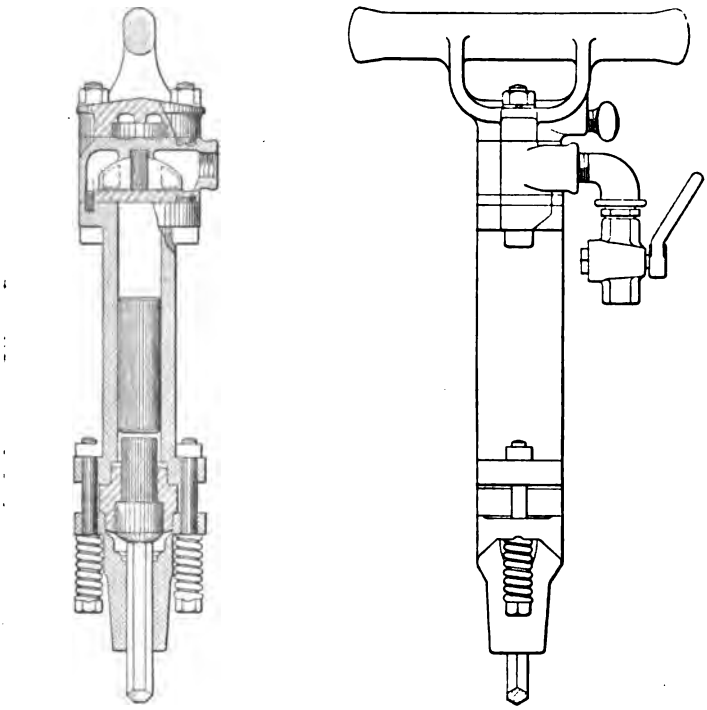

FIG. 4.—PLUGGER FOR DRILLING HOLES.

holes to a depth of 15 ft. It is essentially an all-steel drill. The cylinder, for instance, is drop forged, made of special steel, treated and hardened in the bore. Its most interesting feature is its automatic rotation, which is shown in the skeleton section, Fig. 7. Fig. 8 is a view of Jackhamers engaged in stoping in No. 15 shaft, seventh level, Osceola Amygdaloidal lode of the Calumet and Hecla mine, Calumet, Mich.

Specifications of Jackhamer.

Length, inches	18
Cylinder diameter, inches	2¼
Stroke, inches	2
Steel used, hollow hexagon, inches	⅞
Size of air hose, inches	¾
Weight, pounds	40

3. The Stoper Drill.

The stoper hammer drill is so well known and so generally used that further description is unnecessary. It is but a few years since this machine

FIG. 5.—PLUGGER DRILLS IN OPERATION AT GRAND CENTRAL TERMINAL, NEW YORK.

was invented and its application to general mining work has been very rapid. Thousands of them are in use. Fig. 9 is a sectional view and Fig. 10 shows a stoper in operation. In this particular case a jet discharges a spray of water for allaying the dust.

Specifications of Stoper Drill.

Piston diameter, inches	2
Piston stroke, inches	4
Length of feed, inches	22
Length of machine over all (closed), inches	52½
Length of machine over all (extended), inches	74½
Weight, pounds	73
Size air hose, inch	¾

FIG. 6.—VIEW OF JACKHAMER.

4. The Water Leyner Drill.

The water Leyner drill is shown in section in Fig. 11. Fig. 12 is a photographic view of a drill of the Leyner type driving an inclined raise in No. 15 shaft, Osceola Amygdaloidal lode of the Calumet and Hecla mine. This machine was in actual operation when the picture was taken.

Specifications of Water Leyner Drill.

Weight, drill only, 24-in. feed (standard), pounds	150
Length over all, 24-in. feed (standard), inches	47¾
Weight, drill only, 30-in. feed (special), pounds	160
Length over all, 30-in. feed (special), inches	53¾
Bore of cylinder, inches	2½
Length of stroke of hammer, inches	3
Size of air inlet, inches	¾
Size of water inlet, inches	½
Standard hollow round drill steel and shank, inches	1¼
Most economical depth to which holes can be drilled, feet	8 to 12

Air Consumption.

60 lb.	70 lb.	80 lb.	90 lb.	100 lb.
64 cu. ft.	75 cu. ft.	87 cu. ft.	98 cu. ft.	109 cu. ft.

Records of Recent Experiences.

There is no place where this new type of Jackhamer drill shows to better advantage than in shaft sinking and in bench or foot-wall work in mines and tunnels. For shaft work two or three times the number of

Fig. 7.—Skeleton View of Jackhamer.

drills may be used as formerly. Each man in the shaft who is not engaged in mucking is a driller, the two operations being carried on simultaneously.

At the Newport mine in the Lake Superior iron country a shaft is being driven with Jackhamers. Dimensions of shaft, 11 by 18 ft. in the clear. The progress has averaged 20 ft. per week, using five drills; material, hard quartzite. During February last a progress of 107 ft. was reported, and in March 125 ft., this latter being said to be the record of performance in shaft sinking in that district.

At the Lucky Star mine in the Lake Superior district four Jackhamers are working in a shaft 12 ft. 2 in. by 14 ft. 10 in., drilling ten 6-ft. holes for sinking and twenty 5-ft. holes for squaring after blasting; material, hard diorite. The progress has averaged about 100 ft. per month.

The Norrie-Aurora shaft of the Oliver Mining Co. was sunk 64 ft. in February, employing three 8-hr. shifts of eight men, each using Jackhamer drills, the shaft being sunk, steel timbered and concrete lined. The total

hours drilling was 794 man-hours, out of a total of 4,202 man-hours. In this performance the man-hours required for mucking were almost twice that of the drilling, whereas the time charged for blasting approximated that of the drilling; the timbering and lathing combined were slightly under the drilling figure. The remainder of the time was charged to squaring, cutting hitches, piping, etc. During March 105 ft. of shaft were driven and completed. This included not alone the sinking of the shaft,

FIG. 8.—STOPING WITH JACKHAMERS IN CALUMET AND HECLA MINE, CALUMET, MICH.

but the heavy steel sets were placed in position, the concrete slab-lathing was put in place for the entire distance, also the back-runners. The magnitude of the work may be better understood when it is realized that 24,600 cu. ft. of rock were broken and hoisted to the surface, and the labor of placing the shaft equipment, consisting of two skip roads, one cage road, ladderway, pipe compartment, counter balance and back-runners, completing a modern five-compartment shaft, was all performed in 26 days. Each piece of concrete slab-lathing weighed 130 lb. and the steel sets were made of heavy material. Six Jackhamers were used to sink the shaft.

The East Butte Copper Mining Co. is sinking a shaft through a hard

granite formation with two Jackhamers, from the 1,200-ft. level to the 1,800-ft. level, and reports show that from Jan. 20 to Feb. 15 of the present year 110 ft. of shaft were driven, including timbering. During this period they lost 11 shifts due to outside causes, no work whatever being done. Here an average of 28 holes was drilled per shift, 8 cuts 9 ft. deep, 8 lifters 6 ft. deep, 6 back holes 6 ft. deep and 6 end holes 6 ft. deep. Occasionally as high as 33 holes were drilled. The average net drilling time was 5 hr. This gives a total hole footage per shift of 192 ft., or 96 ft. per drill, an

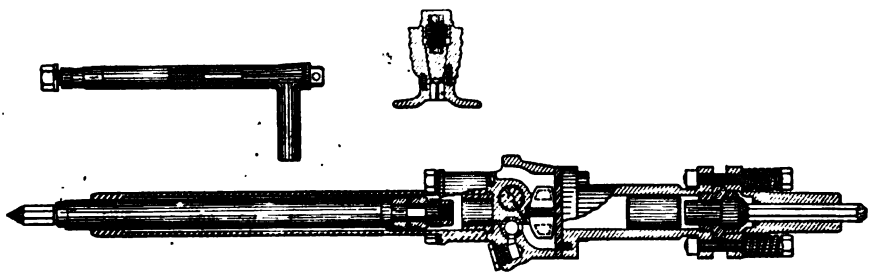

FIG. 9.—SECTIONAL VIEW OF STOPER.

average of 19.5 ft. per drill per hour. The previous best progress, made with heavy tripod drills, was 60 ft. in one month. Size of shaft, 19 ft. 6 in. by 6 ft. 10 in.

J. A. McIlwee, the contractor who drove the Laramie-Poudre tunnel, is sinking a shaft for the Silver King Consolidated mine, Park City, Utah. This is a three-compartment shaft and is being driven a distance of 500 ft. from the 1,300-ft. to the 1,800-ft. level. The work began Mar. 22 with four Jackhamer drills, two on each side of the shaft; four drillers were employed. A great many delays and drawbacks were experienced, mainly on account of water. Some boiler difficulties were incurred, which caused a delay of four days in April. Notwithstanding this, 105 ft. of actual sinking or 95 ft. of completed and timbered shaft have been accomplished. Based on the number of days actually at work sinking, this is a progress of about 5 ft. per day. Holes were drilled 5 ft. deep, the usual time employed being 10.5 min. per hole. The best time made in drilling was 33 holes 5 ft. deep, with 6-ft. cut holes, in 2 hr. 45 min.; three shifts, each shift gang doing the drilling, mucking, and timbering.

Bench or foot-wall work has heretofore been done with large drills, employing from two to three men per drill. Deep holes of large diameter have been drilled, but during recent years the hammer type, both in the stoper and in the Jackhamer, has replaced the heavier machines for this class of work. In one of the Lake Superior copper mines recent extensive tests have shown the following figures:

FIG. 10.—STOPER IN OPERATION WITH JET OF WATER FOR ALLAYING DUST.

Type of Drill.	Tons per Man per Shift.	Mining Cost per Ton.
Stoper	13	$0.41
Piston	12	0.54
Jackhamer	38	0.12

No overhead charges are included in the above figures of cost, nor are charges for air included. This effective saving by the use of stoper drills over the heavier tripod type, and the still further saving by the use of Jackhamers, cannot be maintained in all classes of work. In this particular case the bench or foot-wall afforded the best opportunity for the use of these little Jackhamer drills.

An analysis of the drill situation, in the iron fields of Lake Superior, indicates that four types of drills are necessary for the economical extraction of the oré, their relative proportion being about as follows:

	Per Cent.
1. Mounted hammer drill	20
2. Stope drills	20
3. Jackhamer or sinking drills	10
4. Light piston drill	50

There will always remain a small percentage of drilling for which the two-man piston drill will still be the favorite. An instance is the operation of the Soudan mine at Tower, where the rock encountered is very hard; in fact, it is characterized as the hardest in the United States.

At this time the ore is very hard and dry, and the chief problem is not so much one to be met by the use of some particular type of drill, but rather one of getting the steel to stand, as the bits will not hold in this rock. Records show that as many as 80 starters have been used, with a 3.25-in. machine, to drill a hole 6 in. in depth. Light one-man drills have been tried in this formation, but without success.

In the hematite mines of Lake Superior the light reciprocating mining drill is generally used for the actual breaking of probably 90 per cent. of the ore which requires drilling (hand-auger ground excepted). In this class of work the service required of the drill is light, the ground being classed as very soft drilling, but of such a character as to bring it under the classification of "too hard drilling" for hand augers.

The three charts, Figs. 13, 14, and 15, show a three days' drilling record at the Winona mine, in the Lake Superior copper country. They are typical of the modern trend toward scientific management and greater efficiency in mining operations and are largely a result of the occasional low prices for copper, the decreased percentage of metal in the rock in mines which have been operated for years, and the introduction of labor reducing and time saving devices for the mine.

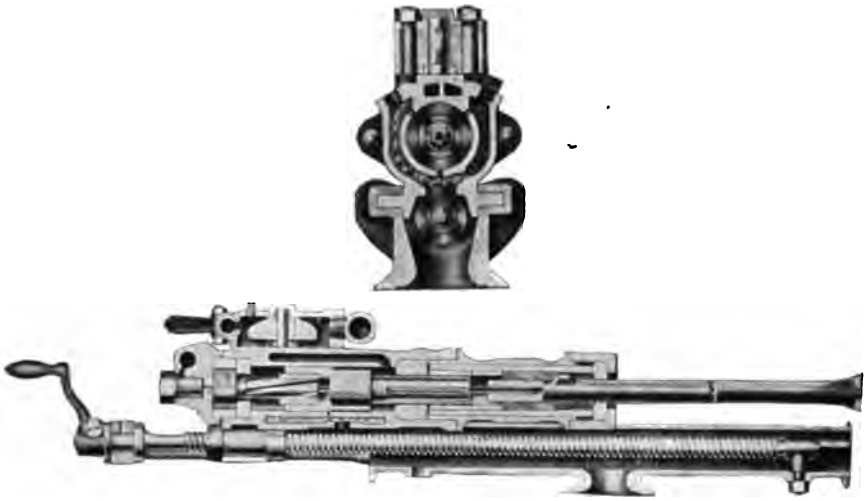

FIG. 11.—SECTIONAL VIEWS OF THE WATER LEYNER DRILL.

FIG. 12.—LEYNER TYPE OF DRILL DRIVING AN INCLINED RAISE IN THE CALUMET AND HECLA MINE.

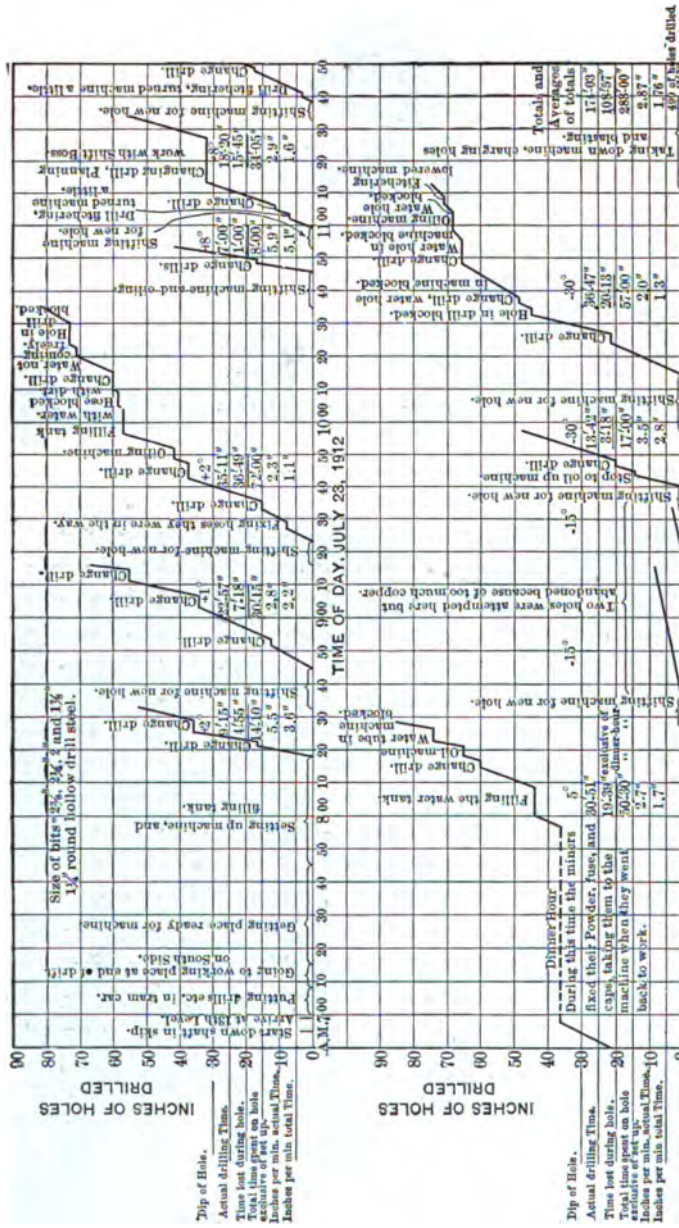

FIG. 13.—DRILLING RECORD FOR ONE SHIFT WITH A NO. 18 INGERSOLL LEYNER DRILL.

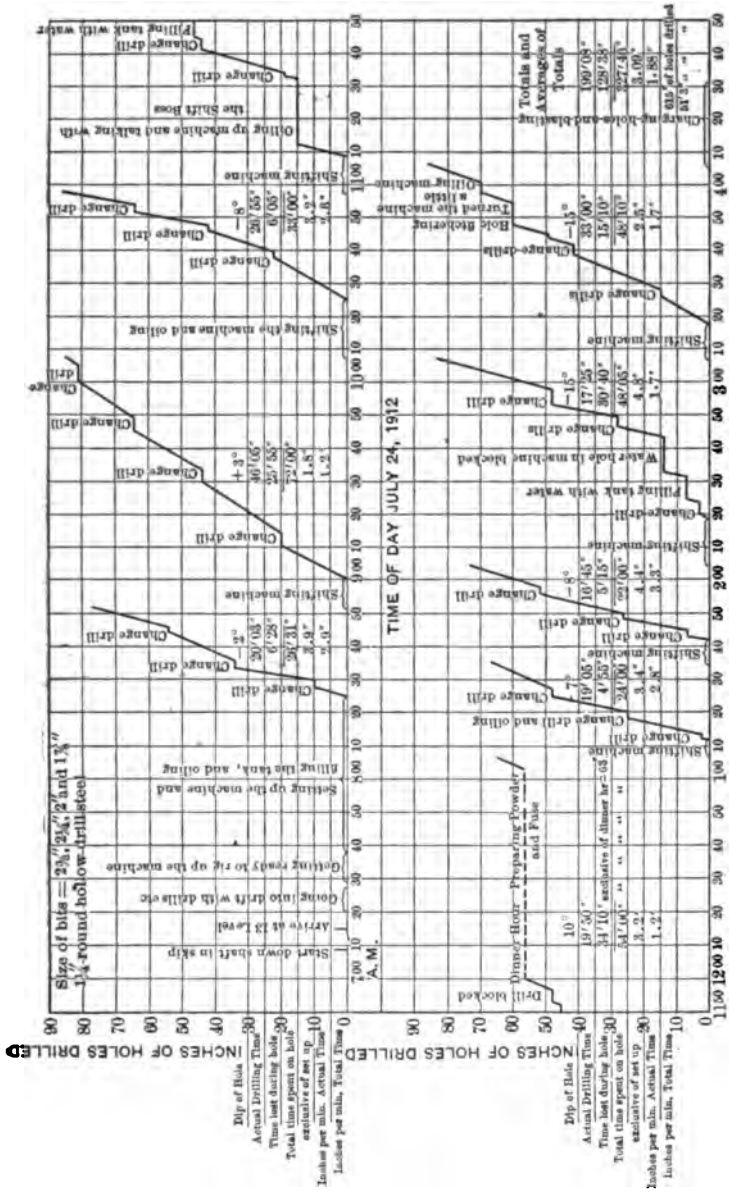

FIG. 14.—DRILLING RECORD FOR ONE SHIFT WITH A NO. 18 INGERSOLL LEYNER DRILL.

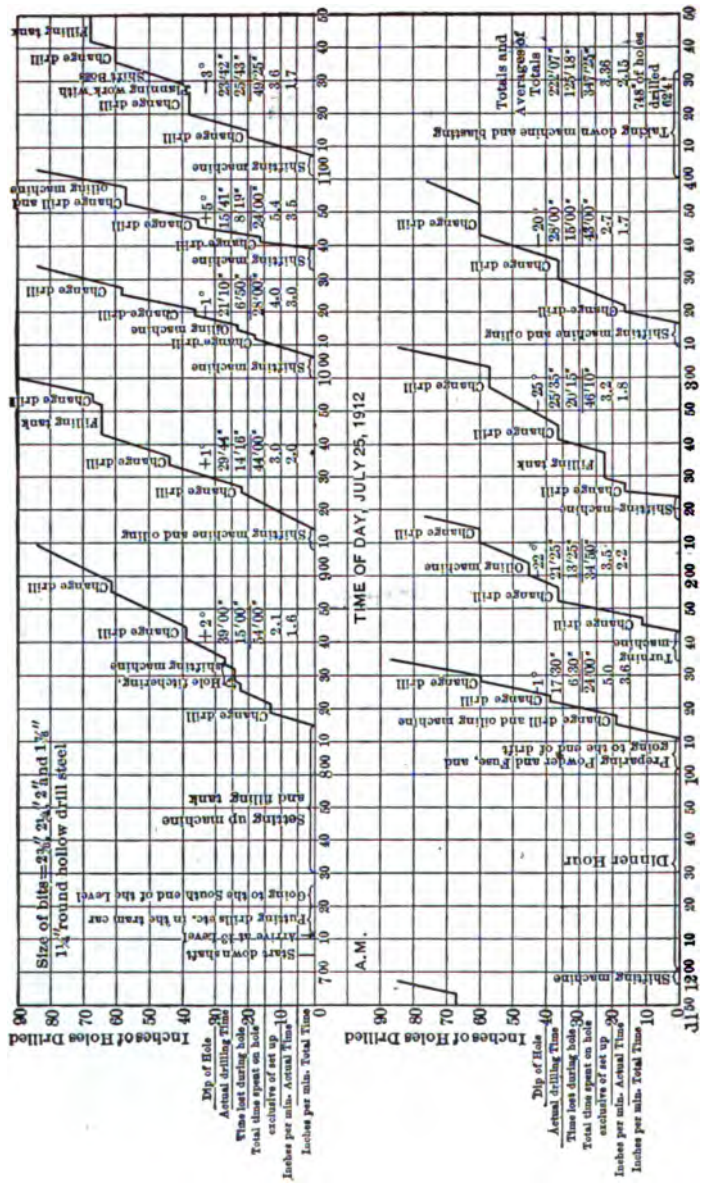

FIG. 15.—DRILLING RECORD FOR ONE SHIFT WITH A NO. 18 INGERSOLL LEYNER DRILL.

In these charts the horizontal lines represent time of performance and the vertical lines distance drilled.

The completeness of the charts will be noted; every minute of the 9-hr. working shift is accounted for.

For instance, Fig. 13 shows that the actual drilling did not begin until 8.18 a.m. The machine then drilled a hole 16 in. deep in 3 min., when it was stopped to change steels, which operation required 1 min., and a further distance was then drilled of 20 in., requiring 3 min., when the machine was again stopped to change steels, 4 min. being consumed in doing so. Drilling was then continued and the hole deepened 15 in. in 3.5 min. The total time consumed in actual drilling was 9.5 min.; total time changing steels, 5 min.; and total depth of hole, 51 in. At this stage the machine was shifted for drilling a new hole.

In the city of Pittsburgh Jackhamers are being employed for driving an 8 by 12 ft. sewer tunnel, 1,200 ft. long. The formation is a hard slate and shale with a decomposed rock top. Single cap timbers are installed and it is necessary to use fore-poling to hold the top. Two drills are installed in the heading, drilling eight 3-ft. holes, with a single steel to the hole. The average drilling speed is 12 in. per minute. Owing to the bad top and the fact that the tunnel passes under two bridge piers and the Pennsylvania Railroad tracks, the shooting is necessarily light; about 2.5 ft. are pulled at each round. Ten feet of completely timbered heading is the advance per day of two shifts.

The following is a record of work done with Jackhamers in the excavation for the Grand Central Station, New York City:

Month.	Hours Drilled.	Total Linear Feet Drilled.	Average Feet Drilled per Hour.
Sept. 22 to 29	60	950	15 5/6
Oct. 1 to 31	255	4,256	16.65
Nov. 1 to 5	48	800	16⅔
	363	6,006	16.5

Cost for repairs (2 pawls, 50c. each), $1, or about 1⅔c. per 100 ft. drilled.
Air pressure, 70 to 90 lb.
Holes drilled, 4 to 10 ft. in hard New York mica schist.

Performance of Jackhamer at the Rockland & Rockport Lime Co., Rockland, Maine:

Total Number Feet.	Total Hours.	Rate per Man per Hour.
350	55	$0.20
488	60	0.22½
372	55	0.22½
240	30	0.22½

Week of May 4 to 10.

Total Number Feet.	Total Hours.	Ft. per Hr.	Rate per Man per Hour.	Cost per Foot, Labor Only.
650	60	10.8	$0.22½	$0.0208
322	45	7.1	0.22½	0.0314
430	50	8.6	0.22½	0.0262
420	50	8.4	0.22½	0.0268
1822				

The average cost per foot being $0.0253.

The diameters of the bits used are as follows:

Length of Steel, Feet.	Diameter of Bit, Inches.
2	2
4	1⅞
6	1¾
8	1⅝
10	1½
12	1⅜

The depth of each hole drilled was 12 ft.

In the lead mines in southeast Missouri they have heretofore drilled the down-holes in the stopes with the large type of two-man drill. Since the Jackhamer has been introduced in these mines the results obtained have been 60 to 80 ft. of 8-ft. holes per man per shift, while with the old system employing two men they were only able to drill 40 to 50 ft. of 8-ft. holes per shift. In many cases where the vein is exceedingly wide they drill 10-ft. holes as easily and readily as an 8-ft. hole can be drilled.

In the Oliver iron mines in Michigan the record is given as 57 stoper drills operated in the year 1912, with a total expense for repairs for the year of $920.23, or $16.14 per machine per year.

The Tin Situation in Bolivia.

BY HOWLAND BANCROFT, DENVER, COLO.

(Butte Meeting, August, 1913.)

THIS article is not presented as a treatise on tin mines and mining in Bolivia. It deals primarily with the tin situation, and but fragmentary information is given regarding individual properties, generalizations being necessary in so short an article. Data on general subjects related to tin mining in Bolivia are necessarily abbreviated and very incomplete. It is believed that the references to be found in this article will help those interested to a more thorough understanding of the general conditions extant in Bolivia.

Bolivia produces from one-fifth to one-fourth of the world's supply of tin. The United States consumes about one-half of the world's supply and produces practically none. Tin ore is not reduced in the United States. These facts are not generally appreciated by American mining men. Many conflicting data have been published regarding tin in Bolivia. The *Transactions* of the Institute contain practically no mention of this subject.[1]

I have made a study of mines and mining conditions in the west coast republics of South America and have been particularly impressed by the possibilities of the Bolivian field.[2]

In consequence the thought seems justified that the present meeting would present a fitting occasion to bring the tin situation in Bolivia to the attention of American mining men.

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[1] Brief mention of tin in Bolivia will be found in A. F. Wendt's very interesting account of The Potosi, Bolivia, Silver District, *Trans.*, xix., 90 to 91 (1890-91).

[2] The possibilities of this field have caused others to write upon the subject, references to several publications appearing in this paper. However, in this article I have tried to present the subject with a view to showing the importance of the Bolivian tin field to the United States, this country being the largest user of tin in the world and up to the present is without a domestic supply. While the following presentation of the subject is in part a repetition of previously published material, it represents nevertheless the result of my own observations, combined, to some extent, with a compilation of data based upon the writings and statements of others.

General Statements.

If the following facts are kept in mind in considering the tin situation in Bolivia the importance of the field to the people of the United States interested in mining will become very apparent.

1. As already stated, Bolivia produces from one-fifth to one-fourth of the world's supply of tin. This production comes from a comparatively few mines, the Bolivian tin industry being in its infancy. Fig. 1 shows the production of tin in Bolivia from 1897 to 1912. During this short period the output has increased from a little over 2,000 metric tons to more than 23,000 metric tons.

2. The United States consumes about one-half of the world's supply of tin, and the domestic production is negligible. Fig. 1 shows the increase in the importation of tin by the United States during the interval between 1897 and 1912. The imports have increased from 25,000 metric tons in 1897 to twice that amount in 1912. This has been largely due to the flourishing condition of the American tin-plate industry.

3. The consumption of tin is steadily increasing, as is also the production, the latter being due largely to the Bolivian supply. This increase in consumption and production has been accompanied by a steady increase in the price of tin, which has risen from 13 c. per pound in 1897 to 50 c. in 1912. To-day, tin is sold for $\frac{1}{19}$ the price of silver. Fig. 2 shows the average monthly price of tin in New York from 1897 to 1912. The increased production, or the world's supply, of tin has been accompanied by a persistent rise in the price.

4. One-half of the world's supply of tin ore is controlled by Great Britain, and is smelted on British territory, the export of this proportion of tin ore being prohibited by reason of the high export tax on tin ore mined in the Malay Straits, British possessions. Fig. 1 shows the world's production of tin from 1897 to 1912. Bolivia's production is also platted for comparison. The production from Bolivia is approximately equivalent to the increase in the world's production.

5. Notwithstanding the consumption of one-half the world's supply of tin by the United States, capital from this country is not invested in tin properties and tin ore is not reduced in the United States. In consequence, by the purchase of one-half the world's supply of metallic tin the United States pays the entire profits resulting from mining and milling of one-half of all the tin ore mined, freight on ore to reduction works, reduction of ore to metallic tin, shipment of metallic tin to the United States, brokers' commissions, etc. Fig. 3 shows the value of the world's production of tin, the value of tin imported into the United States, and the value of the Bolivian tin production from

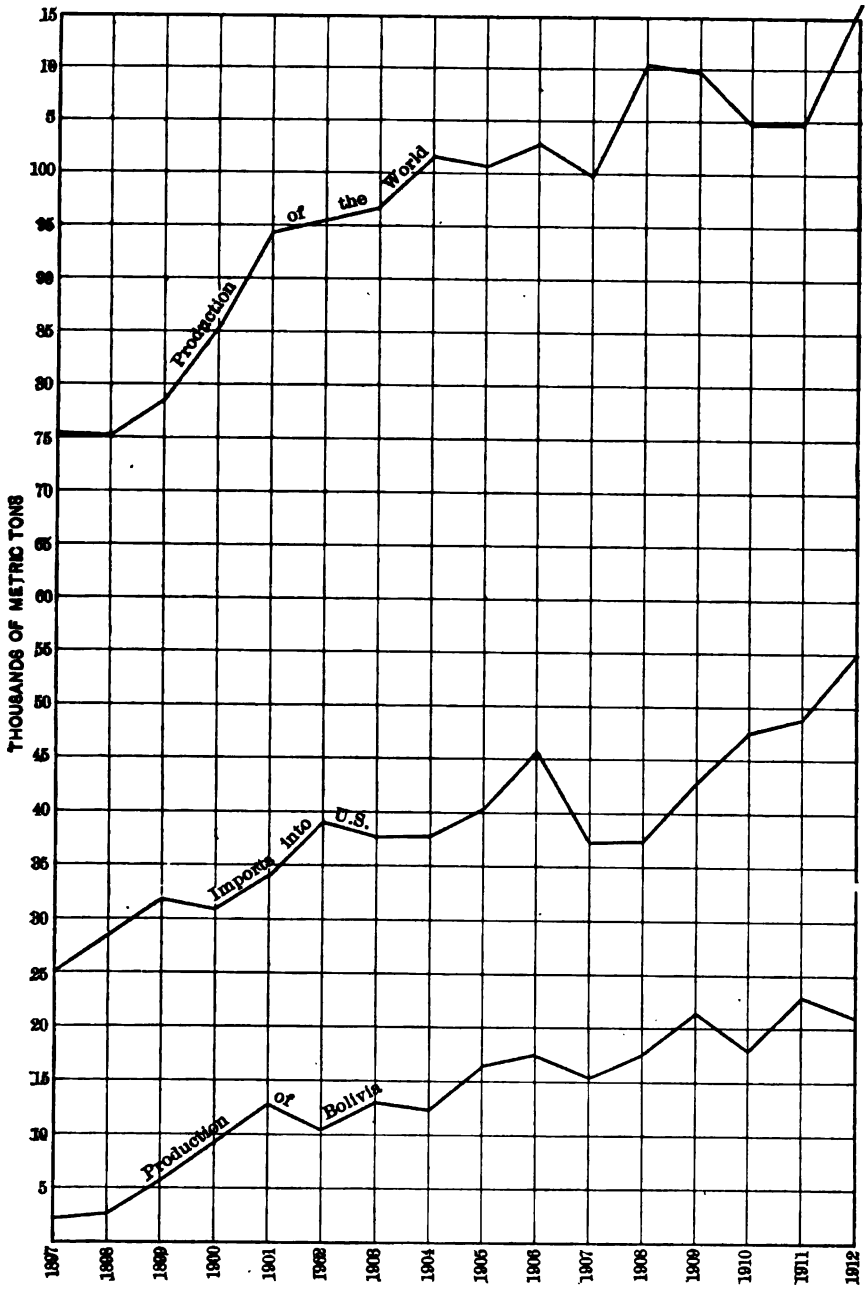

FIG. 1.—WORLD'S PRODUCTION OF TIN; IMPORTS OF TIN INTO THE UNITED STATES; AND PRODUCTION OF BOLIVIAN TIN, FROM 1897 TO 1912.

1897 to 1912. The total output has now reached a figure where the production is worth in round numbers $100,000,000 annually.

6. As above stated, one-half of the world's production of tin ore is not available to purchase by nations other than Great Britain because of the high export duty on tin ore mined in the Malay Straits, British possessions. This leaves for purchase by the United States the tin ore mined in Banka, Billiton, Tongka Harbor, Australia, Africa, and Bolivia. The last named produces as much as all the others mentioned, is by far the most accessible country, and the completion of the Panama Canal will greatly facilitate American operations. The Bolivian mining laws are in many respects superior to those of the United States. The export tax on tin ore is graduated to comply with the market price of tin. For example:

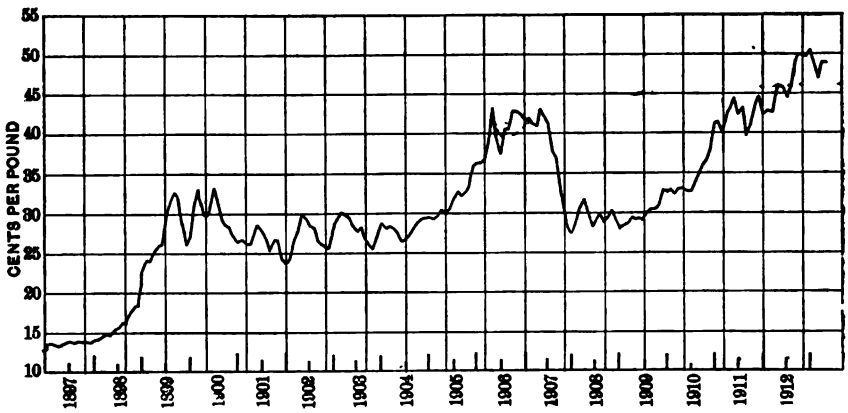

FIG. 2.—AVERAGE MONTHLY PRICE OF TIN IN NEW YORK FROM 1897 TO 1912.

"The duties on tin are graded according to the London quotations for the bars of Straits tin, and rise from Bs. 0.90 for each 100 pounds when tin bars are quoted at £100 to Bs. 3.30 [Bs. 12.50 = £1 = $4.8665 U. S. currency] if the price reaches £200 or more. These rates are for barilla of 60 per cent., but the bars of pure tin pay Bs. 1.80 for each 100 pounds when the London quotation for Straits tin is £100 a ton, and Bs. 4.20 if the price is over £200." [3]

Bolivia to-day ranks first among the tin lode mining countries, producing about four times as much tin from lode mines as Cornwall. In fact, one mine in Bolivia, La Salvadora, is credited with a yearly production equivalent to the total production of Cornwall. As is well known, the largest production of tin in the world comes from the Straits Settlements, this production combined with that from Banka and Billiton contributing two-thirds of the world's production

[3] From a pamphlet entitled *Bolivia*, published by the Bolivian Legation, Washington, D. C. (1912).

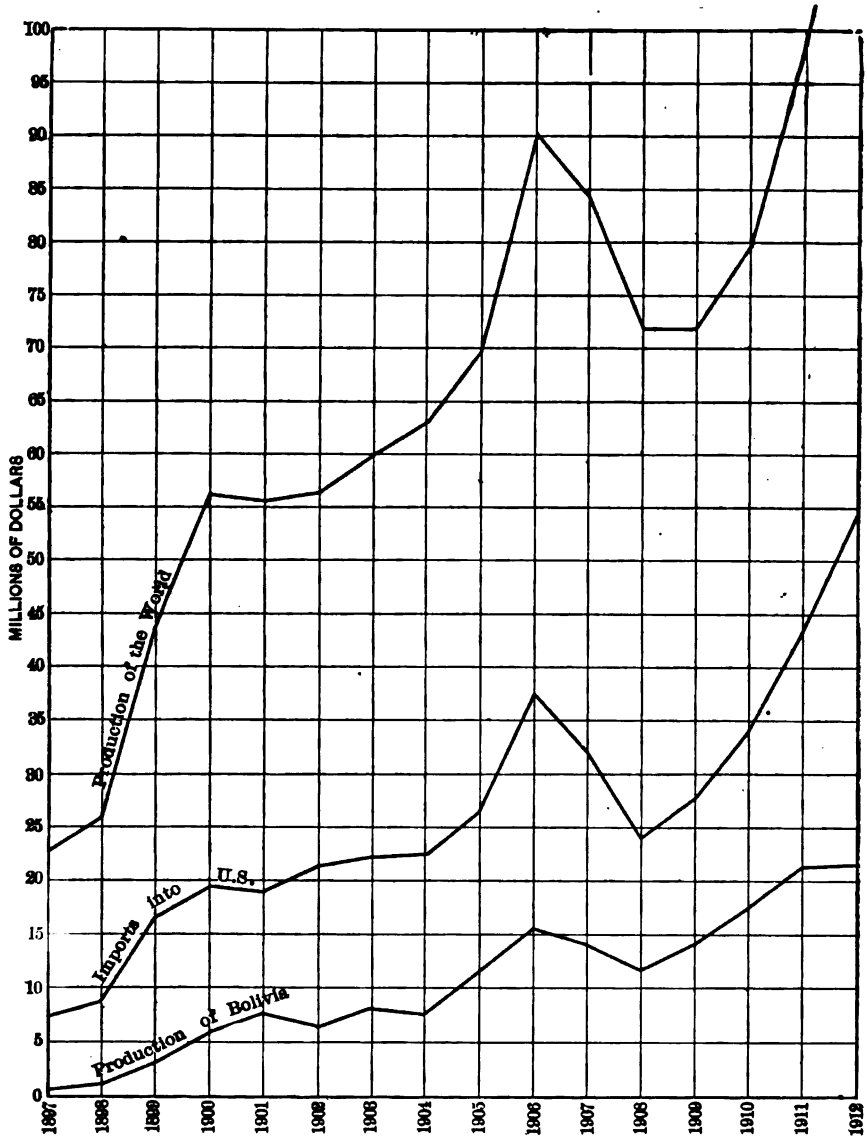

FIG. 3.—VALUE OF THE WORLD'S PRODUCTION OF TIN; VALUE OF THE TIN IMPORTED INTO THE UNITED STATES; VALUE OF THE BOLIVIAN TIN PRODUCTION, FROM 1897 TO 1912.

of tin. Australia, China, and Africa also contribute some tin. Individually, however, they are not at present important factors in the world's supply of this metal. It is to be remembered that all of the tin ore mined in the Straits Settlements, excepting about 3,000 tons produced yearly in the vicinity of Tongka Harbor by Australians, is subject to an export tax of 33⅓ per cent., this tax effectively keeping the tin ore in British hands until it has been smelted. An attempt was made to operate a tin smelter at Bayonne, N. J., about seven years ago. This smelter was never blown in because the export of Straits Settlements ore to other than British possessions was promptly forestalled by the enactment of the English export tax of 33⅓ per cent. Consequently the transports carrying oil from the United States to Asia were compelled to return without a cargo of tin ore from the Straits Settlements, which was contrary to the expectation of the interests concerned in building the tin smelter.

Tin-Producing Area of Bolivia.—It is to be remarked that Bolivia is the only country in South America producing tin ore in quantity, which seems strange in view of the fact that Peru,[4] Chile,[5] and Argentine[6] have many geologic relationships in common with those found in the tin-producing districts of Bolivia.

Tin ores are found in many localities in Bolivia, the principal producing properties being in the Departments of Oruro, Potosi, Cochabamba, and La Paz, the first two named being at present by far the most productive. These Departments cover roughly 100,000 square miles, most of which territory is mountainous, and all of which would be included in the so-called mineral belt of Bolivia.

Between the main ranges of the Andes broad flat pampas extend for many miles, the mean elevation of these being about 12,000 ft. above sea level. These pampas are bounded by north-south trending ranges of mountains, some of which rise to elevations of over 22,000 ft. above sea level, and throughout the mountain districts tin ores are found. In general, it may be said that the tin deposits which are

[4] T. Olaechea, commenting upon tin in Bolivia, says in *Boletin de Minas Industria y Constructiones*, Lima, 1902: "Although the metal referred to [tin] has only been known to exist in the Department of Puno [Peru], it will not be strange if some day, owing to the mineral wealth of the Peruvian soil, tin may be found as a result of new explorations, in conditions where it can be conveniently worked."

[5] A. Götting, *Zeitschrift für praktische Geologie*, June, 1894, pp. 224 to 230, states that cassiterite occurs in Chile in diabase in which are also deposits of cinnabar, siderite, etc. He states that the cassiterite does not appear to be commercial.

[6] H. D. Hoskold, in *Report upon the Mines, Mining, Metallurgy, and Mining Laws, etc., of the Argentine Republic*, Buenos Aires, 1904, states that stream and lode tin have been discovered in Argentine. (Thus far these deposits are not believed to have been proved commercial.—Author.)

being exploited occur between the elevations of 12,000 and 19,000 ft., deposits actually being worked at the latter altitude.

In this connection the following table may be of interest. This table is reproduced from an official publication by the Bolivian Government, *Monografía de la Industria Minera en Bolivia*, by Pedro Aniceto Blanco, La Paz, Bolivia, 1910. Readers are cautioned not to take the information contained in the table too literally; for example, the tin content of the veins is greatly exaggerated. However, it represents a concise, though somewhat inaccurate, summary of a few of the features of some of the principal tin deposits of Bolivia.

Generalized Summary of Features of Some of the Principal Tin Deposits in Bolivia.

Location.	Name of the Mine.	Number of Veins.	Strike.	Dip.	Country Rock.	Per Cent. Tin.	Average Width of Vein.
Huanuni	Catericagua	1	E. & W.	50° S.	Porphyry	11½	1 meter
Huanuni	Barreno	1	N. 75° E.	60° S.	Porphyry	11½	1 meter
Huanuni	San José	1	N. 80° E.	45° S.	Porphyry	11½	1 meter
Huanuni	Dejada	1	E. & W.	50° S.	Porphyry	11½	1 meter
Penny Duncana	Vetilla	1	E. & W.	70° S.	Porphyry	11½	1 meter
Penny Duncan	Vetilla	1	N. 70° E.		Porphyry	11½	1 meter
Huanuni	Various				Porphyry	11½	1 meter
Negro Pabellón	Negro Pabellón	3	N. & S.	Vertical	Slate	9½	0.08 cm. *b*
Morococala		3		Vertical	Slate	10	Variable
Vilacollo	Vilacollo	1	E. & W.	Vertical	Porphyry	8	Variable
Colquiri	Socobón Verde	1		Vertical	Arenaceous shales	5	1.3 meters
Tres Cruses	Sayaquira	1	N. 80° E.	Vertical	Slate	10	1 meter
Oruro	Various				Porphyry	8	Variable
Challa-apacheta	Challa-apacheta	1			Porphyry	7	Variable
Chualla grande	Avicaya	4	N. 45° E.	Vertical	Slate	8	1 meter
Chualla grande	Totoral	4	N. 80° E.	50° N.	Porphyry	8	1 meter
Chuncho	Chuncho	3	N. 80° E.	50° N.	Porphyry	8	1 meter
Uncía	La Salvadora	2	N. 35° E.	N. 80° W.	Porphyry	17	0.8 meter
Uncía	Animas	1	N. 40° E.	74° W.	Porphyry	15	0.8 meter
Uncía	San Miguel	Various	N. 40° E.	70° W.	Porphyry	15	0.8 meter
Llallagua	La Blanca	1	N. 10° E.		Porphyry	15	0.5 meter
Llallagua	San Fermín	1	N. 10° E.		Porphyry	15	0.5 meter
Berenguela	Various		E. & W.		Porphyry	9	Variable
La Paz	Chacaltaya	1			Porphyry and slate	8	Variable
La Paz	Milluni	Various	N. 50° W.		Porphyry and slate	8	1 meter
Chorolque	Santa Bárbara	1	E. & W.	Vertical	Porphyry	9	Variable
Potosí	Various				Porphyry	10	Variable

[a] Penny & Duncan is the name of a company, not a town. They are probably operating at Ventilla.

[b] This is probably a typographical error. The deposits at Negro Pabellón are known to be over 2 ft. wide.

Production.—Prior to 1899 Bolivia was not recognized as an important factor among tin-producing countries. For example, the production from Bolivia in 1883 was only 493 tons. The following year the production dropped to 204 tons, and it was not until 1888 that the annual production exceeded 1,000 tons.[7] From this time until 1898 the production fluctuated under 3,000 tons per annum. Since 1898 the production has been almost continuously increasing until in

[7] *Mineral Industry*, vol. i., p. 450 (1892).

1911 the production for the year reached 23,000 metric tons. See Fig. 1, compiled from data contained in different volumes of *Mineral Industry*. It is interesting to note in *Mineral Industry*, vol. ix., p. 639 (1900), the following statement: "Owing to lack of capital and facilities of transportation the industry is but little developed, and it is stated on good authority that the production could be easily doubled under more favorable conditions." Reference to Fig. 1 will show the 1900 production of Bolivia was nearly doubled in 1906 and nearly trebled in 1911.

Climate.[8]—Several distinct types of climate exist in Bolivia, depending largely upon altitude, though influenced to some extent by latitude. For the convenience of those unfamiliar with Bolivian conditions, the following tables[9] are reproduced, which show the several zones recognized in Bolivia:

Zones.	Altitude. Feet.	Mean Temperature. F.°	Eastern Bolivia Data.		
			Latitude (South).	Temperature. F.°	Rainfall. Inches.
Snow Zone	16,404	34.4			
Puna Brava	15,705	43.5			
Puna	11,857	53.8	10	90.82	31.50
Cabecera de Valle	10,033	59.4	15	86.04	30.79
Valle	8,202	64.2	20	81.61	30.08
Yungas	5,538	69.8	25	76.82	29.37

As all of the tin properties are in the Puna or Puna Brava zones, we are concerned principally with the conditions in the higher regions. Two principal seasons are conspicuous there: the rainy season, which lasts from November until March, and the dry season, from April to October. These two seasons are not marked by any considerable change in temperature. Frequent rains also occur throughout the so-called dry season. In the higher regions of the Puna Brava no rain falls, the precipitation being in the form of hail or snow.

In general, it may be said that the climate of the Puna regions of Bolivia is good, while that of the Puna Brava zone is almost unendurable. Frequent wind storms and the terrific electrical storms that center about the higher mountain peaks of the Andes are exceedingly objectionable. The rare atmosphere extant at these higher altitudes is a serious hindrance to personal comfort and detracts materially from the efficiency of all operations.

[8] For a more complete account of the climatology of Bolivia, readers are referred to a publication of the International Bureau of American Republics, *Bolivia*, pp. 23 to 26 (Washington, D. C., 1904).

[9] *Sinópsis Estadística y Geográfica de la Republica de Bolivia* (La Paz, 1903).

Vegetation.—The Puna and Puna Brava zones are barren of a natural growth of trees and only small shrubs are present. Artificially planted eucalyptus trees flourish at altitudes of 12,000 ft. Because of their rapid growth, these trees would prove a salvation for future generations if planted in large numbers. Certain people have advocated the commercial possibilities of planting large groves of eucalyptus, to be cut when grown and used for railroad ties. Yareta (usually called a fungus), a resinous umbelliferous plant, forms one of the sources of fuel supply. The growth of this plant is slow, however, and as the consumption for use as fuel is high the supply is being rapidly depleted.

Water.—Properties situated near the snow line are peculiarly favored in one respect at least—they have an abundant supply of water. In general, water is scarce in the Puna regions of Bolivia. Nevertheless there are many running streams in Bolivia, and these afford ample water for concentration works as well as power for hydro-electric installations. Although water is not always conveniently near the tin properties, this is not by any means an insurmountable difficulty, for concentration works are built on the banks of the streams, or water is piped to the plants. Electricity is carried by high-power transmission lines from the source of supply and doubtless will play a much more important part in mining operations in Bolivia in the future than it has in the past.

Transportation.—The Departments of Oruro, Potosi, La Paz, and Cochabamba are traversed in places by railroad lines and the completion of railroads now building will greatly facilitate freight shipments. A few of the producers are well situated as regards transportation facilities, but the majority of the deposits are some distance from railroad shipping points, and the tin ore, in the form of barilla, has to be packed from these properties by llamas or mules to the nearest railroad point. These deposits are located in the outlying districts from 5 to 60 miles or more from any kind of a settlement. They represent, however, localities which are practically virgin, and are frequently the only mining locations within the immediate district. Tin ore from Potosi is smelted in water-jacketed furnaces and the impure tin resulting is exported in bars. This procedure saves freight, but yields a very impure product.

Freight rates on railroads in Bolivia are invariably high and are a serious drawback to greater profits resulting from mining operations. The completion of the new Arica á La Paz railroad may at least have the effect of reducing terminal rates.

Without rail connection or wagon roads all material has to be

transported on llamas or mules, and the operators located in out of the way places use this means for transportation of materials. Llamas will carry about 75 lb. and mules will carry from 200 to 300 lb. The former cost $5 and the latter from $30 to $50, and the upkeep of the mules costs much more than for the llamas. During very dry weather this method of transportation is seriously interfered with.

Power.—Steam, generated by burning taquia, yareta, coal, or oil, is used for power in some of the plants. Because of the scarcity of fuel and consequently its high cost, various other means of generating power have been attempted. Anthracite gas-producer engines have been introduced and these have proved economical. According to J. B. Minchin,[10] the working cost of these engines is about 4c. per horse-power hour. Pelton water wheels and petroleum motors are also used. Electricity has been generated by gas engines and hydroelectric installations, this latter method of generating power being the real future for mining and milling operations in Bolivia. A successful electric furnace for smelting tin ores on the ground is not improbable, and the near future may see a revolution in the manner of handling the tin product of Bolivia.

Fuel.—The scarcity of fuel forms one of the chief difficulties in Bolivian mining. Australian coal costs about $12.50 per ton at coast points. However, railroad freight rates from the ports to the interior are exorbitant and have the effect of raising the cost of coal by from $20 to $30 per ton by the time it reaches Bolivian railroad points. This makes the use of coal as a means for generating power almost prohibitive. Yareta, a resinous plant, and taquia (llama dung) are both used for fuel. The former costs about $5 per metric ton and contains one-third the calorific power of coal. Taquia costs about $5 per ton and contains one-sixth the calorific power of coal.

If the coals reported in the vicinity of Lake Titicaca develop into commercial deposits this local supply will greatly ameliorate the trouble arising from the present scarcity of fuel. The Peruvian oil fields represent a nearby source of supply of oil fuels. At La Fundicion, the smelter site of the Cerro de Pasco mines, located at an elevation of 14,100 ft., oil is used for fuel under the majority of the boilers, and it has there been found to be generally more efficient than coal. It is to be remembered that the efficiency of any power requiring oxygen in its generation should be figured about 3 per cent. less for every rise of 1,000 ft. above sea level.

Labor.—Native Indian labor of both sexes is used in the mines and

[10] Tin Mining in Bolivia, *Engineering and Mining Journal*, vol. lxxxi., No. 17, p. 810 (Apr. 28, 1906).

in the mills in Bolivia, with superintendents usually from the country whose citizens control the property. The wage scale varies from 40c. to $2.40 per day for Indian labor, the average probably being about $1 per day. The Indians are natural born miners and if the feast days were not of such frequent occurrence these laborers would prove very satisfactory. There is a scarcity of labor, this fact possibly accounting for the presence of many Indian women in the mines and mills of Bolivia. Mark R. Lamb [11] points out the necessity of care of future generations of Bolivian Indians, predicting that without this care the mines will have no native laborers within a few years.

Mining and Metallurgy.

In general, the natural climatic conditions and high elevations that prevail in the Bolivian tin-mining districts do not tend to facilitate mining. The efficiency of human labor is reduced, as is also the efficiency of any power requiring oxygen in its generation. New mines do not appear to be sought and consequently are not found, regardless of the fact that the tin-mining country is barren of vegetation and consequently easy to prospect below the snow line.

Mining methods are almost universally crude, but few exceptions to this generalization existing in Bolivia. Up-to-date practice is almost unknown and the present efficiency of mining in Bolivian tin properties is consequently far from its possible maximum. By the installation of modern mining methods, the production and profits of many of the properties could be greatly increased and important mines developed from present day prospects. As an example of this the following statement may be of interest: [12]

[A mine in Oruro district]—"run at less than half capacity making £12,000 a year as profit where £100,000 might be made with modern methods and a capital expenditure of only £4,000."

Milling of Bolivian tin ore is more modernized than the mining methods there extant, and several complete up-to-date concentrating plants have been established at the more progressive properties.[13] In general, however, the tin ores are concentrated by hand, which consists of crushing by *quimbaletes* (rocking stones), and washing in moss-bottomed launders. Some of the more pretentious hand concentrating plants are equipped with *quimbaletes*, hand jigs, screens for

[11] Bolivian Tin Mining, *Engineering and Mining Journal*, vol. xciv., No. 26, p. 273 (Aug. 10, 1912).

[12] Present Position of Bolivian Tin Mines (extract from *Mining Journal*) *Mining World*, vol. xxx., No. 18, p. 829 (May 1, 1909).

[13] For a description and flow sheet of one of these the reader is referred to the *Mining Magazine*, vol. vi., No. 3, p. 208 (London, Mar., 1912).

sizing, *bubles*, *bateas*, and canals. The hand concentrated tin ores are washed and rewashed until a 55 to 60 per cent. tin product is obtained.

At Potosi water-jacketed furnaces smelt the tin concentrates, a product containing many impurities resulting.

Generalizations regarding working costs are of little value and specific costs at one property are apt to be quite at variance with those at another. Nevertheless, the following fragmentary data on costs are given with the hope that they may prove somewhat serviceable. Assuredly some of the data will show comparative costs of production, and the probable profits resulting from operations under existing conditions. The total costs of mining, milling, freight, commissions, etc., are given by different operators as ranging from 14 c. to 38 c. per pound on European markets. Naturally so many diverse factors enter into the computation of costs at different locations that there result wide differences in total costs, and consequently in profits. One small company producing 40.57 tons of barilla per month from ore averaging 3 per cent. of tin reported a working cost at the mine of 17 c. per pound. Another company on a production of 2,000 tons showed a working cost at the mine of 7.5 c. per pound, on ore, four-fifths of which averaged 69 per cent. of tin.[14] Transportation by llamas is estimated[15] to cost 81 c. per ton-mile. J. B. Minchin[16] states the cost of transportation by llamas and donkeys at $1.25 per ton-mile. Wire rope tramways installed at several properties have reduced the cost of carrying ores to 12 c. per ton-mile. The fuel problem has already been discussed. At present the most effective power generator seems to be the gas-producer engine. An 80-h.p. engine generates from 50 to 55 h.p. at 12,300 ft. above sea level and the consumption of anthracite is stated to be 1.54 lb.[17] per horsepower hour.[18] It is to be remembered that gas-producer engines are working very successfully on the poorest grade of coals. Consequently, if a local supply of coal could be obtained, no matter how poor in quality, it could be utilized in gas-producer engines to generate power. This would mean a large saving on the high cost

[14] Extract, British Consular Report, *Engineering and Mining Journal*, vol. lxxxviii., No. 26, p. 1284 (Dec. 25, 1909).

[15] Present position of Bolivian Tin Mines (extract from *Mining Journal*), *Mining World*, vol. xxx., No. 18, p. 829 (May 1, 1909).

[16] Notes on Tin Mining in Bolivia. *Engineering and Mining Journal*, vol. lxxv., No. 1, p. 31 (Jan. 3, 1903).

[17] *Loc. cit.*

[18] With coal at $40 per ton this would make the cost of the coal alone 3 c. per horsepower hour.

of imported anthracite. It is thought, as already stated, that electrical power represents the logical future power for Bolivian mining enterprises. Native labor is paid from 40 c. to $2.40 per day, although much of the work is done by contract and has been found to be productive of larger results.

The following detailed cost sheets may be of interest.[19]

1. Mine with Hydraulic Motive Power.

	Cost Per Ton. Bolivianos.	
Mining	8.60	
Conveying from mines to mill	0.12	
Salaries of chief miner and assistants	1.00	
Mine material, dynamite, etc	2.55	
Total mining		12.27
Sorting, hand picking and handling	1.23	
Milling	0.95	
Weighing, drying, bagging, etc	0.20	
Material and repairs	1.44	
Assaying	0.04	
Millman and assistants	1.24	
Total milling		5.10
Manager, assistant, office, etc	0.78	
Mine taxes	0.14	
Total general expense		0.92
Total cost		18.29

18.29 Bolivianos equal $7.12 U. S. currency.

2. Mine with no Hydraulic Power. Fuel, kerosene or taquia.

	Cost Per Ton. Bolivianos.	
Mining	4.13	
Materials, steel, dynamite, etc	1.26	
Walling, filling	1.60	
Total mining		6.99
Fuel	1.00	
Milling	2.46	
Drying barilla, bagging, etc	0.80	
Materials, repairs, etc	0.50	
Assaying	0.10	
Total milling		4.86
Carpenter, mason, blacksmith, foreman	1.20	
Accountant, storekeeper	0.40	
Road repairing, legal services	0.30	
Various incidentals	0.40	
Total general expense		2.30
Manager and assistant	1.60	
Chief miner	0.25	
Various	0.15	
Total administration		2.00
Total cost		16.15

16.15 bolivianos equal $6.29 U. S. currency.

Freight, duties, commissions and insurance on concentrates from mine No. 1 to Europe	$88.22
Freight, duties, commissions and insurance on concentrates from mine No. 2 to Europe	$70.20

[19] These costs sheets are reproduced from an article by Miltiades Armas, Mining and Milling in Bolivia, *Engineering and Mining Journal*, vol. xcii., No. 9, p. 413 (Aug. 26, 1911).

Geology.[20]

So much has been written about the geology of Bolivian tin deposits that a repetition seems needless in a paper of this sort. Suffice to say that the tin deposits occur in five distinct types, which are, in the order of their commercial importance, as follows: 1. Deposits wholly within granular intrusive rocks, quartz-porphyry being the most conspicuous. 2. Deposits cutting both granular intrusive rocks of type 1 and metamorphic sedimentary rocks of type 3. 3. Deposits wholly within metamorphic sedimentary rocks, such as shales, slates, various schists, quartzites, etc. 4. Pegmatitic tin veins. 5. Stream tin placers.

The granular intrusive rocks of the quartz-porphyry type are believed to be of Mesozoic age. This quartz-porphyry, so conspicuous in many of the deposits in Bolivia, may represent a differentiation phase of granitic magmas. I am inclined to think that the andesites and trachytes referred to by Stelzner[21] have no genetic connection with the tin deposits of Bolivia, my views on this subject substantiating conclusions reached by Rumbold. (See reference to Rumbold, *Economic Geology*, Vol. 4, 1909.)

The metamorphic sedimentary shales, schists, quartzites, etc., are thought to be of middle Palæozoic age. Several writers have classed these rocks as Silurian, while others have termed them Devonian. Probably more detailed investigations will show several divisions of the Palæozoic represented by the metamorphic sedimentary series.

Ore Deposits.

In general, the deposits of tin ore in Bolivia occur in quartz veins in granular intrusive rocks which have invaded the metamorphic series of shales, quartz-mica and other schists, quartzites, limestones, dolomites, etc. In places the metamorphic series also contain mineralized veins where contiguous to granular intrusive masses. The mineralization in the latter class of deposits has been found to be disseminated, spreading out over a larger area of rocks and occupying small cracks, joints, and fissures in the metamorphic rock. This is particularly true of the type of deposit which occurs in both intrusive rock and metamorphic sedimentaries, the mineralization becoming

[20] For a more complete description of the geology and ore deposits of Bolivian tin properties, readers are referred to the Origin of Bolivian Tin Deposits, by W. R. Rumbold, *Economic Geology*, vol. iv., No. 4, p. 321 (June–July, 1909); and Genesis of Bolivian Tin Deposits, by Miltiades Armas, *Engineering and Mining Journal*, vol. xcii., No. 7, p. 311 (Aug. 12, 1911).

[21] Zinnerzlagerstätten von Bolivia. Digest, *Mineralogical Magazine and Journal of the Mineralogical Society*, London, vol. x., No. 47, p. 261 (Nov., 1893).

scattered as the deposit is worked out of the boundaries of the granular intrusive. While I did not examine any pegmatitic tin deposits in Bolivia, specimens shown me from deposits which I was invited to investigate left little doubt of the pegmatitic nature of the tin veins. Tin placer deposits are not being extensively worked in Bolivia and I did not examine any, although several were brought to my attention. As transportation facilities increase and the richer lode mines become exhausted, attention will doubtless be directed toward working placers known to exist in Bolivian tin fields.[22]

In width the lodes vary from a few inches up to several feet, the average being from 2 to 5 ft. wide. The length of the deposits along the strike of the veins varies considerably, some veins being traceable over several thousand feet while others appear to extend only a few hundred feet. The general idea that Bolivian tin deposits are superficial is erroneous, for veins have been proved to be commercially mineralized by tin ores to a depth of over 1,500 ft. below the apex. One ore shoot seen by me had been proved to a depth of 900 ft., to have a length along the strike of 360 ft., and to have an average width of 6 ft. of 15 per cent. tin ore. From several places in this ore shoot 60 per cent. tin ore was mined. These values are exceedingly above the average, however, the general run of mine ore in the tin properties in Bolivia being between 3 and 8 per cent. Armas[23] has stated that the average for all of the producing tin mines in Bolivia is 3 per cent.

In the lode mines, cassiterite forms the chief ore mineral. This is associated in places with highly argentiferous tetrahedrite, pyrite, chalcopyrite, and small quantities of the sulphides of tin, lead, zinc, antimony, molybdenum, and bismuth. Wolframite is also associated with the tin ores in places. In general, the silver values are associated with tetrahedrite, which seems to form a more conspicuous part of the vein filling when pyrite is present. The chief gangue mineral is quartz. With this is associated tourmaline, siderite, and brecciated country rock recemented by siliceous solutions. Fluorite has been reported[24] as occurring in some of the tin veins. Limonite is a conspicuous feature of the upper portions of the veins.

[22] For a brief description of a tin placer deposit near Chorolque, readers are referred to an article by Malcolm Roberts, Chorolque Tin Mines and Alluvial Deposits, Bolivia, *Transactions of the Institution of Mining and Metallurgy*, vol. ix., pp. 372 to 375, London (1900–1901).

[23] Armas, Miltiades, "Mining and Milling in Bolivia," *Engineering and Mining Journal*, vol. xcii., No. 9, p. 412 (Aug. 26, 1911).

[24] Spencer, J. L., A description of the mineral specimens brought from Bolivia by Sir W. Martin Conway. See Conway, Sir W. Martin, *Climbing and Exploration in the Bolivian Andes*, p. 345 (Harper and Brothers, New York and London, 1901).

Bolivian lode deposits are believed to be the result of the after effects of igneous intrusion. The invariable association of granular intrusive rocks (of which quartz-porphyry is by far the most common) with or contiguous to the lodes, combined with the mineral associations in the veins, justifies the conclusion that the intrusive magmas and solutions accompanying or following the intrusions represent the source of the tin deposits, which were undoubtedly formed under great heat and pressure.

Future of Bolivian Tin.

Tin deposits are proved in Bolivia, and many mines doubtless await discovery. The principal difficulties which hinder developments are lack of adequate transportation facilities, lack of fuel, lack of sufficient labor, and more than all of these combined, the tremendous price at which productive properties are held. This last difficulty may be obviated by acquiring mere prospects, or investigating new ground only, which procedure has not been followed up to the present time. These difficulties, however, have not kept the production of the country from increasing from 2,000 tons in 1897 to over 23,000 tons in 1911. The Bolivian tin industry is still in its infancy, and offers large rewards [25] to those who are shrewd enough to see its

[25] D. H. Bradley, Jr., in an article entitled Mining in Bolivia, published in the *Mining Magazine*, vol. xi., No. 1 (New York, Jan., 1905), expresses the belief that Bolivian mines offer opportunities for investments paying 25 per cent. premiums within three or four years.

Mark R. Lamb, in an article entitled Bolivian Tin Mining, published in the *Engineering and Mining Journal*, vol. xciv., No. 6, p, 271 (Aug. 10, 1912), says that "A prospecting expedition would stand an excellent chance of success if properly equipped. . . . The opportunities for capital are numerous because there is little capital in the country."

Maurice Frochot, in L'étain en Bolivie, *Annales des Mines*, 9th ser., vol. xix., p. 186 (1901), asserts that "Tin veins close at hand are still untouched."

G. Preumont, writing on The Bolivian Tin Mining Industries and Railways, London *Mining Journal*, vol. lxxxiii. (1908), remarks, "Most of the mines are comparatively yet in virgin ground."

John Penberthy, of Redruth, Cornwall, is reported as having said to a correspondent of the London *Mining Journal* in 1906: "In time, however, the Bolivian output will become a very serious consideration, if not a preponderating factor in the world's supply. That time will come when railway facilities from the mines to both the Atlantic and Pacific seaboards make mining not only of high grade, but of the immense low grade deposits, at present untouched, a payable proposition."

An editorial in the *Engineering and Mining Journal*, Nov. 26, 1898, says: "Certainly it appears somewhat astonishing that America, after establishing a flourishing tin-plate industry, does not look for the raw material to its original sources."

W. R. Rumbold concludes his account of The Origin of Bolivian Tin Deposits, already referred to, with the following statement: "We all hope that the proposed new railway system now under construction by an American company will cheapen the cost of production, that the present mine owners will reduce their prices, and that foreign capital, modern machinery, and up-to-date managers will make what the writer [W. R. Rumbold] believes to be the richest mineral country in the world what it ought to be."

possibilities and who successfully overcome the difficulties consequent to mining in Bolivia, where I believe the tin situation deserves the attention of American mining men and capitalists.

LITERATURE.

For an extensive bibliography of the "Geology and Mineralogy of Tin," the readers are referred to the recent publication of that name prepared by Frank L. Hess and Eva Hess and issued by the Smithsonian Institution of Washington, D. C. In this volume are over 1,700 references to publications on tin, of which about 80 deal with some phase of tin in Bolivia. This publication, No. 1987 of the Smithsonian issues, will prove of immense value to any one interested in the literature of tin.

Attention is invited to the following publications on tin in Bolivia, some of these appearing in the bibliography above mentioned, which is somewhat amplified, containing brief mention of the contents of the publications listed.

1. Minchin, J. B.: Tin Mines of Bolivia, *Engineering and Mining Journal*, vol. li., No. 20, p. 586 (May 16, 1891).

2. Minchin, J. B.: The Mineral Resources of Bolivia, *Engineering*, vol. li., p. 453 (Apr. 17, 1891).

3. Stelzner, A. W.: Zinnerzlagerstätten von Bolivia, *Zeitschrift der deutschen geologischen Gesellschaft*, vol. xliv., p. 531 (1892).

4. Frochot, Maurice: L'étain en Bolivie, *Annales des Mines*, vol. xix. (1901).

5. Rumbold, W. R.: Origin of the Bolivian Tin Deposits, *Economic Geology*, vol. iv., No. 4, pp. 321 to 364 (June–July, 1909).

6. Blanco, Pedro Aniceto: *Monografía de la Industria Minera en Bolivia*, La Paz (1910).

7. Armas, Miltiades: Mining and Milling in Bolivia, *Engineering and Mining Journal*, vol. xcii, No. 9, p. 412 (Aug. 26, 1911).

8. Lamb, Mark R.: Bolivian Tin Mining; *Engineering and Mining Journal*, vol. xciv., No. 6, p. 271 (Aug. 10, 1912).

9. Bancroft, Howland: Mining on the West Coast of South America, *Mining and Scientific Press*, vol. cvi., No. 4, pp. 175 to 176 (Jan. 25, 1913).

The Substitution of Air for Water in Diamond Drilling.

BY RALPH WILCOX, MIAMI, ARIZ.

(Butte Meeting, August, 1913.)

THE diamond drilling of certain characters of unstable rock formation, as, for example, the copper-bearing schists of the Miami district in Arizona, is rendered most difficult by what is known as "caving" of the sides of the drill holes. As this caving is very greatly increased by the action of flowing water, it occurred to me, after experiencing a great deal of difficulty with caving in diamond-drill holes which I was drilling for the Miami Copper Co., that if I could substitute air for water, much of the caving—with resultant difficulties, such as decreased speed of sinking and increased consumption of diamonds—would be overcome.

After some experimenting, I found that it was perfectly practicable to substitute air for the water; forcing the air down the hollow rotating rods in the same manner as is done with water, the air passing out around the bit, which is thus cooled, and ascending on the outside of the rods to the collar of the hole, carrying the borings in just as satisfactory a manner as had been done by the water, but without the accompanying destruction or caving of the sides of the drill holes such as had been so generally experienced with the water. When water was encountered in the drilling this water was forced up to the collar just as it would have been had water been used in the drilling, but with the correspondingly decreased flow there was relatively less washing or caving of the sides of the drill holes.

The only change necessary in the arrangement of the drill is that a cross instead of a tee is used at the collar of the hole. A tightly woven jute bag is attached to one arm of the cross, for collecting and filtering the sample; and when drilling in dry ground a jet of water is attached to the opposite arm of the cross, to collect the dust and wash down the sample into the bag. The rods rotate through a stuffing box attached to the upper opening of the cross. The distention of the jute bag by the issuing air indicates the operating conditions as sufficiently as did the flow of water when using water in the drill hole.

The small percentage of core recovery rendered it necessary to

rely almost entirely upon sludge samples. Great care was taken in recovering these samples, which were all weighed to detect the presence of caving. A winze has lately been completed which was sunk on one of the drill holes for the purpose of checking the sludge samples; the samples from the winze and the samples obtained in the drill hole checked accurately. .

The experience at Miami in the use of air has extended over six months and has shown that the refrigerating action of the air in expanding around the bit is effective in cooling the bit and the diamond consumption has not increased; it has not been found necessary to decrease the speed of rotation of the bit. The difficulties of operating in the friable copper-bearing schist have been decreased fully 75 per cent. by the use of air, and the sample weights and the results of sampling checked perfectly. There is no indication as to whether one would be limited as to depth in dry holes; our greatest required depth was about 800 ft.

Tests as to amount of air used showed a maximum consumption of 82 cu. ft. of free air per minute, while the least with which operating was practicable was 23 cu. ft. of free air per minute. The average used during the several tests was 47 cu. ft. of free air per minute, at 75 lb. pressure. A No. 1 Excelsior meter was used for these measurements, the meter being tested before and after making the measurements.

Apart from affording much relief in diamond drilling of friable rock such as above referred to,—probably the worst in which diamond drilling has ever been done,—there arises the possibility of drilling in regions where a scarcity of water would otherwise prevent operating. Drilling in these regions could be accomplished by the use of a portable compressor driven by a gasoline engine, which is another not inconsiderable advantage which the use of air instead of water in diamond drilling offers.

The Discovery and Opening of a New Phosphate Field in the United States.

BY CHARLES COLCOCK JONES, LOS ANGELES, CAL.

(Butte Meeting, August, 1913.)

In the winter of 1902, while occupying the position of mining and examining engineer for the Mountain Copper Co., Ltd., of Keswick, Cal., I had occasion to discuss with the General Manager, among other things, the question of the enormous waste of sulphur in smelting cupriferous pyrite ore, and to cast about for means to utilize this waste or to render the fumes innocuous to vegetation. The decreasing rate of production of California farms, the consequent need of fertilizers, and the opportunity thus offered to utilize large quantities of sulphuric acid in the manufacture of superphosphates naturally brought the subject to the practical and commercial side of phosphates.

I was informed that the company had made efforts to find phosphates in the West, without success. I advised as a start placing inquiries for phosphate deposits on the Pacific coast in the current mining journals. This was done, with the result that in January, 1903, from an advertisement in the *Mining and Scientific Press* we got track of a deposit in Rich county, Utah, through T. J. Wilson, of the Southern Pacific railroad in San Francisco.

As soon as snow in the mountains would permit, in May, 1903, I undertook an examination of the property, with the result of recognizing the importance of the find, in spite of its lack of facilities for transportation at a commercial figure.

On June 15, 1903, I concluded my report by saying, "This may be one of the world's deposits of phosphates." On Nov. 17, 1903, I concluded my report for the season in the following words: "I have now no hesitation in saying that I believe the Carboniferous formation so largely developed in the Wasatch mountains of Utah and the mountains of the Great Basin will prove to be one of the greatest storehouses of phosphoric acid in the world."

Early History.

In the summer of 1897 R. A. Pidcock, a mail carrier of Ogden, Utah, while on a prospecting trip camped on "12 mile," a branch of

Woodruff creek, Rich county, Utah, in T. 8 N., R. 5 E., Salt Lake Meridian. Finding some old workings in a soft black formation and believing from returns on certain samples that he had a gold mine, he and others took up a number of claims in August and September, 1897. A mining excitement of the usual kind ensued and a large area of country was staked.

O. A. Kennedy, of Ogden, visited the camp Oct. 30, 1897, and later sent samples of the ore (phosphate rock) to the U. S. Geological Survey and to Prof. J. F. Kemp, of Columbia College, requesting analysis, but no returns were ever received.

The principal claims were incorporated into the Alice Mining Co., which during 1898 and 1899 did considerable development work without finding any more gold or silver. Finally a large sample was sent to Thomas Price & Sons, of San Francisco, for analysis and treatment. The returns showed no gold or silver, but revealed the fact that the material was phosphate rock, the following analysis being made by that firm:

	Per Cent.
Phosphoric acid	32.44
Iron and alumina oxides	1.52
Iron sulphide	1.72
Calcium oxide	43.02
Magnesium	0.83
Carbon dioxide	0.85
Sulphur trioxide	2.24
Fluorine	1.36
Organic matter	5.83

32.44 per cent. phosphoric acid corresponds to 70.18 per cent. tricalcic or bone phosphate.

One by one the stockholders dropped out until Frank Howe, a mail carrier of Ogden and one of the original locators, was the sole owner of the claims. His persistent belief in the value of the property was rewarded by the sale of it in 1903 to the agents of the Mountain Copper Co., Ltd., through T. J. Wilson, of San Francisco.

Woodruff Creek Deposits, Rich County, Utah.

In May, 1903, accompanied by Frank Howe, the owner, I examined the deposits of phosphate rock on Woodruff creek, 11 miles west of the town of Woodruff in the Bear River valley. Evanston, Wyo., on the Union Pacific, 24 miles south of Woodruff, is the nearest railroad station.

The phosphate formation extends for about 2.5 miles through Sections 16 and 9 and into Sections 21 and 4, T. 8 N., R. 5 E., Salt Lake Meridian, and dips from nearly vertical to 45° W. No geological

maps of this region were available except in the atlas of the *Geological Exploration of the Fortieth Parallel*, by Clarence King, and this portion was mapped as the Vermilion Creek formation, of Eocene age. The formation as exposed showed dipping to the west a great thickness of quartzite with underlying limestones, in which latter the phosphate occurred as a component bed.

The unconformity between the phosphate-bearing formation and the Vermilion Creek was apparent, and previous to the finding of distinctive fossils I was inclined to place the phosphate in the Silurian, from its general similarity to the deposits in the Bala limestones of Wales as figured by Dr. Penrose in *Bulletin* No. 65 of the U. S. Geological Survey.

An examination of a series of fossils from the apparently underlying limestones by Dr. G. H. Girty proved the formation to be Upper Carboniferous. On Aug. 28, 1903, the U. S. Geological Survey wrote me as follows:

DEPARTMENT OF THE INTERIOR,
UNITED STATES GEOLOGICAL SURVEY,
WASHINGTON, D. C.

August 28, 1903.

Mr. Charles Colcock Jones,
Box 393, Ogden, Utah.

Sir:

Your letter with accompanying specimens has been referred to Dr. G. H. Girty of this Survey, who submits the following report:

"The fossils sent in by Mr. Jones prove to belong in the late upper Carboniferous and not, as was supposed, in the Silurian. The following species are identified: *Stenopora sp.*, *Productus sp.*, *Spiriferina pulchra*, and *Seminula subtilita*. The horizon is that called upper Coal Measures in the section of the Wasatch Mountains established by the U. S. Geological Survey of the 40th parallel. It occurs above the Weber quartzite and below the Permo-Carboniferous as defined by that organization."

As the fossils prove to be interesting and rather better preserved than usual, you would confer an obligation on us by furnishing the locality where they were found, and, if possible, by sending more material from the same beds, for which franks are enclosed.

Very respectfully,
(Signed) H. C. RIZER,
Acting Director.

Here was a confusion in the very beginning. The error in mapping the Vermilion was understandable by reason of the immense territory sought to be covered in a short space of time by the Survey of the 40th Parallel and the comparatively small and isolated area involved, but the position of the fossils in a horizon above the Weber quartzite, but really lying in present position under it, was not clearly worked out until the following year. This will be gone into later in discussing the relations of several of the deposits.

The valley of Bear river follows the line between Utah and Wyoming, with the river flowing north, and has an elevation at Evanston, Wyo., of 6,755 ft. A. T. and at Woodruff, Utah, of 6,450 ft. A. T. It is an old Eocene basin, and going west from Woodruff up Woodruff creek the foothills are made up of Tertiary strata, lying flat or dipping very slightly, composed of soft red and white sandstones and conglomerates designated by the Survey as the Vermilion Creek formation.

On a general north and south line through Townships 7 and 8 N., R. 5 E., S. L. M., the Tertiary strata are found to lie uncomformably on the Upper Carboniferous, which latter in the process of mountain building has experienced an overturning of the strata. Fig. 1 is a general section.

In addition to work done by Howe and associates in Section 16 an incline pit 20 ft. in depth had been sunk long previous on what was thought to be either an oil shale or coal blossom in Section 4.

From the imperfect openings available at the time the following is a general section of the phosphate formation, beginning at east side and going west:

Limestone, comparatively pure.
Cherty limestone and layers of black chert.
Principal bed of hard black oölitic phosphate, 4 ft., 66 to 78 per cent. bone phosphate.
30 ft. of intercalated black limestones and carbonaceous phosphatic shale, 19 to 45 per cent. bone phosphate.
300 ft. heavy bedded crystalline limestones.
Quartite.

The higher-grade phosphate occurs as brownish black oölitic stratified rock with a highly fetid odor, the oölitic structure being very regular and with only a minimum amount of interstitial matter. The rock weathers readily to individual grains, except a few layers of hard oölitic phosphate.

Further search failed to find the phosphate formation immediately north or south of the 2.5 miles spoken of on Woodruff creek and trips were taken in several directions.

A careful study of Hayden's Report of 1871 and the text of the 40th Parallel Survey by Clarence King caused me to examine the Ogden and Echo Cañon regions. The remarkable Z contortion in the Lower Carboniferous limestones spoken of by these writers consists of a series of black limestones and shales 16 ft. or more in thickness bearing from 2 to 24 per cent. lime phosphate, with a 1-in. black oölitic band carrying 48 per cent. lime phosphate.

A like occurrence of this same horizon will also be found in a cañon several miles south of Logan, Utah. There is nothing commercially

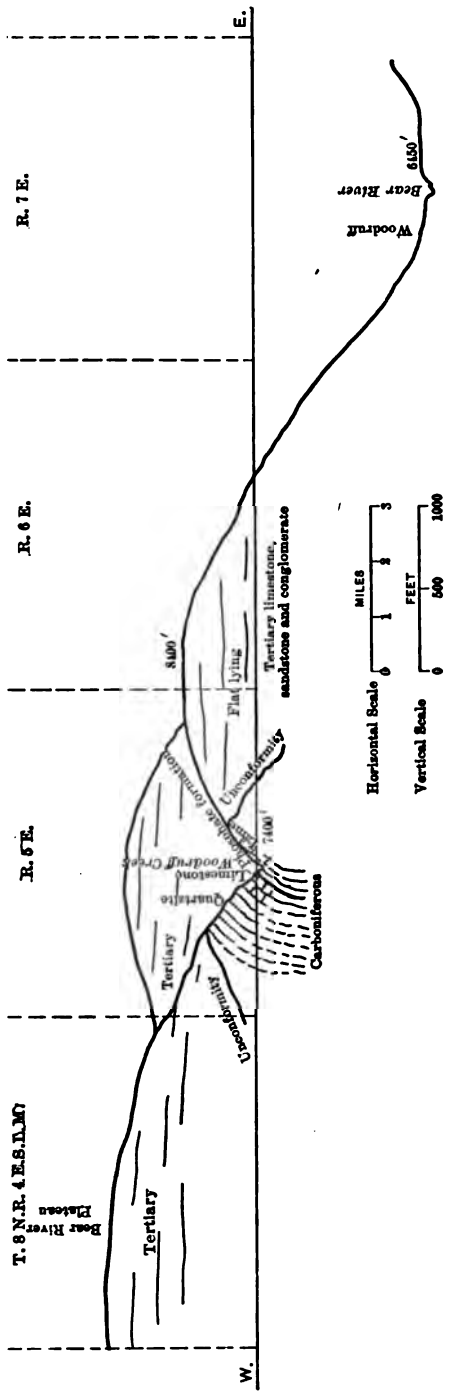

FIG. 1.—PHOSPHATE DEPOSITS AT WOODRUFF CREEK, UTAH.

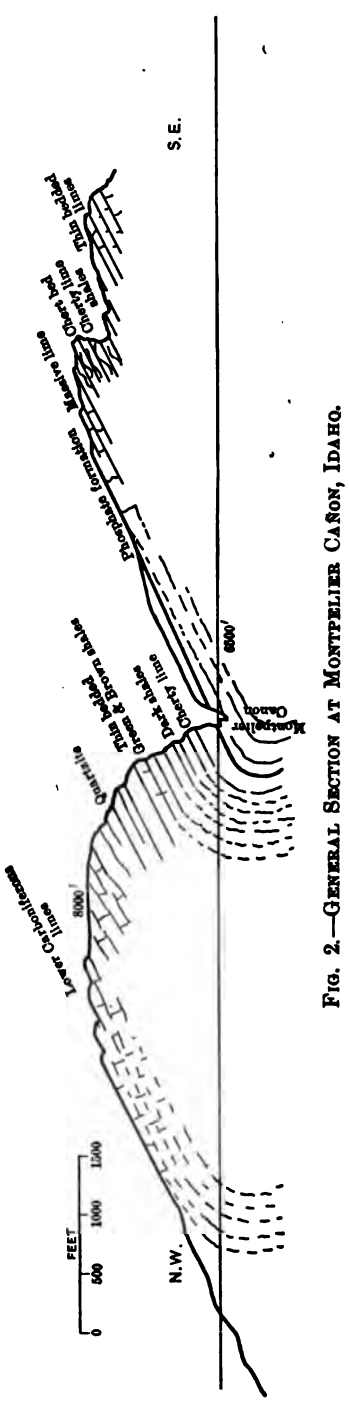

FIG. 2.—GENERAL SECTION AT MONTPELIER CAÑON, IDAHO.

valuable about it, but it has great significance as being the earliest occurrence of oölitic bedded phosphate I have been able to find from the Cambrian to the Carboniferous, and may have had some influence, by the degradation of large areas, in forming the later deposits, or if not that, bearing to an extent on the subject of the origin of the deposits as governed by the bathrymetric conditions prevailing at the time of the Upper and Lower Carboniferous limestones, separated by the great thickness of the Weber quartzite.

In August, 1903, I learned of work being done near the town of Montpelier, Bear Lake county, Idaho, for coal, and upon visiting the locality I found an incline shaft 250 ft. deep had been sunk by T. L. Glenn and associates in the geologically lower black shaly limestone strata of the phosphate formation. Summarizing the matter from a large amount of exploration covering this and the succeeding year, the following are the conditions and conclusions: The phosphate formation is exposed in Montpelier cañon 3 miles east of the town for a distance of 1 mile south in Sections 6 and 7, T. 13 S., R. 45 E., B. M., and 2 miles north in Sections 30 and 31, T. 12 S., R. 45 E., B. M.

The strata composing the entire mountain just north and south of the cañon occupy an inverted position, with the geologically upper rich bed of phosphate lying at the bottom, and the mountain at this point represents a part of an immense reverse fold. The strata strike in a general north and south direction, with a dip to the west of from 20° to 40°. A feature which has already confused geologists and engineers is the occurrence at Montpelier cañon of a thick bed of cherty limestone both above and below the phosphate formation, bearing the same set of fossils. In its normal position the most remarkable bed of chert and cherty limestone lies geologically above the phosphate and serves as a means of solving some of the problems encountered in searching for and placing the phosphate formation, entirely aside from the co-ordinating of like phosphate members of beds at widely separated points. This peculiarity is emphasized by the absence of any distinctive quartzite or indurated sandstone bed comparable to the Weber quartzite, the place of which seems to be taken by the cherty lime and shale beds at the mouth of the cañon, although a mile further north the quartzite is again in evidence 1,000 ft. thick.

The following replies to my letters to the Director of the U. S. Geological Survey are of interest and show the ready assistance afforded by the Survey in pursuing my investigations:

DEPARTMENT OF THE INTERIOR,
UNITED STATES GEOLOGICAL SURVEY,
WASHINGTON, D. C.

Nov. 6, 1903.

Mr. Charles Colcock Jones,
Ogden, Utah.

Dear Sir:

Your letter of October 12 and accompanying specimens were referred to Mr. C. H. Girty of this Survey, who makes the following report:

"The fossils from near Montpelier, Idaho (lot 2), sent in by Mr. Jones are unquestionably Upper Carboniferous; and while it is perhaps hazardous in the present state of our knowledge to go beyond this statement, I believe that their horizon is quite well up toward the Permian. It is perhaps not very far from that of the fossils from Woodruff Creek, though the two faunas have little in common. The bodies shown in lot 3 are in my opinion not fossils, but concretions or segregations of some sort."

We thank you for the additional fossils from Woodruff Creek, and would be glad to obtain some more of the material from Montpelier, which is of considerable interest. In case you have any to spare, some franks are enclosed herewith.

Very respectfully,
(Signed) CHAS. D. WALCOTT,
Director.

DEPARTMENT OF THE INTERIOR,
UNITED STATES GEOLOGICAL SURVEY,
WASHINGTON, D. C.

May 27th, 1904.

Mr. Charles Colcock Jones,
Montpelier, Idaho.

Dear Sir:

Your letter of May 20th and accompanying fossils have been received, and referred to Mr. G. H. Girty, who submits the following report upon them:

"The fossils from Idaho sent by Mr. C. C. Jones unfortunately are new, or little known forms, so that it is possible in most cases only to point out the species to which they are allied, rather than to make a precise identification. The following types have been discriminated:

Lot No. 1.

Chonetes, near C. Geinitzanus Waagen and C. permianus Shumard.
Productus nevadensis Meek?
Omphalotrochus? obtusispira Shumard?

Lot No. 2.

Chonetes, near C. Geinitzanus Waagen and C. permianus Shumard.
Productus n. sp., near P. aagardi Toula.
Pugnas n. sp. near P. kayseri Tschernyschew.
Squamularia n. sp.
Schizodus sp. indet.
Omphalotrochus? obtusispira?
Omphalotrochus? sp., near the foregoing.

"The two lots have essentially the same fauna, and the horizon is without much doubt high in the Upper Carboniferous—probably not Permian, though not far below it.

"The specimen of Lot No. 1 marked 'A' is a *Productus* of the *punctatus* type. It is so imperfect that it can not be identified with certainty, but other better preserved exam-

ples from the same place indicate that its characters are more like Meek's species of the same group, *P. nevadensis*.

"The specimen resembling calcified wood, from Lot No. 2, is probably inorganic—a fibrous, or rather columnar, form of calcite. Calcified wood is rare, and this specimen has not the woody structure.

"Flints and cherts have in many cases been shown to be derived from the siliceous skeletons of sponges, partly in their organic shapes, and partly dissolved and redeposited. The first hypothesis therefore, would be that the cherts mentioned by Mr. Jones have a similar origin, even if no spicules can be detected. Cherts are, however, sometimes quite secondary in their origin, and result from comparatively recent replacements.

"It would be hazardous to venture any definite opinion about the bathymetric conditions prevailing at the time these fossils existed. It is probable that the conditions were not those of a shore deposit nor, on the other hand, of the deep sea. I should judge that the waters were rather shallow or of moderate depth.

"It is asked that you will convey to Mr. Jones an expression of thanks for this collection of fossils, which was sent in response to a request originating with me."

Very respectfully,

(Signed) H. C. RIZER,
Acting Director.

DEPARTMENT OF THE INTERIOR,
UNITED STATES GEOLOGICAL SURVEY,
WASHINGTON, D. C.

August 23rd, 1904.

Mr. Charles Colcock Jones,
5th East Hotel,
Salt Lake City, Utah.

Sir:

The specimens accompanying your letter of August 5th were referred to Mr. G. H. Girty, who reports as follows:

"A fossil bearing rock in Lot 1 contains several specimens of Chonetes, probably relative to C. Geinitzianus and C. Permianus. They probably indicate the Permian or Permo-Carboniferous age of the rocks in which they occur. The few impressions of fossils in Lot 2 can not be determined. It seems probable that the phosphatic nodules and grains (oölitic) are segregations, contemporaneous with the material in which they are enclosed, and not a later enrichment of the rock."

Very respectfully,

(Signed) H. C. RIZER,
Chief Clerk.

The fossils identified in letter of May 27 are all from the underlying and overlying cherty limestones and intercalated lime beds of the phosphate formation.

The fossil-bearing rock mentioned in Lot 1 of letter of Aug. 23, 1904, comes from the top of the series in the chert member.

At Montpelier cañon the phosphate formation occupies a position as a component series of strata in the general lime measures forming the Upper Carboniferous lying above the Weber quartzite. Fig. 2 is a general cross-section at this point, and Figs. 3, 4, and 5 are views showing the character of the formation.

At the bottom geologically it consists of a series of black phos-

FIG. 3.—FACE OF TUNNEL SOUTH OF MONTPELIER CREEK, SHOWING BLOCKY PHOSPHATE AT BOTTOM.

FIG. 4.—PHOSPHATIC LIMES AND SHALES, MOUTH OF MONTPELIER CAÑON, SHOWING LIME CONCRETION.

FIG. 5.—OUTCROP OF 6-FT. BED OF PHOSPHATE, MONTPELIER, IDAHO.

phatic mud beds, black or brown phosphatic shaly limes, lime concretions, and more or less pure beds of limestone, about 60 ft. in thickness, containing from 10 to 53 per cent. bone phosphate of lime, the amount of phosphoric acid in any one bed seeming to be subject to rapid changes in a short distance. A large part of this measure will undoubtedly be found capable of concentration when needed. Sharply divided from this lower member by a band 20 in. thick of hard black limestone, at places very full of *omphalotrochus* shell casts and other fossils, comes the top and commercially workable layer of phosphate rock, 5 ft. 6 in. in thickness. This bed is made up of two members, a lower (geologically) shaly member averaging 2 ft. in thickness, separated by a thin shaly mud seam, exactly similar to a slate parting in a coal bed, from the upper, harder, more blocky phosphate of an average thickness of 3 ft. The whole bed can be classed generally as a black oölitic phosphate very much of the appearance of the bedded black phosphate of Tennessee, and averages 70 per cent. bone phosphate. The fossils are from the intercalated limestones in the phosphate formation. In only one instance have I found a shell directly in the phosphate bed proper and that at Cokeville, Wyo.

From developments at the Hot Springs on Bear lake, 15 miles south, and Thomas fork, 15 miles east, and at Cokeville, 20 miles southeast of the Montpelier deposit, the workable bed described above has held its character and relative position with a definiteness similar to a coal bed over such an area, especially as regards the parting. In other parts of the field its changes are only of such a nature as would be expected in a coal bed of a like area. I draw the comparison at this point to emphasize certain ideas I shall advance later as to its mode of formation and origin.

The next determination of the bed was made by me in the summer of 1904 near Rich's Hot Springs in Section 13, T. 15 S., R. 44 E., B. M. Fig. 6 is a general cross-section at this point.

Again we find the strata in an inverted position, a point I will take up after the description of several further deposits. As will be seen from the detailed section, the workable bed is slightly different from that at Montpelier and the richest bed yet found in the field occurs as a separate one, 18 to 22 in. in width, composed of hard black oölitic phosphate with a layer of curious saucer or disk shaped concretions, all high-grade phosphate.

The formation has been traced through Sections 13, 12 and 1, T. 15 S., R. 45 E., and Sections 36 and 25, T. 14 S., R. 45 E., Boise Meridian, outcropping nearly north and south, with a dip to the west

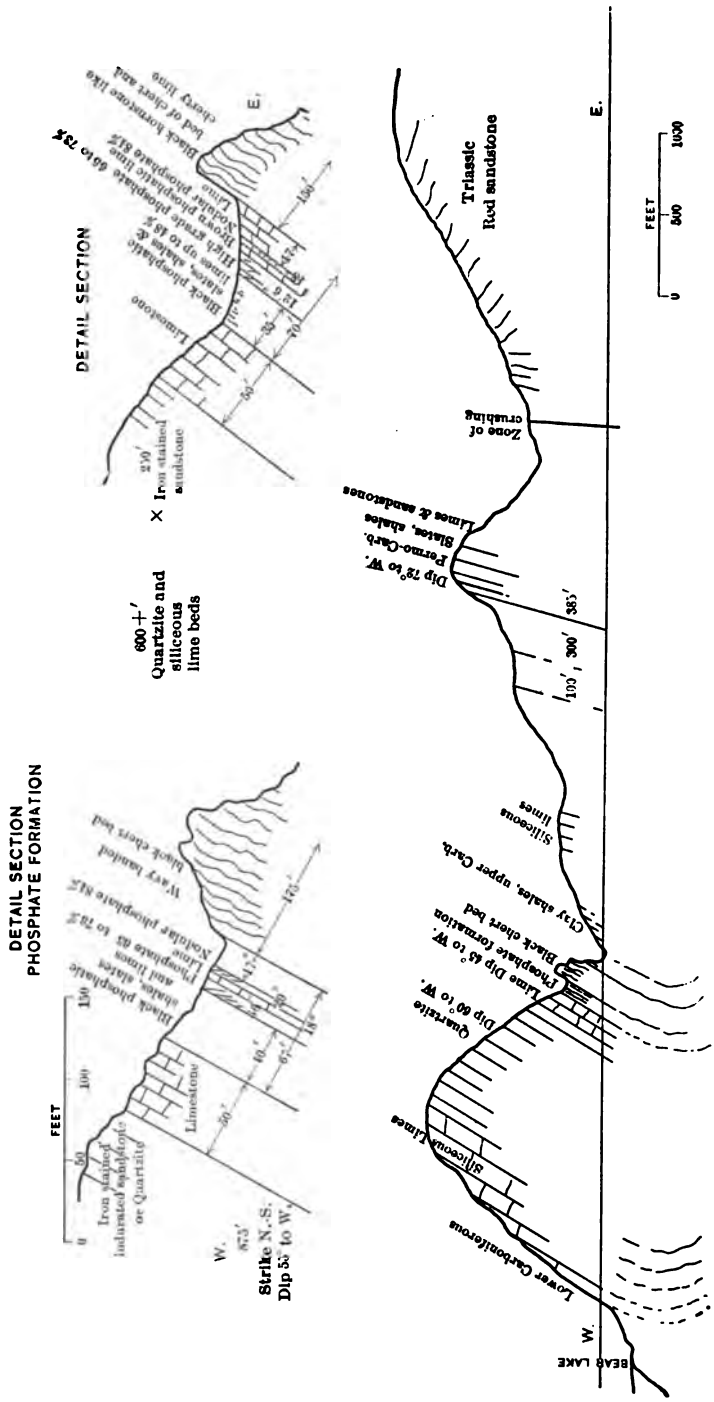

FIG. 6.—SECTION AT RICH'S HOT SPRINGS, IDAHO.

of 45° to 70°. The black chert bed forms a prominent feature, standing up like a dike for 3 miles, Fig. 7.

At the north end in Section 25 the following appears to be the section, beginning at the bottom, geologically, on the east side:

		Bone Phosphate. Per Cent.
Cherty lime bed	100 + ft.	.....
Lime	9 in.	.....
Nodular phosphate	2 in.	.....
Lime	15 ft.	.....
Concretionary oölitic phosphate	15 in.	78.00
Lime with thin bands of oölitic phosphate	4 ft. 3 in.	.....
Gray oölitic phosphate	4 ft. 6 in.	62.40
Gray oölitic phosphate	3 ft. 6 in.	72.80
Parting shale	15 in.	.....
Oölitic phosphate	2 ft. 6 in.	59.25
Black carbonaceous shales	8 + ft.	50.75

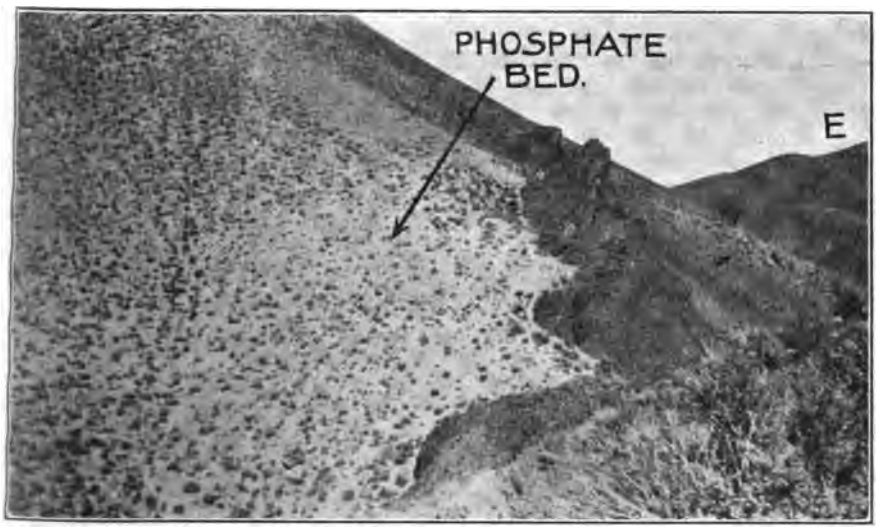

FIG. 7.—CHERT LEDGE AT RICH'S HOT SPRINGS, IDAHO.

This is the first exposure south of Montpelier cañon, and while differing somewhat from either the Montpelier section or the Hot Springs section, the relative position of the workable bed at the top of the series is the same.

In November, 1903, Frank Howe discovered the phosphate formation north of Croyden Station in Section 4, T. 4 N., R. 3 E., S. L. M. Upon examination I found that it corresponded in position with the Woodruff Creek and Montpelier deposits. The geology of this section is well defined by the Survey of the 40th Parallel. At the point

of discovery the formation strikes N. 70° E. and stands vertical or dipping to the northeast. The openings showed a considerable width of material running from 20 to 56 per cent. bone phosphate with intercalated lime ledges. In the spring of 1904 I discovered in Section 34, T. 4 N., R. 3 E., S.L.M., the south extension of the formation 1 mile south of the Union Pacific railway and followed it between the two points at the Weber river and on Dry creek.

The following is a section in N. E. ¼ of Section 34, T. 4 N., R. 3 E., S.L.M., beginning at bottom and rising in the geological scale; strike N. 50° E.; dip 45° SE.:

Weber quartzite	5,000 ft.; King.	
Cherty lime	220 ft.	
Phosphate opening	7 ft. 4 in.	Average 45 per cent. bone phosphate.
Covered with lime débris	40 ft.	
Yellow phosphatic limes	38 ft.	
Cherty limestones	11 ft.	
Covered with lime débris	92 ft.	
Hard cherty lime	61 ft.	
Thin-bedded shaly limes with thin bands of hard siliceous phosphate	23 ft.	
Hard black phosphate containing nodules	2 ft. 3 in.	
Hard gray and black cherty limestone with calcite nodules	32 ft.	
Heavy-bedded gray limestones.		

The phosphate formation in the cañon south of this is covered by the later formation unconformably and its next appearance is just east of Salt Lake City, north of Fort Douglas. It had been prospected here as elsewhere for coal. The strike is NW–SE. and the dip 45° to the northeast. It shows about 100 ft. of phosphatic limes, and cherty beds with a few thin oölitic bands, but apparently no workable bed of commercial value.

Western Wyoming and Eastern Utah.

In the summer of 1904 I was directed to some workings that had been prospected for coal a number of years previous in the north half of Section 7, T. 26 N., R. 119 W. of 6th P.M. in the Sublette range of mountains of Wyoming, just east of the Idaho line, on Thomas fork of Bear river. From this I determined the phosphate formation, running nearly north and south, dipping from vertical to 65° to the east through Sections 18, 7, and 6, T. 26 N., R. 119 W. 6th P.M. Fig. 8 is a typical section at the line between Sections 6 and 7, T. 26 N., R. 119 W. 6th P.M.; elevation 6,600 ft. A. T.

The east bed is the commercially workable one, and the similarity

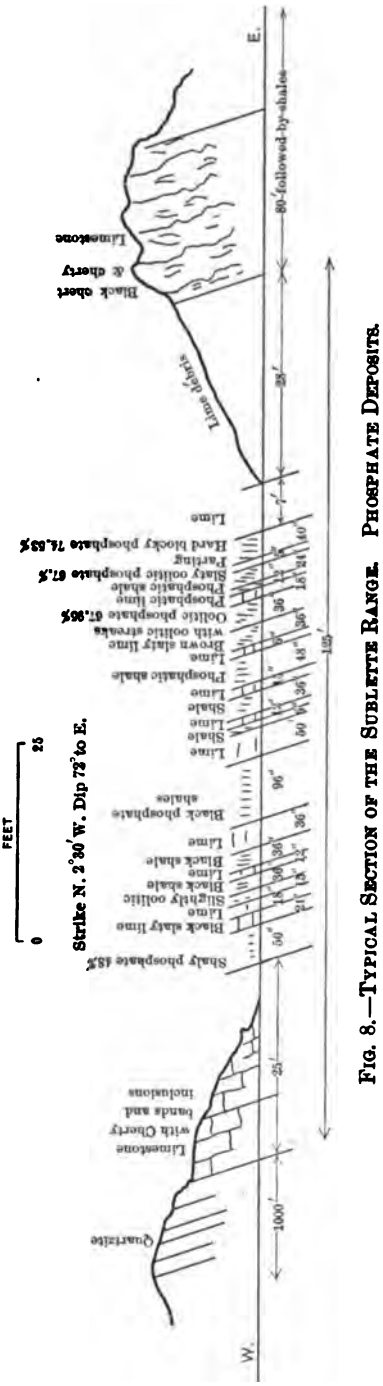

FIG. 8.—TYPICAL SECTION OF THE SUBLETTE RANGE. PHOSPHATE DEPOSITS.

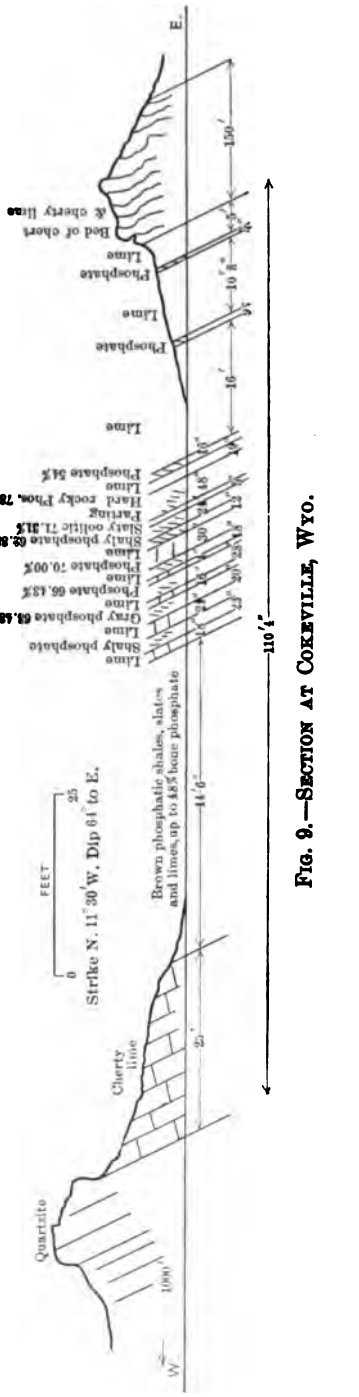

FIG. 9.—SECTION AT COKEVILLE, WYO.

to the Montpelier deposits is marked, even to the shale or mud partings separating the two members of the bed. The heavy black cherty bed lying above the phosphate formation is easily correlated with the same bed at Rich's Hot Springs and at Montpelier cañon, and the succession of strata from this in ascending order geologically is as perfect as at the latter place. The working out of the geology in this section and later south of it convinces me that the lowest bed at Montpelier in its present position was really geologically the top one. Analyses are shown on the cross-section.

In November, 1904, finding the Carboniferous to outcrop in two sharp peaks, 1 mile east of Cokeville, Wyo., in Section 4, T. 25 N., R. 119 W. 6th P. M., on patented land of John W. Stoner, I entered into negotiations with him to examine and acquire the lands in the spring when the snow was off. The result of the examination showed the quartzite, about 1,000 ft. thick, to form the ridge in the form of a close fold. The phosphate formation follows to the east and dips under the valley of Smith's fork of Bear river in a broad syncline, showing exposures of the rocks from the Carboniferous to the Cretaceous, with the phosphate formation again exposed, 8 miles east of Cokeville in Sublette cañon, dipping to the west.

Later investigations showed the continuation of the latter exposure to the south on Rock creek, but so far without commercial beds being opened.

Fig. 9 is a typical section at Cokeville, and the photographs, Figs. 10, 11, and 12, show the physical appearance of the bed.

In July, 1905, I discovered a small exposure of the Carboniferous in the low hills 1.5 miles east of Sage Station, Wyo., on the Oregon Short Line railway.

The occurrence here is on the west side of a gentle anticline, the west side of which has been abruptly faulted in the making of the Bear River valley. Compared with other exposures the phosphate formation is thin. At the base is the Weber quartzite, forming the backbone of the ridge; overlying this is a series of thin, bedded limestones about 125 ft. thick. Then comes the phosphate formation, not to exceed 30 ft. thick, covered by the characteristic cherty limestone, the whole dipping about 25° to west and striking north and south through Sections 2, 11 and 15, T. 21 N., R. 120 W. 6th P. M.

So far as developed there seems to be a workable bed from 7 to 10 ft. thick, showing a top member of 4 ft. of hard, jaspery red and green oily looking rock averaging 74 per cent. bone phosphate separated by a 5-in. shaly parting from 6 ft. of soft gray oölitic rock running as high as 80 per cent. bone phosphate.

FIG. 10.—PHOSPHATE WORKINGS NEAR COKEVILLE, WYO.

FIG. 11.—SURFACE WORKINGS AT COKEVILLE, WYO.

Lower Member. Parting. Upper Member.

FIG. 12.—OUTCROP OF PHOSPHATE BED NEAR COKEVILLE, WYO.

The continuation of this formation was located by the Bradley interests in the Bear hills east of Bear river in Utah in Sections 5, 8, 17, and 19, T. 11 N., R. 8 E., S. L. M., and by myself in Sections 7, 18, and 19, T. 11 N., R. 8 E.

The Bear River fault has brought up the Carboniferous formation in a very steep mountain in which the formation is exposed by faulting and erosion in both an anticline and a syncline showing the phosphate formation in several parallel lines.

A section at Brazier cañon in Section 18, T. 11 N., R. 8 E., S. L. M., is shown in Fig. 13.

At Bennington, Idaho, in the NE. ¼ of Section 14, T. 12 S., R. 44 E., a disconnected and faulted portion of the phosphate formation is shown. A tunnel at this point, run as a coal prospect, has crosscut the formation 45 ft., striking N. and S. and dipping to the west. The material runs from 25 to 50 per cent. bone phosphate. Further work will be necessary to determine its position and relation to the other deposits.

In the west half of Section 21, T. 14 S., R. 43 E., B. M., 2 miles west of the town of Bloomington, Idaho, a phosphate formation about 75 ft. thick has been opened by a tunnel prospecting for coal. It consists of beds of very light weight brownish porous material devoid of oölitic structure or casts. In value it runs from 12 to 45 per cent. bone phosphate, with only a few feet of the latter. The correlation of this with the other deposits is also a matter for further investigation, although it appears to be in the Upper Carboniferous. The strike is N. 30° W., dip 45° to SW.

Thirty miles north of Montpelier I have found the phosphate formation in a cañon north of Soda Springs, Idaho, and at a number of points between the two places locations have since been made by various parties. A casual examination of these shows the general conditions as already described.

From a great number of analyses, the following are the general characteristics of the rocks:

Moisture rarely exceeds 1 per cent. and averages about 0.4 per cent., obviating any drying of the rock, which disintegrates readily by weathering without absorbing moisture to any appreciable extent. Insoluble runs from 5 to 7 per cent.; iron and alumina, below 3 per cent.

Regular shipments of lime phosphate from 63 to 75 per cent. and higher can be made by separating special beds up to several feet in thickness. The specific gravity is from 2.85 to 2.98. There is considerable organic matter, which will be a slight detriment in making superphosphate.

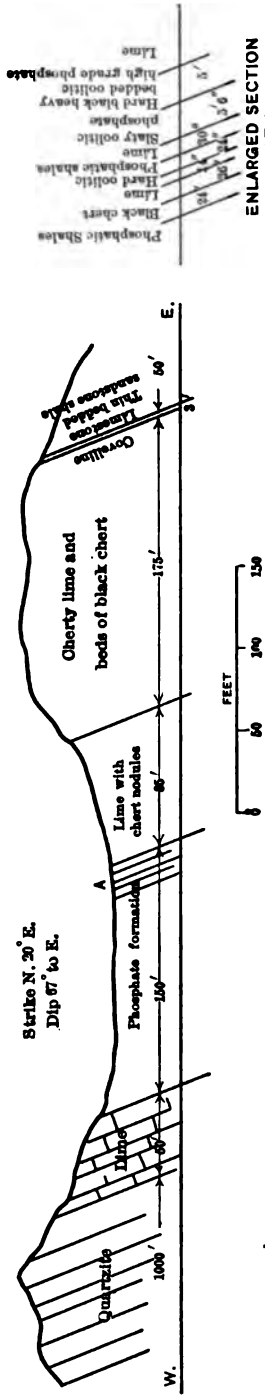

FIG. 13.—SECTION ON NORTH SIDE OF BRAZIER CAÑON.

Situated as the field is, practically half way between the Mississippi river and the Pacific coast, transportation will always be a serious item, especially as far as any export business is concerned, which is, perhaps, a good thing.

At present there are three concerns active in developing and shipping: The San Francisco Chemical Co., the operating department for the Mountain Copper Co., Ltd., which latter is an English concern, shipping to the coast from Montpelier, Idaho; the Union Phosphate Co., of San Francisco, which acquired my interests, shipping from Cokeville, Wyo., to the coast; the Bradley interests, shipping at Sage Station, Wyo., from Utah deposits.

The exposures in highly tilted beds render mining above water level a simple matter for a long time to come.

General Remarks.

The work outlined above, primarily undertaken by me in the interests of the Mountain Copper Co., Ltd., up to June, 1904, and later conducted by myself personally, has demonstrated the existence of a very large phosphate field in the high intermountain country of northeastern Utah, southeastern Idaho, and western Wyoming. The occurrence is as bedded or stratified deposits in the Upper Carboniferous lying above the Weber quartzite of the 40th Parallel Survey, or an analogous formation. The principal rich deposits so far as found seem to lie in that arm of the Carboniferous sea bounded on the north and east by the granite backbone of the Rocky mountains in Wyoming, with a few islands of the same granite lying to the west. It must have been a shallow sea, devoid of currents, so far as we can judge from the bedding of the limestones, with the waters richly charged with phosphoric acid, which by animal, vegetable, and chemical means has concentrated into the beds of oölitic character.

The source of the enormous amount of phosphoric acid is an interesting one, and I hazard the suggestion that a study of the granitic areas surrounding this ancient sea may afford a clue to its origin. There also may have been injections of phosphoric material from volcanic sources, and while the Leucite hills of Wyoming, with their abnormal content of phosphoric acid, are long subsequent to the Carboniferous, they show at least a deep-seated source for phosphoric acid which may also have been active in a contiguous area during the Carboniferous age. The description of the Carboniferous sea by Dana is interesting as bearing on the subject.

The Carboniferous formation is exposed in the area under consideration in a series of north and south mountain ranges, as a result of

block faulting on a large scale, resulting in limited monoclines, anticlines, synclines, and close folds. The valley of Bear river is especially interesting as following a series of faults; rising about 60 miles east of the Great Salt lake, it follows north for a distance of 120 miles and then turns west and south to Great Salt lake.

Especially interesting and worthy of special study is the cherty bed overlying the phosphate formation at the top of the Carboniferous. In places it attains a thickness of 175 ft. of bands of black sinuously bedded hornstone-like material and at others is a mass of dumbbell disk, lily, or sponge-like and other fantastic forms in the lime. There is also a wide distribution of cherty forms in the underlying lime and quartzite beds. The conclusion is forced upon me that these represent sponge or beds of other low organisms.

In the phosphate formation we seem to have a gradually enriched series of beds from the bottom upward, the top bed being the most uniform and practically workable one.

An examination of the geologic folios of Carboniferous areas in the intermountain section shows a constant reference to chert and black or dark beds at the horizon in question, and I feel convinced that future search with the key afforded by the investigations already made will greatly enlarge the field. Much of the country is at present commercially inaccessible. A wise Providence seems to have made the Carboniferous coal bearing in the Appalachian and phosphate bearing in the Rocky Mountain region, the difference arising in the one being a swamp with a collection and laying up of vegetable material, and the other a shallow sea with the deposit by animal, vegetable, and chemical means of the vast amount of phosphoric acid borne by it from the degradation of the land areas.

It behooves the government of the United States to preserve for future generations this great storehouse of phosphoric acid, so essential to the wellbeing of any nation, and not to allow it to be distributed to every other nation for the temporary profit of a few dollars. This can be protected now when the private holdings are comparatively small and the bulk of it still to be discovered on the public domain. It would seem to be a legitimate function of the U. S. Geological Survey to have control of the matter.

Los Angeles, Cal., Jan. 23, 1907.

The foregoing is the copy of a paper hurriedly prepared on the request made to me in one of the following letters, which are reproduced in chronologic order.

The appended letter to the President contains, I believe, the first suggestion as to the national conservation of our phosphate resources; and while the procedure has not yet worked out in the rational way I had in mind, it is in such shape that simple legislation ought to open the way for leasing on some such basis as coal. In the meantime there seems ample competition in the field to supply the present markets properly.

MONTPELIER, IDAHO, October 16, 1906.

To His Excellency,
Theodore Roosevelt,
President of the United States,
Washington, D. C.

Sir:

I have the honor to address you on a subject worthy of your close investigation.

In removing from entry all remaining coal lands in the public domain I would suggest that there be included also all phosphate lands.

In 1903 I undertook to search for phosphate lands in the West in the interest of the Mountain Copper Company of California. In that search I was successful in finding what promises to be the largest phosphate field in the world, lying in Western Wyoming, Idaho, Utah and Nevada.

Certain irreconcilable differences arising between myself as an American Engineer and the above alien English corporation caused a severance of our relations in 1904. Since that time, on my own means and initiative I have made further vast discoveries which in 1905 became the property of a domestic California corporation.

In making to you the suggestion to remove from location and entry all phosphate lands remaining on the public domain until the subject can be thoroughly investigated by the U. S. Geological Survey I am actuated by motives of the purest patriotism. By remaining quiet I could probably make a great deal of money in the next few years, but looking ahead I can see that the field will probably be owned and dominated by vast interests inimical to the best interest of the nation as a whole.

Phosphates and phosphoric acid are necessary to the human race in raising its food supplies and such deposits should essentially be the property of the nation, to be leased out under a proper system and in proper quantities to responsible parties.

The U. S. Geological Survey has recently had an engineer in the field and is aware of some of the facts I have given you above.

Now seems the time for action to preserve properly these deposits and I trust you will appreciate that I am writing you strictly from patriotic motives.

I am, sir,
Your obedient servant,
CHARLES COLCOCK JONES.

In answer to which I received the following:

DEPARTMENT OF THE INTERIOR,
UNITED STATES GEOLOGICAL SURVEY,
WASHINGTON, D. C.

Nov. 1, 1906.

Mr. Charles Colcock Jones,
247 Pacific Electric Building,
Los Angeles, California.

Sir:

Your letter of October 16 addressed to the President has been referred to this office for reply. I agree with you as to the desirability of preventing the newly discovered phos-

phate deposits of Idaho and adjacent States from falling into the hands of a monopoly. This result could doubtless be prevented by the withdrawal of the lands containing the deposits from mineral entry. Such withdrawal, however, involves a radical departure from the Government's policy with reference to mineral deposits on the public domain. A precedent for such withdrawal has, however, been established by the recent action of the President in regard to coal lands, and it is probable that he would look with favor on a similar line of action in connection with these phosphate deposits. It is necessary, however, before the withdrawal can be made that the exact location of the deposits should be known. Arrangements were made last winter with a geologist in private practice, who had been engaged in the examination of these phosphate deposits for some time, to prepare a report for Survey publication. Up to the present, however, this report has not materialized and the Survey, therefore, has no exact information regarding their location, on which to base recommendations. If such information is anywhere available I should be glad to receive it, and in any case have planned to make a thorough investigation of the deposits as early as possible next spring. I should be glad to receive from you any information you have regarding the location, character and value of the deposits. Such information will be treated as confidential and used only in such manner as may be approved by yourself.

Very respectfully,

H. C. RIZER,
Acting Director.

LOS ANGELES, CAL., Dec. 5, 1906.

Director U. S. Geological Survey,
Washington, D. C.

Dear Sir:

Your letter of November 1st, was received on my return to Los Angeles some few days ago; a continued absence in Utah and Idaho rendering it impossible to forward the same to me.

Since broaching this subject of the withdrawal of phosphate lands, I have given the matter considerable thought and during the time, have been over a large part of the field. As a practical question, I hardly see now how the withdrawal from entry could be accomplished, unless the Survey instituted a regular series of exploration which would prove up the available territory.

I find that it will be very much larger in extent than I had at first supposed.

A reference to my letters to the U. S. Geological Survey, beginning in August 1903, shows that the formation has been definitely determined as the upper carboniferous and this extends over vast areas of western Wyoming, Utah and Idaho, and while the present developments have proved the existence of phosphate in a number of places contiguous to railroads the exposures by reason of block faulting are comparatively few, so that the statement of the exact location of the deposits would be of little value as bearing on the question of withdrawal from entry.

Having been the original locator of most of these deposits and having, perhaps, the largest accumulation of information in my note books and reports on the subject, I feel that I am, perhaps, well qualified to make a report for publication on the subject.

I was disappointed, while recently in Salt Lake City, in not meeting Mr. Walcott as I would have been pleased to discuss the matter personally with him.

I am now working on my notes, maps, etc., with the intention of either submitting them during the next month to Lehigh University or to the Survey. The paper will deal with the entire history of the discovery and contain a great many facts, known only to myself, and I will be pleased to have the views of the Director in this matter.

I have had a number of slides made and would be pleased to learn from you where I can have micro-photographs of the same made.

Investigations, since forwarding my letter to the President have convinced me that the field is considerably larger than even I had thought it and while as a general proposi-

tion the conservation of such deposits for the people as a whole is desired, unless as I said, the Survey is prepared to have a special bureau investigate and establish the workable areas of phosphate lands to be withdrawn from entry it would be a drawback to development; however, it might be possible to so restrict holdings of lands as to obtain the object in view.

Very respectfully,

CHARLES COLCOCK JONES.

DEPARTMENT OF THE INTERIOR,
UNITED STATES GEOLOGICAL SURVEY,
WASHINGTON, D. C.

December 12, 1906.

Mr. Charles Colcock Jones,
247 Pacific Electric Building,
Los Angeles, California.

Dear Sir:

Referring to your letter of December 5, relating to phosphate lands in Utah and Idaho:

As stated in my letter of November 1, it is hoped that detailed examinations of these deposits may be undertaken next season. In view, however, of the great interest in this matter I should be glad to see published at an early date such information regarding them as is available. As you are doubtless aware, a volume is published each spring entitled *Contributions to Economic Geology*, made up largely of abstracts of more extended reports and short papers of economic interest. I am sending you under separate cover the bulletin for 1905. If you should care to prepare a brief paper for the 1906 bulletin and could submit it before February 1, 1907, I would be glad to consider its publication.

Very truly yours,

H. C. RIZER,
Acting Director.

Acting upon this request the foregoing paper was written and forwarded with the following reply to letter of December 12:

LOS ANGELES, CALIFORNIA,
January 23rd, 1907.

Director U. S. Geological Survey,
Washington, D. C.

Dear Sir:

I have the honor to submit to you the brief paper for 1906 bulletin of *Contributions to Economic Geology*, as requested in your letter, of December 12th, 1906.

I enclose also with this, a drawing of the sections mentioned, from which tracings can be made at your office, and the combined map of those portions of Utah, Idaho and Wyoming with the known outcrops of the phosphate formation marked by red lines.

I have thought it best to put the preliminary article in its present form, not so much for any honor, as far as I am personally concerned, but as an interesting narrative of a work that absorbed me greatly as being somewhat of a guide to others in the field, carrying out investigations on a purely geological basis.

I have tried to make it so it would require as little editing as possible and leave it to your judgment as to the propriety of some of the last general sentiments expressed in the paper.

I am forwarding you also, illustrative photographs of openings and a box of microscopic slides, the latter of which, if not of any present use, can be returned to me.

In addition to the fossils, I have had the pleasure of forwarding you, I have a number

of interesting specimens which either now or later I will put at your disposal for illustrative purposes.

Trusting that this paper meets your wishes and hoping that I may be able to render you further service at any time, I am

Very truly yours,

CHARLES COLCOCK JONES.

Rather to my surprise this paper was returned with the following explanation:

In reply please refer to GB and date of this letter.
SUBJECT: Returning manuscript on phosphates of the United States.

DEPARTMENT OF THE INTERIOR,
UNITED STATES GEOLOGICAL SURVEY,
WASHINGTON, D. C.

February 2, 1907.

Mr. Charles Colcock Jones,
247 Pacific Electric Building,
Los Angeles, California.

Dear Sir:

Your letter of January 23, accompanied with a paper on the "Discovery and Opening of a new Phosphate Field in the United States," was duly received.

The paper has been carefully examined and in its present form is not adapted for publication in the *Contributions to Economic Geology*. Since this report will go to press in a few days there will be no opportunity for you to revise the paper. Moreover, as stated in my letter of November 1, 1906, arrangements were made last winter for the publication of a paper on this subject just received from the author, and this will be supplemented with information obtained by one of the Survey geologists during the past season. Your paper contains much matter of interest and I trust you will have it published so that this information may become available. I have no doubt that any of the mining journals would be very glad to secure it.

I am returning the manuscript and illustrations to you under separate cover, registered.

Very respectfully,

H. C. RIZER,
Acting Director.

In the *Engineering and Mining Journal* of May 18, 1907, appeared an abstract from this paper, and which was perhaps the first actual description of the phosphate field.

Shortly afterwards *Contributions to Economic Geology*, 1906, was issued containing "Phosphate Deposits in the Western United States," by Weeks and Ferrier, and subsequent annual issues of the same publication have kept the world informed of the enormous extent of this phosphate field as its boundaries have been extended by the investigations of the Survey.

The remarkable thing about this phosphate field is that it lay unnoticed so long and practically undiscovered, for at Rich's Hot Springs on Bear lake characteristic phosphate openings were shown to many by the late Joseph Rich.

T. L. Gleen, of Montpelier, Idaho, while a member of Congress about 1904, submitted samples to the Smithsonian Institution, which reported the phosphatic material not a coal and worthless.

Since the opening of the field the statement has been made through the technical press by several that they knew of the existence of phosphate rock in Wyoming, but so far as I have been able to learn the analysis by Thomas Price & Son, in 1899, was the first authentic recognition of the true nature and value of the rock.

The Survey has accomplished a vast deal of work in the phosphate field, the results of which it has published with a wealth of detail in various publications. The following extracts from *Classification of the Public Lands*, 1913,[1] are illuminative:

"*Lands Containing Phosphate.*

"Deposits of phosphate in the Rocky Mountain States were first discovered in northeastern Utah and southeastern Idaho, in the vicinity of the Idaho-Utah-Wyoming line. From this locality the deposits have been traced south, east, and west halfway across the States of Idaho, Utah, and Wyoming and northward to the vicinity of Helena, in west-central Montana, so that the phosphate beds now known cover an area extending about 220 miles from east to west and 420 miles from north to south. Of course only a small part of this territory is underlain by deposits that are commercially valuable."

"The estimated quantity of high-grade rock (containing 70 per cent. or more of tricalcium phosphate) included in the area surveyed in detail to date is more than 3,000,000,000 long tons."

"*Summary of Phosphate Situation.*

"The question of the future adequacy of our phosphate resources for our own needs had been mentioned by several authorities prior to the conference of the governors in 1908, in which the discussion of this and kindred topics drew public attention to the situation. At this conference the possibility that foreign investors might acquire the better-known and supposedly richer portions of our deposits was suggested, the wisdom of permitting the exportation of so essential a quasi-public commodity was questioned, and the desirability of an early examination of the available supplies was emphasized. In part as a result of these indications of public interest, in part as a continuation of the policy already adopted in reference to coal lands, and in part because of the legal dilemma existing in the western fields through the inadequacy of the laws governing the disposal of mineral land the Secretary of the Interior, on December 10, 1908, withdrew from entry about 7,000 square miles of public land in Idaho, Utah, and Wyoming, pending an examination of their phosphate resources. In the following summer the United States Geological Survey began the examination of these lands and the investigation has been continued up to the present time, some 4,000 square miles having been examined in a preliminary way and about 2,500 square miles surveyed in detail.

"The first withdrawal was based partly on information collected by the Hayden Survey in 1877 and partly on later detailed and reconnaissance examination made by the United States Geological Survey. Field work done subsequent to this withdrawal revealed the regularity and the character of the phosphate deposits, so that it has been possible not only to revise the estimates of the reserves in the area actually examined since the first withdrawal but also to make a closer interpretation of the information gathered by the earlier surveys."

[1] *Bulletin No.* 537, *U. S. Geological Survey*, pp. 124 to 127 (1913).

Notes on the Occurrence of Some of the Rarer Metals in Blister Copper.

BY A. EILERS, NEW YORK, N. Y.

(Butte Meeting, August, 1913.)

A NUMBER of the copper refineries in this country have lately separated some of the rarer metals from the slimes in the refinery tanks. One of these has furnished me the following table of recoveries from given amounts of blister.

I publish this to show the proportions, to gold and silver and to each other, in which these rarer metals may be expected to occur in copper from certain localities.

TABLE I.—*Reeoveries of Rare Metals from Blister Copper.*

	Blister. Tons.	Gold. Ounces.	Silver. Ounces.	Platinum. Ounces.	Palladium. Ounces.	Selenium. Pounds.	Tellurium. Pounds.	Bismuth. Pounds.	Nickel. Pounds.
Garfield	5,000	14,400	174,000	17.09	59.14	2,800	277	305	2,000
Per 100 tons blister	.....	288.0	3,480	0.342	1.183	56.0	5.54	6.1	40.
Steptoe	3,000	5,070	16,500	30.48	132.10	3,308	None	10	1,920
Per 100 tons blister	.....	169.0	550	1.016	4.402	110.1	.....	0.33	64.
Omaha	800	2,880.	184,720	14.60	51.89	213	537	151	7,552
Per 100 tons blister	.....	360	23,090	1.825	6.486	26.6	67.1	18.6	944.
Mountain, Cal	150	2,127.	16,485	1.98	0.91	54	5	41	172
Per 100 tons blister	.....	1,418	10,990	1.320	0.607	36.0	3.3	27.3	11.5
Tacoma	800	17,496	69,680	5.68	26.62	336	None	46	6,160
Per 100 tons blister	.....	2,187.	8,710	0.710	3.327	42.0	.....	5.7	770.
Aguascalientes	1,100	5,302	740,300	4.58	2.49	1,870	None	44	132
Per 100 tons blister	.....	482.	67,300	0.416	0.226	170.0	.....	4.0	12.
Cerro de P.	2,000	3,400	198,000	6.38	11.77	275	None	270	640
Per 100 tons blister	.....	170.	9,900	0.319	0.589	13.7	.....	13.5	32.
Mt. Lyell	800	3,716	57,640	4.99	10.99	336	None	34	1,328
Per 100 tons blister	.....	464.5	7,205	0.624	1.374	42.0	.....	4.3	166.

GARFIELD, UTAH:—The blister copper comes principally from the Bingham porphyry copper mines.

STEPTOE, NEV.:—All the blister copper is produced from the Nevada Consolidated mines. (Also porphyry deposits.)

OMAHA, NEB.:—Blister copper comes principally from concentrated copper-lead mattes shipped from the different lead-silver plants of the Rocky Mountain region to the converters at the Omaha lead refinery.

MOUNTAIN, CAL.:—Ores occur in connection with diorite (?).

TACOMA, WASH.:—The blister is produced from Pacific Coast and Alaska copper ores, all of which occur in connection with diorite dikes and masses.

AGUASCALIENTES, MEXICO:—Blister is produced from the smelting of silver and gold ores with mostly low-grade copper ores, coming from all parts of Mexico and from many different geological occurrences and connections.

CERRO DE PASCO, PERU:—Blister from copper and lead-copper mattes produced at the Cerro de Pasco works. Ore originates in veins occurring in limestone near andesite masses.

MT. LYELL, TASMANIA:—Blister is from the well-known low-grade copper deposit of Tasmania. The geological occurrence is not known to me.

I am aware that the above recoveries do not represent the complete contents of the rare metals named, and I suspect that there are others in the original ores, which are lost in the successive metallurgical processes. Nevertheless, an imperfect record is better than none. Furthermore, it may serve as an incentive to our chemists for further research.

It may also direct the attention of some of our geologists, who work on the lines of chemical and physical influences in the original deposition of ores, to trace the reasons why the proportions of the rarer metals to each other are so different in different ores. It seems to me that geological conditions—broadly considered—must be to a great degree responsible.

As to the financial outcome of the extraction of the rare metals named, it must be said that, while a few have a good market, the majority have not. For them, markets must be made by new uses in the arts.

The Reducibility of Metallic Oxides as Affected by Heat Treatment.

BY WOOLSEY MCA. JOHNSON, HARTFORD, CONN.

(Butte Meeting, August, 1913.)

In metallurgical circles it is known widely, but somewhat vaguely, that the ease of reduction of metallic oxides depends largely on the way they have been prepared. It is likewise known that different forms of carbon have different and greatly varying reducing powers. It is the purpose of this paper to show somewhat more definitely and scientifically these facts.

Some years ago the writer published an account of pyrometric determinations of the reduction temperature of zinc oxides variously prepared and of several kinds.[1] In said paper it was shown that the temperature at which zinc oxide evolves zinc according to the equation

$$ZnO + C = Zn + CO$$

depends upon the nature of the two reacting bodies. The determinations had a range of accuracy of from 2° to 6° C., whereas differences were found to amount to an extreme of 90° C. So the differences are real ones and must have a definite cause. The equation of boiling water,

$$(H_2O)_4 \leftrightharpoons 4\ (H_2O)$$

has a fixed temperature for equilibrium of 100° C. at 760 mm. pressure. In general, all equations reacting in the liquid or the gaseous phase have well-defined constant physical conditions which govern the equilibrium point and reaction velocities.

In a reaction where we have one or more solid phases, the temperature conditions requisite for the unbalancing of the stable system are found to vary. These variations are due to the fact that in a solid there is a very complex molecule and it is not a question solely of the tearing away of two interlocked atoms, but also of the destruction of a complex molecule containing hundreds, or possibly thousands, of smaller molecules.

In other words, there are two forces to be overcome: a certain

[1] *Transactions of the American Electrochemical Society*, vol. v., p. 211 (April, 1904).

chemical force, F_c, and a certain "physical" force, F_p. Theory indicates that F_c is constant and F_p is variable. F_p depends on the physical configuration of the small molecules of the metallic oxide.

The writer's researches in this line have been confined to differences caused by heat treatment of zinc oxide and reducing agents. In the zinc business we all know that roasted blendes of various kinds work differently in retorting. Roasted blendes from one mine work off easily

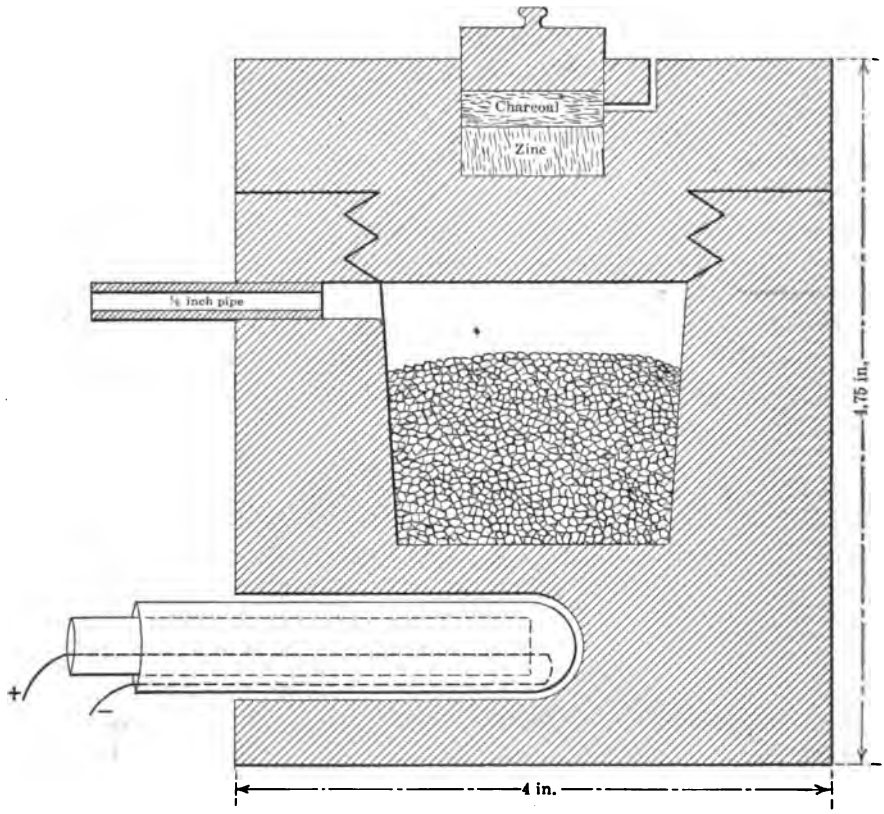

FIG. 1.—JOHNSON "REACTION TESTER."

at a low temperature, whereas roasted blendes from another mine, and of the same analysis and with the same reducing agent, work hard and slow even with high firing. As the furnaceman says, such ores need a touch of "high life."

Fig. 1 shows the Johnson "reaction tester." This is made of Acheson graphite electrode by machining 4-in. round stock in a lathe. The pyrometer is inserted in a hole very near the charge, so that there is but a small temperature difference between it and the charge. The

starting of the reaction is very sharp. The only precaution necessary is to see that the reaction, which starts at, say, 1,040° C., is active at 1,043° C., energetic at 1,045° C. and violent at 1,050° C. As a milligram of zinc will give quite a flame, a false value of the sporadic reaction of reagents, not typical of the mass, will result unless this precaution be used. To avoid misunderstanding let us suppose that in the 10 g. of zincky material in the tester, 5 mg. were of a soft nature, reducible at 20° C. below the real reduction temperature of the mass; this would give a false value unless reaction proceeded to an active extent and was well marked.

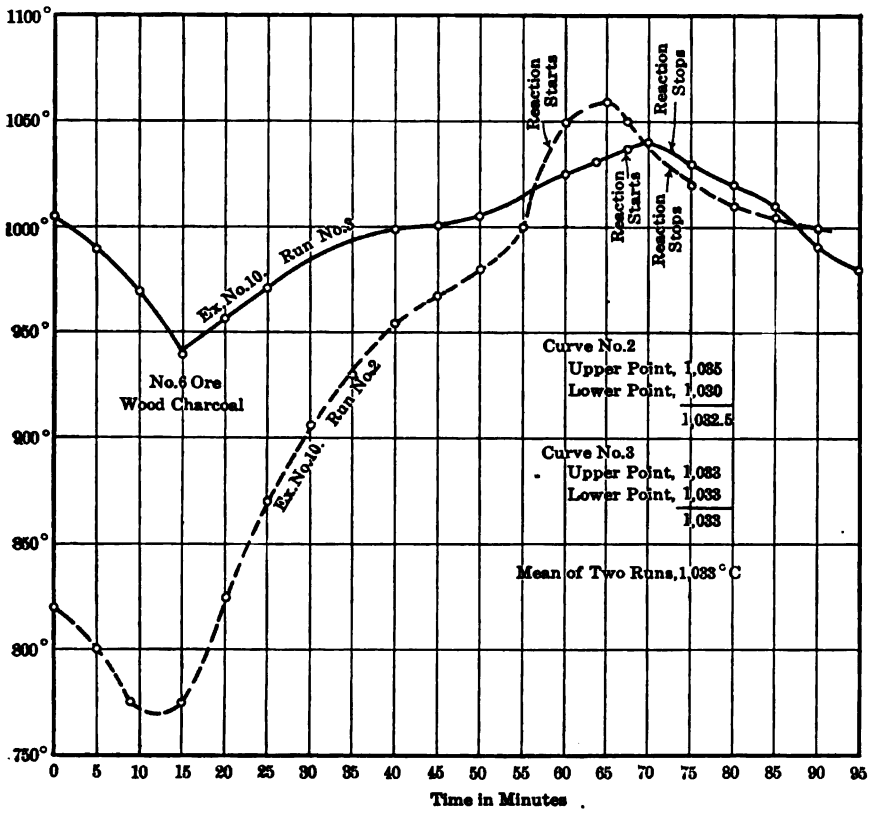

FIG. 2.—REDUCTION TEMPERATURES.

On cooling down, the reaction stops at nearly the same temperature at which it began. The mean of starting and ending is taken, and two or three determinations of each test are made. With careful and gradual increase of temperature the mean of means does not vary from the extremes by over 2° C. A curve of a test better than the average is shown in Fig. 2.

The pyrometer was checked by determining the 920° C. point by the boiling point of zinc. In the latter tests this was so accurate that we could determine the differences in atmospheric pressure by changes in the boiling point. The zinc was put in a 0.5-in. hole with a plug in the tester and well covered with charcoal. Unless this is done the air will rush in at 900° C. and burn the zinc vapor which exists there in quantities, making a partial vacuum by the formation of solid zinc oxide, and the boiling points will vary widely.

As the 920° C. point is taken with each determination, there is little chance for the pyrometer to get wrong.

Looking at Table I., we see that Test 102, with C.P. zinc oxide made by the wet method and soft 600° C. charcoal, gave a reaction temperature of 1,022° C.; 48-hour Indian Territory coke gave, with the same substance in Test 101, a temperature of 1,029.5° C.; with Acheson graphite turnings the reduction temperature was 1,085° C.

The same substance, C.P. zinc oxide, was treated by heating at definite temperatures in an oxidizing atmosphere for 12 hours and its reaction temperature with soft coke was determined. The three values of C.P. wet-method zinc oxide are as follows:

	Reduction Temperature.
No previous treatment	1,029.5° C.
1,100° C. treatment	1,048° C.
1,300° C. treatment	1,061° C.

The effect of the different reducing agents is shown as follows:

C.P. Cadmium Oxide, Tests Nos. 80, 76, and 81.

Wood charcoal burnt at 1,100° C.	772° C.
Indian Territory coke	813° C.
Acheson graphite turnings	849° C.

Referring to Table I., it will be seen that this law can be deduced: *The higher either of the reacting bodies is previously heated, the higher is the reduction temperature.*

The fact that the values are concordant with this law, and the fact that the reductivity of the zinc charge varies with the previous treatment, as determined by measuring the reaction velocity according to another method of the writer's, shows that the heat treatment of the reacting bodies modifies the physical-chemical nature profoundly.

Exactly how the zinc oxide parts with its oxygen and exactly how the oxygen gets to the carbon of the reducing agent is a mystery, but no more a mystery than any reaction between solid particles. A natural assumption is that zinc oxide has a slight vapor pressure (indeed,

zinc oxide heated to a high temperature in a crucible will slowly evaporate), that carbon has also an infinitesimal vapor pressure, and that when the sum of these vapor pressures equals a certain figure, the zinc oxide dissociates into oxygen, which combines with the carbon, and metallic zinc. Carbon monoxide is formed and both gases are evolved. This speculation, if continued, would lead us into the domain of molecular physics; but that which the writer has given, though theoretical in its nature, has a strict practical bearing. There have been zinc metallurgists who have the opinion that zinc oxide reduction goes into two stages.

$$\text{I.} \qquad ZnO + CO = Zn + CO_2.$$
$$\text{II.} \qquad CO_2 + C = 2\,CO.$$

With this view the writer is unable to agree. It is of course plain that CO will reduce zinc oxide, for many experiments, including the writer's, have proved this, but not to zinc directly to any extent, for the reaction

$$ZnO + CO \leftrightharpoons Zn + CO_2$$

is reversible and CO_2 can exist only in small amounts at 1,100° C.—say about 1 per cent.—without oxidizing zinc vapor. The direct chemical "push," or the atomic kick that starts the reaction and keeps it going, comes from the carbon molecule. The fact that variations in the physical nature of the reducing agent make a difference of 80° C. in the critical temperature at which this kick becomes effective indicates that this kick comes from the carbon molecule, and not from the secondarily-generated carbon monoxide.

The writer does not argue that the reactions I. and II. do not occur, for that would be as foolish as to say that it is impossible to go to Boston except on the trains of the New Haven railroad. It would of course be possible to take the New York Central train to Albany and then the Boston and Albany to Boston, but it would cost more. And nature does not waste energy in an endothermic reaction. The writer has a high regard for this endothermic reaction:

$$ZnO + C = Zn + CO.$$

This reaction takes the path of least resistance. The fact that reduction proceeds very much faster in the writer's electric zinc-smelting furnace, when only enough carbon for reduction is added, than in retort practice would seem to dismiss most of the importance attached to equations I. and II.

The conclusion follows that, when the temperature of the reacting bodies is considerably above the initial reduction temperature, the direct chemical path is so crowded that the double reaction I. and II. operates to some extent: When the New Haven railroad trains are full one would go to Boston via Albany.

Taking up the question of the changes in the nature of the zinc oxide as affecting reduction temperature, it might be well to consider zinc ferrite, zinc silicate, and zinc aluminate. All these have been made by the writer by heating Fe_2O_3, SiO_2, and Al_2O_3 mixed with molecular equivalents of zinc oxide. They are insoluble in ammonium chloride and dilute acids, and, if strongly heated, insoluble in concentrated acids. They are also inert to reduction by carbon and have high reduction temperatures.

We can, however, derive a useful lesson from these compounds. For, *if zinc oxide can combine with metallic oxides as aluminum oxide and iron oxides, why can it not combine with another metallic oxide: to wit, zinc oxide?* Accordingly, we can say that, when zinc ores are heated very strongly, "zinc zincate" is formed, and that the energy of this compound must be supplied to break it up before the reaction can proceed.

The writer has found that these experimental facts agreed with the practical experience of himself and many others in the zinc business; for instance, it is a fact that "spiegel oxide," which is formed in the downcomers and in the hot-blast stoves of the New Jersey Zinc Co.'s spiegeleisen furnaces working on residues from the "oxide" furnaces, and which is thus made at a low temperature, requires little heat in "retorting," whereas "refuse oxide," made in the Wetherill furnaces of the same company at a temperature vastly higher, requires a much higher temperature to "retort." "Dead" coal, or coal from the strip pits of western Missouri, has been known to reduce zinc oxide much more easily than Arkansas semi-anthracite, as the tests show. It is natural to expect that the weathering action makes the carbon more active.

The tests show, of course, only the initial temperature of reduction and say nothing about the rate of reduction. About 150 tests were made to determine this value by heating different mixes for a definite time and determining reduction by analyzing the mix before and after test. With equivalent amount of fixed carbon and ash in the mix the rate of reaction is roughly proportional to the initial temperature or statical point. This is in accordance with theory.

Before any chemical action takes place in the highly endothermic reaction, the reduction of zinc oxide by solid carbon, a definite thermochemical potential must first be obtained. Of course, in such a reaction the free energy determines the velocity of reaction according to the Gibb-Helmholz equation.

The chemical potential of the reverse reaction is higher than that of reduction before this thermochemical potential is attained. The

difference of potential is a function of the difference of temperature between the "reaction temperature" and the temperature of the reacting bodies. Thus, should we have the reduction temperature at 1,050° C. and the function be a linear one, then, when we have the reacting bodies at 1,100° C., the reaction velocity will be five times faster than it would be should the reacting bodies be at 1,050° C.

As in Ohm's law $\text{Amperes} = \frac{\text{Volts}}{\text{Ohms}}$, so Professor Nernst has shown that, in like manner, the rate of chemical change is equal to $\frac{\text{Chemical force}}{\text{Chemical resistance.}}$ The rate at which zinc is reduced in a retort is determinable should we know the chemical force or chemical potential, because then the reaction velocity can be found by means of the equation given above.

In all heat treatment the question of time is important, for the formation of the "zinc zincate" proceeds very slowly. The heating was usually for 12 hours, or over night.

In the classical work of the late Sir Lowthian Bell, *Chemical Phenomona of Iron Smelting*, on p. 17, *et seq.*, it will be seen that he experimented with iron oxides. The substances he treated were spread out in a weighed dish so as to secure free access to the gas. The following results were obtained by him by exposing the substances enumerated below to the action of pure CO for 7 hours. The temperatures were indicated by the use of pure lead, zinc, and antimony test pieces.

Exposure.		Percentage of Original Oxygen Removed.
26. Calcined Cleveland ore to	CO at 417° C.	9.4
27. Pumice stone with Fe_2O_3	CO at 417° C.	23.8
28. Calcined $FeSO_4$ (Fe_2O_3]	CO at 417° C.	61.7
29. Fe_2O_3 precipitated	CO at 417° C.	66.7
30. Fe_2O_3 from nitrate	CO at 417° C.	72.7
Exposure for 7.5 hours at the temperature of softened zinc (410° C.):		
31. Lancashire hematite,	40.3 per cent. of Fe.	37.1
32. Fe_2O_3 from nitrate		59.9
Exposure for 7.5 hours at the temperature of softened zinc (410° C.):		
33. Calcined Cleveland ore,	40 per cent. of Fe.	20.7
34. Calcined spathose ore,	51.7 per cent. of Fe.	28.4
35. Lancashire hematite ore,	40.3 per cent. of Fe.	57.4
Exposure for 6 hours at a temperature of about 410° C.:		
36. Calcined Cleveland ore,	40 per cent. of Fe.	37.3
37. Lancashire hematite,	66.6 per cent. of Fe.	35.9
38. Elba specular ore,	66.8 per cent. of Fe.	16.8
39. Calcined spathose ore,	51.7 per cent. of Fe.	15.4
40. Pure Fe_2O_3, precipitated,	70.0 per cent. of Fe.	49.2
41. Pure Fe_2O_3, from calcined $FeSO_4$	70.0 per cent. of Fe.	60.8

Discussion.

F. L. Grammer, Leesburg, Va. (communication to the Secretary *):—Mr. Johnson's paper must interest by suggestion those connected with the "burning" of lime, the production of cement, and the smelting of iron.

All iron men know how quickly the impalpable purple ore of wet origin reduces compared to finely ground red hematites of approximately identical composition obtained from Lake lump ores.

Most housekeepers appreciate that the several forms of carbon—charcoal, coke, and anthracite, not to include graphite and diamond—have different temperatures of ignition. The temperature usually rises with the density and hardness.

Some of the scientific bureaus of the government might appropriately conduct some tests along lines suggested by Mr. Johnson's article. We are unable to say whether the topic lies within the spheres of activity of the Bureau of Mines, the Bureau of Soils (Agricultural Department), the Geological Survey, or the Bureau of Standards. The calcining qualities of limestone is a topic too wide for individual effort and is of wide enough interest to merit government or State activity. It is of interest, as aforesaid, to burners of lime, cement manufacturers, and smelters.

The chemical composition of a limestone does not tell all the story—if over 1-in. size lumps. The limestones of Annville and Myerstown, Pa., break at a tap; those on the Hudson above Poughkeepsie act like "nigger heads." The manager of lime kilns near Lake Champlain told me his fuel consumption was high compared to that at other points. Yet the Hudson and Annville stones were both of greatest purity, being rivaled by only the marble at Rutland, Vt., or some stones near Martinsburg, W. Va. A local kiln operator says the conglomerate or pudding limestones of Loudoun county, Va., act very differently in the kiln from the more normal limestone deposits nearby. The tilted and twisted bedding in the anthracite region tells the story of hard coal origin. We might therefore expect that the limestones of greater geological age or depth or metamorphism (which are usually those most suited for building and ballast) would be least easily calcined.

Heretofore these practical differences recognized by the man who swings the sledge have been shown chiefly in repair items on the crusher. They would be more properly expressed in temperatures

* Received Aug. 9, 1913.

and heat units. But the calcination should be complete to make the comparisons of value. Manganese ores lose their first 40 or 50 per cent. of oxygen more readily than iron ores, but the final elimination is accomplished by a much larger expenditure of heat.

Often hardness and specific gravity will afford adequate indices of the refractory qualities of ore and stone. Every one appreciates that hard and heavy magnetites (usually attributed to igneous origin) are more refractory than hematites of water origin.

In handling a highly sulphurous roasted magnetite I once noted that if low sulphur in pig iron were the chief aim the very pure soft Annville limestone was most acceptable, but when off casts were made the loss through buckshot was much greater than with a poorer bastard dolomite found in the neighborhood. This may not be pertinent, but I am not persuaded that the several per cent. of magnesia made the difference between the two fluxes.

Many of us have acted as though the sole satellites of such men as Barus, Arrhenius, Nernst, and Jones were the followers of electrometallurgy and industrial chemistry. Physical chemistry concerns us also. The catalytic value of manganese in the fertilizing serviceability of phosphoric basic slags is an appropriate topic for our papers as well as for the *Transactions* of other societies. Perhaps we can later have other tests made as to effect of physical nature on chemical activity.

It is probable that a given temperature and pressure of blast is best suited to each of the several forms of carbon fuel—charcoal, coke, and anthracite, and this also would be a proper subject for government inquiry.

Certainly the electric furnace offers a means of accurate experiment unknown to Sir Lowthian Bell; possibly some of his conclusions would be altered.

The sample of fused lime mentioned by Mr. Johnson would seem to us to indicate that where heat consolidates and hardens, the second state of the substance is more obstinate to chemical change—such as wood to charcoal or coal to coke; but where heat disperses and opens, the second state of the substance is more amenable to change by later heat action, viz., ice melted to water, heating ore lumps to make friable—expelling combined water and CO_2—making same more reducible.

H. O. HOFMAN, Boston, Mass. (communication to the Secretary*):—The formulated statement of the author that in the reduction of

* Received Sept. 3, 1913.

metallic oxides, the higher either of the reacting bodies is previously heated, the higher is the reduction temperature, will strike home with all who have considered the subject. The fact is one which seems familiar; it required, however, the evidence furnished to make it sure.

The data of Bell which the author quotes serve as a reminder of the experiments carried on by Laudig and Bachman in 1896,[1] in which they subjected various iron ores to the reducing action of the tunnel-head gas of an iron blast furnace at 432° C. and noted the reduction and carbon deposition.

Below are given the results of some experiments I carried out with an electric resistance furnace in connection with some other work, in order to obtain numerical data in regard to the reduction of a hard hematite by means of several gaseous reducing agents.

The furnace was a vertical alundum tube 2 in. in diameter and 10 in. long wound with excello wire and packed in magnesia inclosed in a sheet-iron pipe. In order to protect the alundum against slagging, the tube was lined with a thin sheet of copper. The bottom of the tube was closed by an inverted crucible with perforated bottom and the joint made tight with a litharge-glycerine cement; the top was covered with wire gauze. The furnace was approximately standardized by passing through the heating wire the amperes necessary for obtaining certain temperatures. In making the tests the ore was charged into the tube, the gas passed through, the necessary electric current turned on, maintained for the desired time, turned off, and the ore allowed to cool in the gas current. The treated ore was emptied on to paper by tilting the tube and drawing it back. This spread the ore in the form of a long strip, from which samples were taken for analysis from top, middle, and bottom.

The reducing agents used were:

Marsh gas, with approximately CH_4, 90; H, 8; CO_2, 2 per cent. vol.

Carbon monoxide, practically pure.

Boston illuminating gas. Average composition: C_2H_{2n}, 15.0; CH_4, 25.9; CO, 15.3; H, 27.9; CO_2, 2.9; N, 3.0 per cent. vol.

Producer gas of an industrial plant with approximately: CO, 25; H, 10; N, 65 per cent. vol.

The metallization of iron was determined by boiling a finely ground sample of ore with a definite amount of solution of blue vitriol of known concentration, filtering, and titrating the copper in the filtrate.

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[1] *Trans.*, xxvi, 269, 1061 (1896).

The results obtained are given in the accompanying Table I.

1. *Marsh gas* has practically no effect within a range of 680° and 1,000° C.; only one of the six tests made showed some metallization.

2. *Carbon monoxide* gave no metallic iron, as shortly after a test had been begun the carbon deposition became so vigorous that the passages between the ore particles were choked with soot, and the test had to be stopped. If the gas had been diluted with nitrogen, less carbon would have been deposited, and some of the iron oxide reduced to the metallic state.

3. *Boston illuminating gas* shows a reduction of oxide between 880° and 1,040° C., the two highest percentages of metallization being 57.81 and 64.61 per cent. The C_2H_{2n} is in part decomposed by heat setting free carbon; the ore, firm when charged, has become friable; the soot, carbon monoxide, and hydrogen have penetrated the ore more or less; finely divided metallic iron is disseminated through unreduced ore.

4. *Producer gas* showed reduction of oxide in all tests; here again the hard ore has become friable and permeated with soot and with powdery metallic iron in intimate contact with oxide.

The results show that the two active gaseous reducing agents, carbon monoxide and hydrogen, reduce only in part a hard hematite, as had been shown for carbon monoxide by Bell; and that, irrespective of the reversibility of the reaction, the progress of the reduction is not simply a function of temperature and time.

TABLE I.—*Reduction of Hard Hematite.*

(20-Mesh.)

Reducing Gas.	Per Cent. Iron in Untreated Ore.	Time of Treatment. Hr. Min.	Current in Amperes.	Temperature °C.	Sample of Ore in Furnace, Taken From	Per Cent. Iron in Treated Ore.	Per Cent. Metallic Iron in Treated Ore.	Per Cent. Metallization
Marsh gas	47.26	5–25	7.2	840	bottom	48.44	none	none
				940	middle	47.07	none	none
				680	top	45.71	none	none
		5–55	7.7	890	bottom	47.17	5.74	12.16
				1,000	middle	48.26	none	none
				730	top	47.04	none	none
Carbon monoxide	47.26	2–25	7.6	880	bottom	46.21	trace	none
				990	middle	47.23	none	none
				720	top	47.14	none	none
Boston illuminating gas	47.26	8–0	4.7	615	middle	47.06	none	none
				435	top	47.15	none	none
		8–25	7.6	880	bottom	48.67	31.45	64.61
				990	middle	51.85	8.81	16.99
		9–10	8.0	930	bottom	50.65	8.84	17.45
				1,040	middle	51.93	30.02	57.81
				760	top	47.12	none	none
		9–0	3.1	340	bottom	.....	none	none
				405	middle	.....	none	none
Producer gas	68.25	7–0	7.6	880	bottom	72.96	31.26	42.84
				990	middle	73.64	20.79	28.24
				720	top	73.71	28.87	39.17
		8–0	5.0	560	bottom	67.80	10.58	15.61
				655	middle	67.35	14.66	21.45
				460	top	68.08	8.92	13.10

The Smelting of Copper Ores in the Electric Furnace.*

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(Butte Meeting, August, 1913.)

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I. Introduction.

In presenting this paper the writers wish to call attention first of all to the fact that the electric furnace was not developed as a competitor of the combustion furnace, but:

1. For the purpose of doing high-temperature work which it is not possible to do in the combustion furnace; and

2. For the treatment of ores from deposits which are located in regions where fuel is scarce and costly, and where hydro-electric power is comparatively cheap, as is the case in Chili, in Canada, in certain parts of this country and Mexico. This paper is not presented with the idea of trying to prove that the electric furnace should replace the reverberatory or the blast furnace as used at present in the smelting of copper ores, but that it may be substituted for them in those localities which are not favorable to them, and where either of the two following conditions may exist:

(a) A district remote from railway transportation facilities, and where there is a deposit, or deposits, containing sufficient values to warrant their being worked, but which, due to the cost of getting coke in for operating a smelter and of getting ore out to a smelter, have to remain unworked. And where, on the other hand, there is plenty of water power which can be developed at reasonable cost for the production of electric power, by the use of which for smelting the ores in an electric furnace, with subsequent Bessemerization if necessary, a concentrated product may be obtained, which can easily stand the necessary transportation charges.

(b) Or, comparatively speaking, the transportation facilities may be all right, but the cost of fuel be too great to permit of the ordinary methods of smelting, whereas, on the other hand, hydro-electric power can be developed in the district at a low cost, and the ores could be smelted by the electric furnace at a cost that would make their treatment possible.

With these possibilities in mind we have attempted to make a brief comparative study of the problem for the purpose of aiding those interested in the subject, in determining whether it would be possible metallurgically, and feasible commercially, to use the electric furnace in those localities where, by reason of excessive fuel costs, the cost of operating the furnaces for smelting copper ores is either excessive or prohibitive.

Although the smelting of copper ores in the electric furnace has received considerable attention and more or less experimental work has been done, so far as we are aware there are no electric furnaces in the United States which are being worked on copper ores. In Norway, however, trial smeltings of copper ores with an electric furnace of 1,000 h. p. and an estimated producing capacity of 2,000 tons per annum, have been

conducted at the Ilen Smelting Works, Trondhjem. A consignment of 25 tons of pure copper, the first copper ever produced by electric smelting in Norway, was exported from the Bitovaria Minea at Kaafjord, near Dyngor. Some months ago it was reported that, at Christiansand, the electric smelting and refining of nickel and copper would be undertaken by the A/s Kristianssands, Nikkelroffeneringsverk.

II. Chemistry of Copper Smelting.

Without going into details, the objects aimed at in copper smelting may be stated as follows:

1. To concentrate the copper and precious-metal values. If a native copper or an oxide ore is smelted, the concentrated product is in the form of a metal; if a sulphide ore is treated, the product is a matte.

2. To get rid of the excess iron and gangue materials as slag.

In general we may say that three classes or ores have to be dealt with in copper smelting, namely: 1. Native copper ores; 2. Oxide and carbonate ores; 3. Sulphide ores.

1. Native Copper Ores.

From the nature of occurrence of the valuable mineral content in the native state, the smelting of a native copper ore or concentrate resolves itself into a melting operation and separation of the metallic copper and gangue by the difference in specific gravity. After removal of the slag a further refining of the black copper from the melting operation is necessary. This is the simplest form of copper smelting. The electric furnace is particularly adapted metallurgically to a simple melting operation, in a neutral atmosphere, and appears to have particular application in this branch of copper smelting, especially for fine concentrates, as shown by recent experiments of the writers, to be described later.

2. Oxide and Carbonate Ores.

The chemistry of smelting of oxide, carbonate or silicate ores of copper consists simply of the use of coke as a fuel and a reducing agent in the blast furnace, to produce metallic copper. Here also the separation of the slag and metal is important, and there is need for careful regulation of the coke to prevent reduction of iron by any excess carbon. The atmosphere is reducing and not neutral. This also is a condition easily obtainable in the electric furnace, as is done to-day in electric iron

smelting. The experiments of Stephan, to be described later, show this possibility.

3. Sulphide Ores.

The smelting of sulphide ores presents further complications. In order to concentrate the copper and precious metal values in a matte, one must first eliminate as much of the sulphur of the ore as may be necessary to obtain the desired results, and must cause the resultant oxides to unite with silica, which may be present in the ore or which may be added, preferably in the form of siliceous ore.

a. Elimination of Sulphur.

As pointed out by Peters,[1] "when fused in a reducing atmosphere, certain of the sulphides, such as Cu_2S, FeS, PbS, etc., melt down without change, while the very common and important mineral, pyrite, loses about one-half of its sulphur, which is driven off as sulphur, in the shape of yellowish fumes, which will deposit a coating of brimstone on a cool surface." As also stated by Peters,[2] "The only two sulphides of much interest to the metallurgist, which lose a portion of their sulphur by heat alone in the manner just described, are: (1) pyrite (FeS_2), which is assumed to lose one-half of its contents in sulphur by mere heating, without air; and (2) chalcopyrite (Cu_2S, Fe_2S_3), which (as generally assumed by metallurgists) is composed of one-third copper, one-third iron, and one-third sulphur, and loses (by melting without air) one-third of its sulphur, or one-ninth of its entire weight."

This has been quoted from Peters in order to show clearly and concisely that there would be no object in simply melting down sulphide ores in an electric furnace. That is, if we did so, we could only get an altered form of the ore, somewhat concentrated, to be sure (to the extent of the loss of the elemental sulphur), but not to the desired extent.

Such being the case, the smelting of sulphide copper ores resolves itself into one of the three following methods, or a combination of them, in order to effect the object named above.

1. The mixing of oxides and sulphides in such proportions as to get the proper reaction between them to effect the degree of concentration that is desired.

2. The roasting of the sulphide ores, thus converting them into oxides, or partially so, depending upon the degree to which the roasting is carried out, and then melting down the roasted ore in a suitable furnace. In this way the excess iron and gangue materials present may be elim-

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[1] *Principles of Copper Smelting.* [2] *Loc. cit.*

inated, for sulphur has a greater affinity for copper than it has for iron, and any sulphur present in the charge during the melting down of the latter will combine first with the copper, and only the excess over this amount will unite with iron. The remainder of the iron will be present as iron oxide (FeO). This will unite with silica and form a slag, and thus the concentration of the copper and precious metals into a matte is brought about.

3. As the roasting of the ore necessitates an additional step in the process, it is to be avoided if possible, and the desired oxidation of the sulphides accomplished in the blast furnace by means of the oxygen forced into the furnace through the tuyeres. This is what the modern copper blast furnace does, namely, combines the functions of roasting and smelting. In other words, it oxidizes and smelts simultaneously, and also makes use of the heat that is generated by the oxidation of the sulphur and iron. The utilization of the heat received from the oxidation of the iron and sulphur in this manner is called pyrite smelting, although, as will be seen later, true pyrite smelting means the smelting of ores by heat arising solely from their own oxidation and without the addition of any carbonaceous fuel.

Having thus outlined briefly the objects aimed at and the method used in the smelting of copper ores, let us now turn our attention to the manner in which the smelting of copper ores is carried on.

III. Reverberatory Copper Smelting.

Inasmuch as copper ores are smelted in one of two ways, that is, either in a reverberatory or in a blast furnace, it will first be necessary to ascertain if the conditions essential for the successful carrying out of these two methods of smelting can be attained in the electric furnace.

As stated by Peters,[1] "the one fundamental and pre-eminent duty of this type of furnace is to produce a smelting temperature so that the ore may become liquid, and thus be able to separate into slag and matte." From the standpoint of this discussion, the reverberatory furnace may be said to be used for the smelting of ores and concentrates which cannot be successfully treated in a blast furnace.

"The manner in which the heat from the fuel is applied in this method of smelting, though much improved in modern practice, is a peculiarly wasteful one, as the mere rapid passing of a flame over the surface of a layer of ore resting upon a comparatively cool hearth, is a particularly incomplete way of transferring heat from the flame to the ore particles,

[1] *Principles of Copper Smelting.*

especially as the latter are usually poor conductors of heat. . . . The very large surface of walls, arch, flue, stack-lining, etc., is in just as favorable a position for being smelted as is the ore itself, and in spite of its refractory composition, needs frequent repairs and renewals. . . . Such a method could only originate in a district possessing cheap flaming coal, and inexpensive refractory materials, and Swansea (where the process originated) has these in abundance. . . . The reverberatory process is but little complicated by the two active extraneous agents which exert so marked an effect upon the chemistry of the blast furnace smelting of roasted ores, and the pyrite smelting of sulphide ores; namely, carbon in the one case, and oxygen in the other. . . . Speaking in a broad general sense, the ore in the reverberatory smelting furnace is subjected to neither of these influences to any marked degree, and is thus enabled to work out its own salvation under the influence of heat alone. Such chemical reactions as take place in the ore mass during the smelting result almost entirely from the behavior of substances already contained in the ore, and affected by the mutual action of the various constituents of the charge itself. This renders the chemical part of the operation unusually simple, and enables the smelter to concentrate his efforts upon developing the high temperature essential to the complete fusion of the ore."

IV. Substitution of the Electric Furnace for the Reverberatory Furnace.

If, then, as Peters states: "The proper function of the reverberatory furnace, in smelting copper ores, is to generate heat as rapidly as possible," there is no apparent reason why copper ores cannot be smelted equally as well in an electric furnace, and perhaps even more advantageously, for, as pointed out by Peters, and as is well known, the thermal efficiency of the average reverberatory furnace is only from 5 to 8 per cent., although about 20 per cent. is now obtained in the large Anaconda reverberatories, whereas, that of an electric furnace which would be used for smelting copper ores suitable to refractory smelting, would probably have an efficiency of as much as 70 per cent. Chemically the only ill effects that might possibly result from the use of such a furnace would be the effect of the carbon of the electrodes upon the iron oxides which might be contained in the charge and which would result in:

1. The loss of electrodes;
2. The production of metallic iron, which would contaminate the metallic copper and increase the cost of refining; or, if a matte was the final product, might necessitate re-concentration.

3. The loss of electric energy, due to the additional heat consumed by the reduction of iron.

Since the reverberatory furnace is essentially a means of melting, whether used for sulphide, oxidized or native ores, the results obtained in the electric smelting of native copper ores will serve for the sulphide and oxide ores also, for comparison with the reverberatory in order to clear up the three points referred to above. The slags would be somewhat similar in each case. The atmosphere of the electric smelting furnace charged with sulphide ore would not be as neutral as in the furnace charged with native copper ore, because of the tendency of the elemental sulphur and any sulphur dioxide formed to dilute the air so as to make the furnace practically reducing as far as the consumption of electrode by oxygen from the air was concerned. If the electric furnace was not kept closed, considerable sulphur dioxide might be formed, which would possibly be reduced in part by the carbon electrode. Even with such reduction of sulphur dioxide, the electrode consumption would probably be less when working on sulphide ores than when working on native copper ores. This is shown by Bureau of Mines experiments, when, with a charge of native copper concentrate, the maximum electrode consumption in fourteen experiments was 48.0 lb. of graphitized carbon per ton of charge, the minimum, 22.1 lb. and the average 34.5 lb. In the same furnace, with charges of sulphide, oxide and siliceous ores mixed, a matte being the product, the maximum electrode consumption for 20 runs was 60.0 lb., the minimum, 10.8 lb., and the average, 23.3 lb.

V. Smelting of Fine Michigan Native-copper Concentrates in the Electric Furnace

For the purpose of investigating the feasibility of smelting fine Michigan native-copper concentrates in the electric furnace, instead of the reverberatory furnace, experiments were conducted by the writers, the detailed results of which will appear later in a Bureau of Mines Technical Paper. The objects of these experiments were:

(1) To note the effect of variation of the three main slag constituents, silica, ferrous oxide and lime, upon the power consumption, the operation of the furnace and the purity of the copper produced;

(2) To ascertain the extent of iron oxide reduction caused by the graphite electrode;

(3) To compare intermittent smelting with continuous smelting.

Two grades of concentrates were smelted: No. 3 and No. 4, of which No. 3 was much the coarser; of the former, 69.73 per cent. passed through

a 60-mesh sieve, as compared with 88.12 per cent. of No. 4. The chemical analyses of the ore were as follows:

	No. 3 Per Cent.	No. 4 Per Cent.
Cu	37.35	25.35
SiO_2	33.33	35.92
FeO	15.40	21.60
Al_2O_3	8.78	12.40
CaO	5.18	5.98
MgO	1.09	1.09
S	0.056	0.096
Fe (metallic)	0.30	0.50

Fifteen experiments were performed in a Siemens type of electric furnace, having a capacity of 50 kilowatts. The furnace was operated as a resistance furnace. The charge and products of a typical experiment were the following:

Charge		Black Copper		Slag	
	Lb.		*Per Cent.*		*Per Cent.*
Concentrate No. 3	22.0	Cu	98.59 (by difference)	Cu	0.15
Hematite	1.1	Fe	1.10	SiO_2	44.88
		S	0.29	CaO	6.92
		As	0.02	MgO	2.32
				FaO	29.80
				Al_2O_3	15.34

1. Conclusions.

(1) Fine Michigan native-copper concentrates can be smelted in the electric furnace with the production of a good grade of black copper and without excessive losses of copper in the slag or by other means.

(2) The percentage of copper in the slag need not exceed 0.25 per cent., with a slag of proper composition. The loss in the slag should not exceed 0.50 per cent. of total copper charged. Other losses should not exceed 1.0 per cent.; giving a total loss of 1.5 per cent. of all the copper charged.

(3) The loss of copper by volatilization is high, if the slag is much more acid than a mono-silicate, and if it contains much alumina. The lowest losses were obtained by addition of limestone to form a slag containing 35 per cent. SiO_2, 22 per cent. CaO and MgO, and 25 per cent. FeO.

(4) The difference between limestone and hematite as a flux is not marked, one being about as efficient for this purpose as the other, if the slag has not too high a specific gravity. No more iron was found in the black copper product when hematite was used than when limestone was used.

(5) With a furnace operating at a low temperature on a mono-silicate slag, the reduction of iron by the carbon electrode should not be excessive or sufficient to result in a product containing less than 95 per cent. metallic copper. With care a product containing 98 per cent. copper could probably be produced regularly.

(6) The total electrode consumption greatly exceeds the amount of carbon necessary for reduction of the iron found in the metal. An average of 9.35 per cent. of the total was used in iron reduction. The electrode consumption should not exceed 10 lb. per ton of charge.

(7) The electric furnace operates satisfactorily with the high siliceous alumina slags, produced by the natural gangue without flux, but the losses by volatilization are high because of the high melting point of the slag. There is no copper oxide in the product, as was shown by microscopic examination.

(8) The power consumption for smelting fine Michigan native copper concentrates of between 25 and 40 per cent. copper in an electric furnace of 750 kw. or greater capacity should not exceed 640 kw. hr. per ton of ore smelted.

VI.—Proposed Process for Electric Smelting of Fine Native-copper Concentrates.

The commercial smelting of fine native copper concentrates in the electric furnace to attain the greatest economic success would be carried out in a continuous operation, consisting of charging the mixture of ore and flux at intervals into the top of an electric furnace having a short stack above the smelting crucible for preheating the charge before it reached the smelting zone, and tapping off the metal at intervals, the slag being permitted to run continuously from the furnace through a slag notch above the metal hole. The possibility of operation in this manner was shown in an experiment where there was an average of 1.10 per cent. iron in the product and 1.37 per cent. copper in the slag. The total loss of copper was as low as in the intermittent runs, and was largely due to the small scale operation. The slag loss was high because of imperfect settling, and because of the tapping of partially fluid material with the slag; a condition that would not occur in a large furnace operated properly. The power consumption was lower than in the other experiments.

The essential conditions for the successful performance of this process are a crucible, where the electrical energy is introduced and where the metal settles from the slag, and a short shaft above the crucible for preheating the charge before it reaches the smelting zone. As there is no reduction to be performed, this shaft need not be higher than to give plenty

of head room between the top of the crucible and the charging floor for the moving of the electrode.

A furnace of the type shown in Figs. 1 and 2 satisfies these conditions. It consists of a smelting chamber lined with fire brick or silica brick, having three carbon electrodes suspended vertically through the roof with a

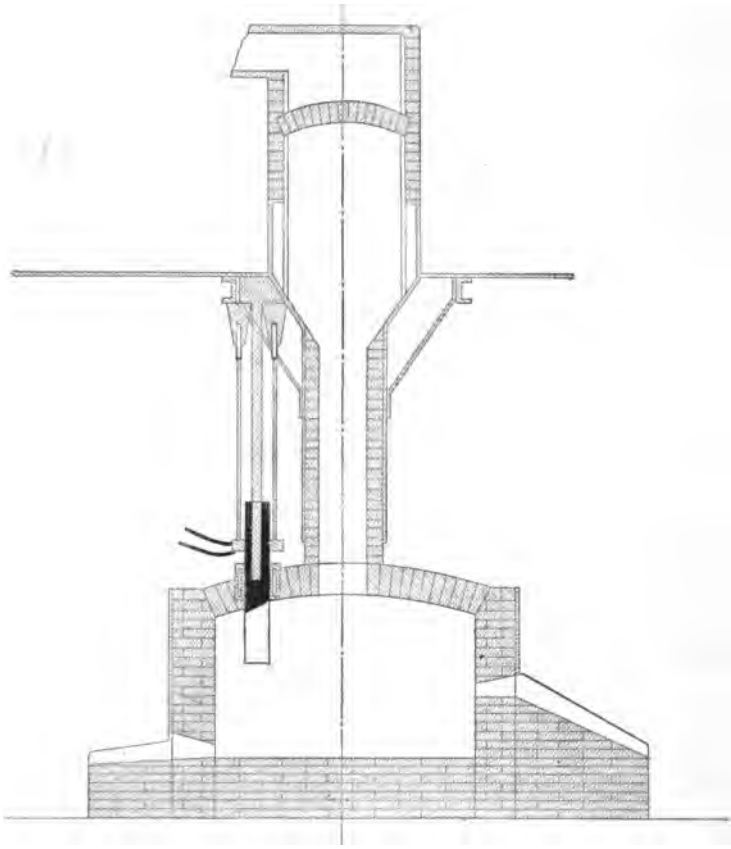

FIG. 1.—ELEVATION OF ELECTRIC FURNACE FOR SMELTING NATIVE COPPER CONCENTRATE.

feeding shaft lined with fire brick over the crucible. A 750-kw. furnace would treat, per 24 hr., about 23 tons of native copper concentrate, containing 25 to 40 per cent. copper. This furnace would be operated with three-phase current at from 50 to 100 volts. The general internal dimensions of the crucible are 14 ft. 6 in. in diameter, by 9 ft. high, with a shaft 1 ft. 6 in. in diameter by 18 ft. 6 in. high. With a furnace of this type regulation of electrodes would not be necessary after regular operation was established, so that the electrical connection is made close

to the roof of the furnace and regulation is by hand. There is less difficulty in handling the electrodes if they are suspended vertically; and, as the shaft is low, it is made vertical, thus eliminating the necessity of suspending them at about 55 degrees.

The slag from such a furnace should not contain over 0.25 per cent. copper, and could be discarded at once without further treatment. It could be granulated with water as discharged from the furnace. The metal would require further refining for elimination of iron and other impurities, which could be done by charging the hot metal directly into a reverberatory furnace, where it would be refined as previously described.

Briefly, the present process for treating these concentrates consists of the following steps: first melting and refining the coarse concentrate

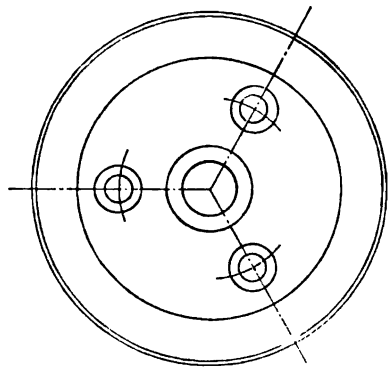

FIG. 2.—PLAN OF ELECTRIC FURNACE FOR SMELTING NATIVE COPPER CONCENTRATE.

in either one or two reverberatory furnaces, from which a slag is produced containing from 10 to 30 per cent. copper; second, agglomerating the fine concentrate in a reverberatory furnace, or briquetting it with lime; third, smelting the slags of the reverberatory furnaces and the agglomerated or briquetted fine material, with coke in a blast furnace, the slags from which contain 0.6 to 0.8 per cent. copper and the metal, 90 to 95 per cent. copper; and fourth, retreatment of this metal in a reverberatory furnace.

The use of the electric furnace for treatment of these concentrates gives the following advantages: first, no agglomeration or briquetting would be necessary; second, no slags produced by this furnace would require retreatment; third, a large proportion of the higher-grade concentrates, containing 75 per cent. copper, could be mixed with the fine concentrates and treated directly in the electric furnace, thus reducing the amount of slag necessary for re-smelting; and fourth, if cheap power was available, the electric furnace could be used for reducing the copper from the reverberatory slags, and the resulting metal would probably be lower in iron

and the slag lower in copper, because of the greater ease of regulating the reducing atmosphere, as only sufficient coke is present for the reduction of combined copper.

The limit to which the higher-grade concentrates could be mixed with the fine material would be determined by the electric conductivity of the mixture in the furnace. In the experiments performed with a charge containing 37 per cent. metallic copper, no difficulty from short circuits or other causes was experienced. The point to which the amount of copper in the charge could be increased would have to be ascertained by experiment.

Technically the electric furnace does away with the necessity of briquetting or nodulizing, and, in general, facilitates handling the materials. The commercial factors influencing the cost would largely depend upon local conditions as to cost of power and fuel. Opposed to the cost of power would be the cost of fuel and briquetting or nodulizing, with possibly a higher labor cost for the present practice. If the cost of power was equal to these three factors, and no less, the electric furnace might still have the advantage of facility of operation. Also, in the case of a mine remote from smelting plants, it might be cheaper to concentrate the low-grade product of wet concentration by smelting in a small electric furnace, and thus reduce the cost of freight. Finally, to save losses of wet concentration, the ore might not be concentrated to so high a degree in this manner, but a low-grade concentrate made, which would be smelted directly in the electric furnace and the high-grade black copper shipped to the custom smelter.

We have obtained figures from a reliable source showing that the cost of treatment of these fine concentrates by agglomeration in a reverberatory furnace, refining of this copper produced, retreatment of slags in the blast furnace, and refining and casting the cupola copper into marketable shapes, is $8.64 per ton of 35 per cent. native copper concentrate. This includes overhead charges, all repairs and maintenance of plant, but not amortization.

The treatment of the same material by the proposed electric furnace process to produce marketable shapes is estimated as follows:

Cost of Electric Power		Cost of Treatment Per Ton of Concentrate
Per Kilowatt Hour	Per Kilowatt Year	
0.5	$43.80	$7.18
0.625	54.80	8.08
0.75	65.80	8.87

A rate of one-half cent per kilowatt hour with a high load factor is possible in many districts with the use of steam turbine plant, if cheap hydro-electric power is not available. The load factor in this case would be above 90 per cent., and the power factor not below 0.85. In practice the cost of refining "cupola" block of the electric furnace would probably be lower than in the case of blast furnace cupola blocks, because of the higher percentage of copper in it. In the reverberatory process, 81.3 per cent. of the material charged must be retreated as slag in the blast furnace. In the electric furnace process, the only material to be retreated would be about 5 per cent., as slag from the refining of the cupola blocks in the reverberatory. This slag could be retreated in the electric furnace. The great saving in retreatment charges accounts for the possibility of use of the electric furnace in this connection, even with expensive electric energy.

VII.—Ordinary Copper Blast Furnace Smelting.

Ordinary copper blast furnace smelting, that is, smelting the ore with carbonaceous fuel, differs widely from pyritic smelting, which consists in smelting the ore with heat derived from the oxidation of its own sulphur and iron contents.

As stated by Peters[1], "The main characteristic of ordinary blast furnace smelting is the employment of carbonaceous fuel, mixed with ore, as the principal source of heat." Such being the case, the problem that presents itself is to determine whether it is metallurgically feasible to substitute electricity for carbonaceous fuel as a source of heat, and whether in so doing, we will obtain as favorable results chemically as it is possible to obtain in ordinary blast furnace smelting.

1. Prerequisites.

In the smelting of copper ore, sulphide ores in the main have to be dealt with, and the problem consists in getting rid of the sulphides other than copper, and of concentrating the latter into a matte containing anywhere from 20 to 60 per cent. copper. The first step is, therefore, to free the ore from its sulphur. We will first consider how this is done in ordinary copper blast furnace smelting. As pointed out by Peters[2], due to the constant presence of a large amount of glowing carbon in the furnace shaft, extending from the tuyeres for several feet up the shaft of the furnace, and often to the charging door itself, the oxygen of the air blown in through the tuyeres is consumed with extraordinary rapidity and thoroughness, and there is consequently but a poor chance for the oxidation of any substances in the ore, which possess less affinity for oxygen than carbon does.

[1] *Principles of Copper Smelting.* [2] *Loc. cit.*, p. 109.

"Therefore, in ordinary blast furnace smelting with carbonaceous fuel, the sulphides of iron and copper, and such small quantities of other metallic sulphides as may be present, stand little chance of obtaining any of the oxygen to combine with, to form sulphur dioxide and metal oxide. The atmosphere is almost always strongly reducing, and the sulphides tend to melt down with almost the same result that they would in a closed crucible, with complete exclusion of air. . . . It will thus be seen that, owing to the reducing atmosphere which prevails, we can, when smelting sulphide ores, look for the removal of but little sulphur in the blast furnace beyond the portion that will be driven off by heat alone without the presence of oxygen."

Of the two sulphides in the charge which have to be taken into consideration in this connection (pyrite, FeS_2, and chalcopyrite, Cu_2S, Fe_2S_3), the former loses one-half of its contents in sulphur by mere heating, without air, while the latter, similarly heated, loses one-third of its sulphur and one-ninth of its entire weight.

Continuing, Peters states, "It is obvious, then, that there would be little object in smelting unroasted ores carrying a high percentage of metallic sulphides in a blast furnace running on carbonaceous fuel."

From what has been stated, it would appear that electric heat could be used as well as heat derived from the combustion of coke for the melting down of the ore. However, such melting would not, in either case, give us the concentration we desire, and so we next turn our attention to that phase of the problem, namely, the concentration of the copper and accompanying precious metal values into a matte, by the removal of sulphur and the subsequent oxidation of the iron.

The removal of sulphur in ordinary blast furnace smelting is brought about in two ways: (1), by heat alone, which drives off a part of sulphur as elemental sulphur, and (2), from the reaction of oxides and sulphates present in the ore as such, upon the sulphides. The presence of oxides in the charge may be due to the use of oxidized ores, while, if roasted ores are used, both oxides and sulphates may be present. These compounds react with the sulphides present, a partial oxidation of the latter takes place, and some of the sulphur is removed as sulphur dioxide (SO_2). It is to be remembered, that the atmosphere of the ordinary blast furnace is reducing and hence these reactions in this instance have to take place in a reducing atmosphere, and for this reason do not take place as freely as they would if in a neutral atmosphere. In general, it may be stated that, with no oxides present in the ore charge, practically no sulphur (aside from the elemental sulphur removed by heat, as above stated), would be driven off, except, as stated by Peters, "the small amount that would be oxidized by the blast, in spite of the generally reducing atmosphere of the furnace." He also states, judging from his

own experience, that this "loss of extra sulphur in the blast furnace cannot be placed higher than 5 per cent. of the sulphur remaining in the ore after deducting the sulphur which the pyrite and chalcopyrite lose by direct volatilization."

Summarized then, we may say that the ordinary blast furnace smelting of copper ores consists simply in a melting-down process, and the sulphur removed is that which is removed when a sulphide is heated without air, and that any concentration that takes place is due to the reactions which take place between the oxides and sulphates present, and which would take place better in a neutral atmosphere than they do in a blast furnace, the atmosphere of which is reducing. Such being the case, a blast furnace operating in this manner is only a melting-down furnace and gives the same results which would be obtained by melting such an ore charge in an open crucible. The only advantage which the blast furnace has in this instance over the reverberatory furnace is that it is more efficient as a melting furnace, because it affords intimate contact between the fuel of the charge and the resultant gases, and permits a continuous operation; whereas, the reactions between the oxides and sulphates do not take place as readily as they would in the comparatively neutral atmosphere of the reverberatory furnace, or of the still more neutral atmosphere of the electric furnace. So far then, aside from the question of costs, there is apparently no reason why the electric furnace could not be substituted for the ordinary blast furnace when operated on the class of ores mentioned above.

2. Losses.

Let us now turn our attention to the subject of the losses which occur in ordinary blast furnace work, and, as far as possible, determine their causes. These losses are, according to Peters:

1. The loss of small particles, in the form of dust, which contain valuable metals, and which loss is brought about every time the ore is moved.
2. Flue dust losses, due to blast.
3. Volatilization losses.
4. Losses in slag due to "particles of matte or metal which have failed to separate properly from the slag, and to a lesser degree from the oxidation of the oxides of the valuable metals—especially lead and copper—that have entered into combination with the silica, and have thus become chemically part of the slag."

Would these same losses occur if an electric furnace (with no blast) were used instead of a blast furnace, operated in the manner above described?

Loss 1. This would be common, of course, to both the blast furnace and the electric furnace.

Loss 2. Although there would be more or less fine particles carried away as dust by the gases escaping from the electric furnace, the amount of the same, of course, would not be as great as would be the case with the blast furnace.

Loss 3. An electric furnace used for this purpose would be operated more in the man-

ner of a resistance furnace (the charge acting as the resistor), than as an arc furnace, and hence the heat adjacent to the electrodes would not be as great as would be the case if an arc were used, and probably not greatly in excess of that which is generated at the tuyeres of such a blast furnace as we have been considering.

If an electric furnace of the type shown in Fig. 2 be used, the volatilization would take place at the base of the shaft of the furnace, and hence the volatilized metals would have a chance to be deposited on the cold part of the charge in the upper part of the stack, and, as it would not be necessary to use a blast when operating the electric furnace for the purpose we have been discussing (as the blast of the blast furnace in this particular instance is used only for furnishing air for burning the coke to produce the heat necessary for the process), the volatilization loss should not, and probably would not, be as great as in the case of the blast furnace.

Loss 4. As to the losses in the slag due to particles of matte or metal, which have failed to separate properly from the slag, such losses would occur just the same if an electric furnace were substituted for the blast furnace, but there is no reason to believe that they would be any greater and on the other hand they should not be as large, for, due to the fact that it is possible to so control an electric furnace as to obtain the temperature desired, a sufficiently high temperature could be given to the matte and slag before it left the furnace to insure its being fluid enough to cause perfect separation in the settler, which would thus obviate this loss. As to the oxidation of valuable metals, this loss should be reduced to a minimum in a furnace operation without a blast, for the loss by oxidation is doubtless due to the action of the blast upon the products of the furnace as they pass the region of the tuyeres.

Having thus made a brief comparative study of the possibility of using an electric furnace for the smelting of mixed sulphide and oxide ores (it being understood, of course, as was stated at the beginning of this paper, that we are not trying to prove that the electric furnace should replace combustion furnaces, but to determine, if possible, whether it can be substituted for the same in those localities which are not favorable to the latter), it may be well at this point to consider some of the experimental work which has been done along the lines above indicated.

VIII. Experimental Data on the Electric Smelting of Raw Sulphide, Oxidized and Carbonate Ores.

1. Vattier's Experiment.

One of the best known of the experiments which have been conducted upon the smelting of copper ores in the electric furnace is that which was made by Vattier at the Livet works of the Compagnie Electrothermic Keller et Leleux, France, April 23. 1903.[1] These tests were made by Vat-

[1] For a full account of this work see Report of the Canadian Commission, 1904.

tier for the Chilean government, which wished to secure such data as it would be possible to obtain in making experiments on a commercial scale. These tests were made before a Commission, which was composed of some of the best known metallurgists of England and Europe.

As stated by Vattier, the main object of these experiments was to determine, if possible, whether electric heat could be substituted for the heat derived from coal, either wholly or in part. This is especially important to Chile, for it has plenty of valuable ores, and, so it is stated, an abundance of water power, but coal is costly.

The experiments were conducted on two different kinds of ore:

1. An ore the copper contents of which was approximately 7 per cent., present principally as copper pyrites. It also contained 8 to 9 per cent. of sulphur, and the gangue was made up mainly of micaceous copper oxide, together with silicates and a little carbonate of lime.

2. Low-grade copper ore from the mining regions in the vicinity of Santiago, Chile, mixed with a small proportion of manganese and lime.

The composition of the ore, matte and slag was as follows:

	Ore Per Cent.	Matte Per Cent.	Slag Per Cent.
SiO_2	23.700	0.80	27.20
Al_2O_3	4.000	0.50	5.20
CaO	7.300	.....	9.90
MgO	0.330	1.40	0.39
CO_2	4.310	.....	.....
S	4.125	22.96	0.57
Fe	28.500	24.30	32.50
Mn	7.640	.....	8.23
P	0.046	.....	0.06
Cu	5.100	47.90	0.10
As	Trace	.....	.....

2. *Wolkoff's Experiments.*

In some experiments that were made by Wolkoff[1] on the smelting of copper ores, the ore experimented with was a sulphide, containing 8.20 per cent. of copper, with an acid gangue. Several experimental runs were made, but only the results from one or two of them will be mentioned in this connection. For instance, using 12 kg. of ore (25.5 lb.) and with the addition of 6.2 per cent. of hammer scale to furnish iron oxide for the siliceous gangue, he obtained a thoroughly fused product, the matte containing practically the whole of the copper, the slag retaining only 0.15 per cent. of the same, and he also states that the volatilization losses were very light.

In another experiment, he smelted 10 kg. of ore (22 lb.) with 1.25 kg.

[1] *Metallurgie*, Vol. VII, pp. 99 to 102.

(2.74 lb.) of roasted matte, which contained 75 per cent. CuO, 8 per cent. Cu_2S, and 15 per cent. Fe_2O_3, the object being to determine the extent of the reaction, $Cu_2S+2CuO=2Cu_2+SO_2$. On smelting this charge with a current of 400 amperes and 75 volts for half an hour, Wolkoff obtained 1.78 kg. of crude copper with 92 per cent. Cu, 3 per cent. Fe, and 1 per cent S. He also obtained 10.9 kg. slag with 0.10 per cent. Cu. In this experiment 96 per cent. of the metal was extracted; the slag contained 0.6 per cent. of the total quantity of copper; therefore, as stated by Wolkoff, the copper balance is as follows:

The charge contained		1.706 kg. Cu
1.78 kg. crude copper with 92 per cent. Cu =	1.638 kg	
10.9 kg. slag with 0.1 per cent. Cu =	0.011 kg.	
Losses	0.057 kg.	
		1.706 kg. Cu.

3. Experiments of M. Stephan.

At the first general meeting of the recently formed "German Metallurgical-Engineers," M. Stephan, Superintendent of the Girod Electric Steel Works at Ugine, France, gave an account of some experiments made by him on the electric smelting of non-ferrous ores, one of which was copper. The following is an abstract of Stephan's paper:[1]

"The ore came from the Belgian Congo, being mined by a Belgian-English concern. In five different analyses given, the CuO varies from 21.01 per cent. to 5.73 per cent.; SiO_2 from 28.48 per cent. to 78.55 per cent., with from 4 to 13 per cent. Al_2O_3; from 4 to 16 per cent. Fe_2O_3, and, besides smaller amounts of impurities, from 2 to 7 per cent. CoO; no nickel. . . . The moisture in the ore varied from 7 per cent. to 32 per cent. It was not removed, in order to meet the conditions of practical operation and to carry the experiments out under most unfavorable conditions. . . . Charcoal, coke and anthracite were used successfully as reducing agents. Charcoal would probably be the cheapest in this particular instance. . . . Electric furnaces, similar to the Girod ferro-alloy furnace type, were used and the dimensions changed in several runs within wide limits, in order to secure enough data for the construction of a large furnace for the same work. Electrodes were used suspended from the top and inserted in the bottom, also in the sides for heating by radiation, and a system was adopted of heating with a smothered arc mainly by the resistance of a thick layer of slag over the conducting metallic charge. The temperatures were measured with LeChatelier, Wanner and Fery pyrometers.

[1] Einiges Über die Erzeugung von Metallen in elektrischen Ofen, von M. Stephan, *Metall. und Erz*, Oct., 1913; abstract in *Metallurgical and Chemical Engineering*, p. 22 (Jan., 1913).

"A slag of the composition—

SiO_2	Al_2O_3	CaO	MgO	Fe_2O_3	MnO	CuO	CoO
51.90	11.31	16.83	13.71	3.55	0.94	0.46	0.87

begins to melt at 1250° C.; begins to flow in a pasty condition at 1350° C., and is liquid enough at 1400° to allow the little copper balls to settle completely. At 1550° C. the slag is liquid enough to flow off freely; 1920° C. were necessary to render the slag liquid when the highly acid ore was smelted without any fluxes. The pig copper (*Schwarzkupfer*) produced analyzed in six different runs from 65 per cent. to 95 per cent. Cu, containing from 1 to 21 per cent. Fe, and from 1 to 11 per cent. Co. The purity of the product will be higher the lower the smelting temperature. The lower the temperature the smaller will be the chance for reducing any of its impurities, but, at the same time, more copper will also be retained in the slag, so that the output is decreased."

A.—*Conclusions of Stephan.*

"It will be a matter of commercial calculation, according to the composition of the ore and other special conditions, whether a high-quality product or a maximum output will be desirable. . . . A continuous run for several days with the same ore, aiming at slags of about the composition mentioned above, required 1000 to 1200 kw.-hr. per ton (2205 lb.) of ore. . . . These figures are high, as a result of the amount of heat required to keep the very viscous and abnormal slags in liquid condition. With an easily fluxible ore, the specific power consumption was only about 500 kw.-hr. The electrode consumption averaged 8 kg., or 17.6 lb., per ton of ore. It was operated with 4 amperes per sq. cm. (25.8 amperes per sq. in.). Coal for reduction was used at the rate of 25 per cent. of the copper in the charge. The best lining to withstand the severe conditions of the furnace was tamped from fire-clay with 80 per cent. SiO_2 and 15 per cent. Al_2O_3."

IX.—Smelting of Sulphide Ores in the Electric Furnace.

To study conditions in the smelting of sulphide ores in the electric furnace, the writers have recently conducted a series of experiments at the Bureau of Mines Laboratory. The objects in view were: (1) to determine if there are any conditions arising which make electric smelting without air anything but a simple melting operation; (2) to note the percentage of concentration and the sulphur removal; (3) to study the possibility of condensation of the elemental sulphur as such; (4) to get

general figures on power consumption with varying charges; (5) to determine gold, silver and copper losses, and (6), to study the use of a low-copper matte as a collecting agent for gold and silver.

1. Ores Treated.

The ores used in the experiments were: A heavy sulphide low-grade copper ore; a gold; a silver, siliceous ore, and some roasted ore of the analyses shown below. The limestone contained 63.2 per cent. CuO and 5.7 per cent. MgO.

	Pyrite Per Cent.	Nodule Per Cent.	Siliceous Ore Per Cent.
SiO_2	2.50	4.00	74.53
Fe	44.07	65.60	10.71
S	48.20	.....	8.36
Al_2O_3	0.24	1.76	0.40
CaO	0.08	0.45	.....
MgO	0.73	0.46	0.26
P	0.02	.....	.....
As	0.20	.....	.....
Cu	1.30	0.07	.....
Au	0.01 oz per ton	0.03 oz. per ton	1.28 oz. per ton
Ag	0.14 oz per ton	0.05 oz per ton	3.20 oz. per ton

The ores were treated in the electric furnace described under the native copper experiments. The top was roofed and kept closed tightly to prevent escape of sulphur and admission of air. A condenser, consisting simply of three rectangular, horizontal chambers with several baffles in it, was attached to the furnace to condense the sulphur and catch any escaping dust.

Twenty experiments were made with these ores. A typical charge was as follows:

Charge		Matte		Slag	
	Pounds		*Per Cent.*		*Per Cent.*
Pyrite	8.8	Cu	1.22	Cu	0.05
Nodule	13.2	Fe	64.18	SO_2	35.25
Siliceous ore	5.76	S	22.38	FeO	41.30
Limestone	2.73		*Oz. per Ton*	Al_2O_3	4.40
		Au	0.72	CaO	10.00
		Ag	0.96	MgO	1.64
				S	3.66
					Oz. per Ton
				Au	0.03
				Ag	0.32

2. *Conclusions.*

(1) Smelting in an electric furnace treating sulphide ores consists simply of melting the charge, volatilizing about 60 per cent. of the sulphur as elemental sulphur, and separating the slag and matte.

(2) The ratio of concentration is simply that possible from a separation of matte and slag, together with a reaction of oxides and sulphates upon sulphides, and the volatilization of 10 per cent. more than one atom of sulphur, but with no formation of iron oxide to enter the slag, because there is nothing present to oxidize the iron sulphide.

(3) Qualitatively, it is possible to condense some of the elemental sulphur driven off.

(4) The loss of copper, by volatilization and in the slag, is low.

(5) A matte containing about 1 per cent. copper, from a charge containing 0.30 per cent. copper, makes a good collecting agent for gold and silver, if a clean separation of slag and matte is obtained.

(6) There is very little loss by volatilization of gold in the electric furnace, but there is some loss of silver in this way. This would not probably occur in a larger furnace with closer temperature regulation.

(7) The electrode consumption in the smelting of a sulphide ore is low, and in practice need not exceed five pounds per ton of charge.

(8) In a large commercial furnace the power consumption for most ores would be about 480 kilowatt-hours per ton of ore, or 0.055 kilowatt-years.

X.—Electric Furnace vs. the Reverberatory Furnace and Blast Furnace as a Melting Agent.

So far, we have only considered the use of the electric furnace as a melting furnace in the treatment of copper ores. From what has been said, we believe that we are justified in making the following statements as to the possibility of using the electric furnace for this purpose:

(1) The melting of native copper oxide or sulphide ore of copper can be done just as efficiently in the electric furnace, and perhaps more so, than in either the reverberatory furnace or the blast furnace.

(2) The reactions desired in reverberatory smelting, or ordinary blast furnace smelting, can be obtained in the electric furnace as well, and perhaps better, than in either of those furnaces.

(3) The loss of electrodes is small, varying from 5 to 10 lb. per ton of ore smelted, and the presence of the carbon electrode does not cause enough reduction of iron to be troublesome, or increase the consumption of electrical energy appreciably.

(4) The losses of copper, gold and silver by volatilization and in the slag would be no greater, as shown in all the experimental work cited,

than they are in reverberatory smelting or ordinary blast furnace smelting.

(5) A matte containing as low as 1 per cent. copper can be used as collecting agent for gold and silver in the electric furnace, as well as in combustion furnaces.

(6) The comparison of costs would depend entirely upon the nature of the ore treated, and the relative cost of coal or coke, and electrical energy. In general, from the work done in the experiments, we may say that from 500 to 700 kilowatt-hours per ton charged would be required for smelting copper ore, depending upon the nature of the ore.

XI.—Smelting of Sulphide Ores without Roasting, or Partial Pyritic Smelting.

In discussing the smelting of copper ores by ordinary blast furnace smelting, we noted the fact that the process consists essentially in melting down the charge, and that, during the melting, certain reactions take place between the constituents of the charge, which bring about a concentration of the copper and precious-metal values (if the latter are present) into a matte, and the excess iron and gangue material into a slag; that, in this method of smelting, the heat necessary for carrying out the process was obtained by the combustion of coke at the tuyeres, and that the blast used was simply for the purpose of supplying the necessary oxygen for combustion. In other words, the oxygen entering the tuyeres plays no part in the reactions which take place in the furnace, so far as the constituents of the charge other than the coke, are concerned. Such being the case, the process is simply a melting down process and, so far as the reactions of the process are concerned, the melting down could just as well be done in an electric furnace as in a blast furnace. Now, however, we turn our attention to those processes of copper smelting where the air entering at the tuyeres not only furnishes the oxygen necessary for the producing heat, but also for bringing about the necessary reactions which take place during the progress of the process, and which result in the production of a matte and a slag, as in ordinary blast furnace smelting. In this connection it is to be remembered that no matter what process we employ in obtaining metallic copper (as a final product) from a sulphide, the three essentials are:

(1) The removal of sulphur.

(2) The oxidation of the iron with which the sulphur was combined.

(3) The removal of this iron oxide by causing it to unite with silica to form a slag.

Hence, as stated by Peters, "the first step in the smelting of a sulphide ore of copper must be to oxidize it." In the process which we are now

to consider, the object is to bring about the required degree of oxidation and the melting, at practically the same time, and to obtain either a whole or a part of the heat required for the process from the combustion of the iron and sulphur contained in the ore. As is well known, if practically all the heat necessary for the process is obtained in this manner, we have what is known as pyrite smelting. If only a part, semi-pyrite smelting. However, Peters states that so far as he is aware, in all pyrite furnaces of the world, a small amount of fuel (usually coke) is added to the charge, and "this amount may vary anywhere from 0.5 per cent. of the weight of the charge up to a proportion that might be actually enough to melt the ore without any heat at all being derived from the sulphides." The question, therefore, that presents itself in connection with the smelting of copper ores in the electric furnace is to determine whether electric heat may be used to replace the heat which is derived from the combustion of coke in pyrite and semi-pyrite processes. In other words, whether it would be possible, and if so, whether it would be commercially feasible, to carry out these processes in an electric furnace so constructed as to use a blast, thus obtaining as much heat as possible from the oxidation of the sulphur and iron, and supplying by electric energy whatever additional heat might be needed to carry out the process successfully.

To begin with, let us assume that our furnace is similar in construction to a modern copper blast furnace, and that part of the furnace, including the tuyeres, is practically identical with the same. Below the tuyeres, the furnace could be constructed as shown in Fig. 3. By referring to this, it will be noted that there are electrodes extending down into the crucible, the arrangement of which, along the sides of the crucible, can be noted by referring to the plan of the furnace in Fig. 4. Let us now briefly consider what would happen if we attempted to smelt a charge which would be suited to "partial pyrite smelting," that is, one in which so much coke would have to be used that "its influence upon the oxidizing power of the focus begins to be plainly discernible," which, as stated by Peters, is "the division line between true and partial pyrite smelting." Inasmuch as no coke would be used in a charge smelted in an electric blast furnace, the point to be determined is how the charge would act when such a furnace was used, and if the desired results could be obtained, as before stated, in a feasible and economical manner.

The object aimed at in operating a copper blast furnace, when working under partial pyrite conditions, is to supply enough blast to completely oxidize the coke, and also to oxidize as much of the iron sulphide for slag forming purposes as can be spared from the matte. If, therefore, there were no coke to be oxidized, only enough air would be forced through the tuyeres to effect the required degree of oxidation of sulphur and iron.

In general, we may assume, as does Peters, that a partial pyrite furnace

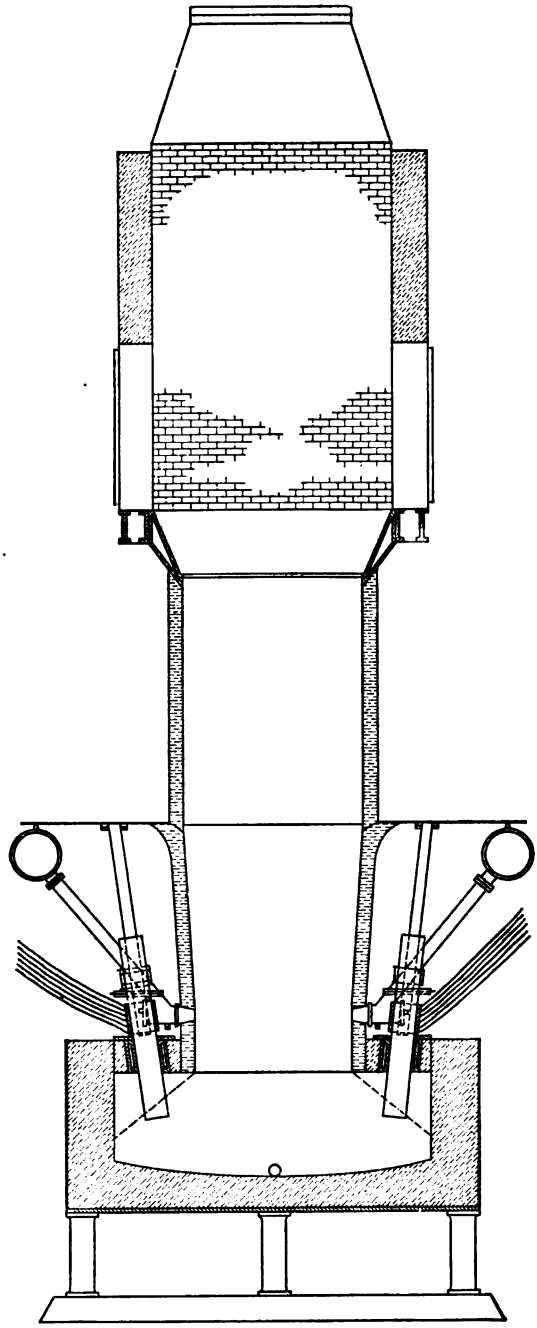

FIG. 3.—ELEVATION OF ELECTRIC BLAST FURNACE.

is "provided with but a scanty supply of pyrite," and that it produces a slag "rather high in silica and earths," and, "consequently would require a considerable amount of coke." In the electric blast furnace, as before stated, we would not use coke, but electricity instead for obtaining the equivalent heat value of the coke. If, therefore, we supply our electric blast furnace with a charge, such as indicated above, that is, a charge such as could be successfully treated in a partial pyrite furnace, what differences may we expect in the behavior of the charge as compared to

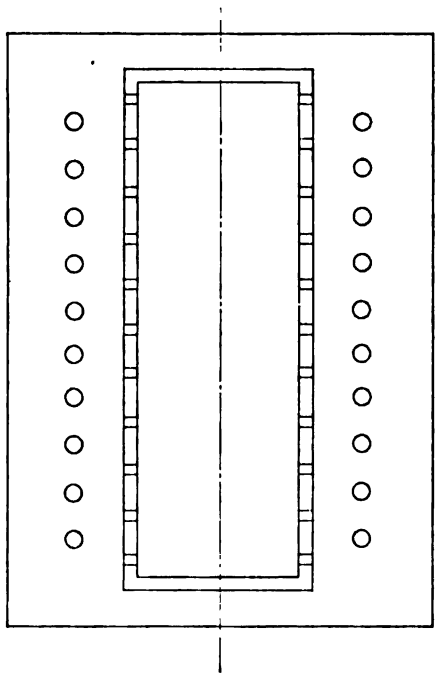

FIG. 4.—PLAN OF ELECTRIC BLAST FURNACE

the behavior of the similar charge when smelted in a copper blast furnace with coke? In making the comparison, let us first study the behavior and effect of the coke itself. Again quoting from Peters[1], he states that "the amount of coke present carries the melting process high up above the proper zone of oxidation, and to regions where there is yet no formation of FeO. Half-fused masses of acid, earthy silicates are formed, and much—in some cases, all—of the free silica is combined with the alumina, lime, magnesia, manganese, alkalies, and already oxidized iron, all of which substances are likely to be present in the ore mixture. It is not that the affinity of the silica is satisfied in forming these preliminary,

[1] *Principles of Copper Smelting*, p. 333.

temporary, refractory acid silicates; but the edge of its appetite is blunted, and the eventual formation of ferrous silicate seems to proceed somewhat sluggishly, even when ample air is blown into the furnace.

"As may be imagined, the main endeavor of the metallurgist in this type of smelting is to keep the proportion of coke to ore as low as possible; not merely because coke costs money, but still more because any excess of coke causes a lowering in the grade of the matte, due to its interference with the oxidation of the sulphides. A furnace in proper condition, and running on a suitable charge, is extraordinarily sensitive on this point. The increase of the coke from a standard charge of 60 lb. per 2,000 lb. of ore up to 65 lb. per 2,000 lb. of ore may be followed—as has come under my personal observation—by a dropping of the grade of matte from 35 per cent. copper, and a raising of the silica contents of the slag from 41 per cent. This results, of course, from the consumption by the new coke of a certain amount of oxygen, which previously had been employed in burning iron sulphide. Under this new condition, this iron sulphide entered the matte, at the same time robbing the slag of just so much FeO."

If now we smelt the charge in the electric furnace without coke, and with just enough blast to perform the necessary oxidation of the sulphur and iron, there would doubtless be no "half-fused masses of acid, earthy silicates formed," etc., as stated by Peters, and since, as shown by him, the control of the coke is a matter of great importance, due to the "extraordinary sensitiveness of the furnace on this point," and its resultant great irregularities, it would seem that it would be a great advantage if possible, to do away with coke. If so, and the smelting be done in some such furnace as shown in Fig. 3, we would expect the charge to enter the tuyere zone of the furnace practically unaltered, except, perhaps, for loss of elemental sulphur. Inasmuch as the sulphides are easily melted (950° C.) we may assume that there is enough heat present, from the oxidation of the sulphur and iron, and radiated from the crucible, to cause the iron sulphide to melt and to become oxidized to FeO, and then be instantly seized by SiO_2 to form a slag. However, at this temperature, the result might be the formation of a silicate, high in SiO_2, because, at low temperatures, the saturation point of silica for iron is low, and which, perhaps, would at once freeze, so to speak, and descend into the crucible and be gradually melted in the neighborhood of the electric current. In other words, we would have formed above the tuyere zone the artificial boshes, observed by Sticht, Freeland and others, and described by Peters, thus contracting the smelting and slag forming area "to a comparatively narrow opening, running along the middle of the furnace shaft." Sticht states that these boshes "are the contact lines between the active combustion zone and the relatively dead part of the shaft above," and that the

absence of coke (speaking of pyrite furnaces) accentuates the sharp division line between the active and stagnant regions of the furnace. Reasoning by analogy, it would seem that this division line would be as pronounced (if not more so) in operating an electric blast furnace, such as we are discussing, than would be the case in the pyrite furnace.

XII. The Electric Blast Furnace vs. the Copper Blast Furnace for Partial Pyritic Smelting.

1. Metallurgical Considerations.

To recapitulate, there does not seem to be any metallurgical reason why the chief objects of the pyrite smelter cannot be carried out in an electric copper blast furnace as well as in a coke copper blast furnace; namely, "to melt the great mass of SiO_2 and inert earths, to oxidize enough of the sulphides in the charge to insure a suitable matte—incidentally obtaining the heat evolved by the oxidation." On the other hand, it would seem that the difficulties ordinarily encountered in operating a pyrite furnace might be avoided when using an electric blast furnace, namely, the difficulties which arise when too much or too little coke is added to the charge, and moreover that it not only permits "introducing heat into the furnace, without at the same time robbing the combustion zone of oxygen," of which Dr. Peters states, "nothing would be so welcome to the furnace man as to do this," but would also permit of the heat being entirely under control and easily regulated, and thus avoid freeze-ups with their consequent vexations and costly delays. By this we do not mean that any sort of a charge could be put through the furnace and not freeze it up, an idea which seems to be quite prevalent in regard to electric furnaces, but that, if the charge be properly calculated, a very much wider variation would be permissible in the composition of the slag from that calculated, than would be the case in blast furnace smelting.

2. Mechanical Considerations.

By referring to Figs. 3 and 4, it will be noted that the chief difference in the construction of the furnace would be in that part of it below the tuyeres,—in other words, in the crucible. Because electric furnace construction has received the attention of some of the very best mechanical and electrical engineers, and because a crucible based upon the principle shown in the design in Fig. 3 is now extensively used in the iron electric reduction furnaces of Norway and Sweden, we believe that no difficulty would be experienced in this respect. Various methods and arrangements

can be used for connecting up electrodes. If this be true, we have only to consider the matter of costs.

3. *Comparison of Costs.*

a. *General Considerations.*

From what we have said, it will be evident that a comparison of costs is made simply for the purpose of giving some idea as to the outlay that would be necessary to erect and operate an electric furnace plant for the purpose of smelting copper ores.

Naturally, the first thing to be considered is whether it would be feasible to attempt to substitute electrical energy for coke, due to the cost of the former.

b. *Cost of Plant.*

As a matter of fact, the cost of an electric furnace plant would be the cost of a regular plant plus the electrical installation, exclusive of the generating plant, because it is assumed that it would be possible to purchase power from some power company, and, if not, the generating plant would probably be considered as a separate organization, selling power to the smelter at so much per unit. Therefore, by electrical installation we mean the cost of transformers, bus-bars, cables, instruments, etc. In order to get some idea of this cost we will assume that our furnace is to smelt 384 tons of charge per day[1] and that the composition of the charge is:

	Per Cent.
Cu	5.21
SiO_2	26.41
FeO	18.60
S	11.46
Al_2O_3	4.26
CuO	17.49

This was the average composition of the entire blast furnace charge which was smelted at the Washoe smelter, Anaconda, for several months.[2] In this plant 8.2 per cent. of coke was used. If we should attempt to smelt a charge of this nature in an electric furnace, what amount of electrical energy would be required?

c. *Amount of Electrical Energy Required to Replace Coke.*

Since 8.2 per cent. of coke was required at the Washoe smelter to smelt the above charge:

[1] At the Washoe smelter, Anaconda, the furnaces were formerly 56 by 180 in. (hearth area, 70 sq. ft.) and smelted on an average a little less than 400 tons of charge daily, or 5.6 tons per square foot. From *Principles of Copper Smelting*, Peters, p. 156.

[2] *Practice of Copper Smelting*, Peters, p. 267.

2,000×8.2 = 164 lb. of coke, were used. This coke contained 80.24 per cent. fixed carbon; therefore, the carbon equivalent of the coke is: 164×80.24 = 131.59 lb. If one pound of carbon be completely burned to carbon dioxide, we obtain 8,100 lb. cal. Therefore, 131×8,100 = 1,061,100 lb. cal. that would be obtained from 131 lb. of carbon contained in the 164 lb. of coke. As 1 kilowatt hour is equivalent to 1,897 lb. cal., it would require: $\frac{1,061,100}{1897}$ or 559 kw. hr. to replace the theoretical calorific value of the 164 lb. of coke. However, as a copper blast furnace at its best probably does not have an efficiency of over 50 per cent., the energy really obtained from the coke, so far as work performed in the furnace is concerned, is only 530,550 cal. and this would represent theoretically, $\frac{530,550}{1897}$ or 280 kw. hr. per ton. Inasmuch as the efficiency of electric furnaces of this type may be as much as 85 per cent., and as that of an open top ferro-silicon furnace is said to be about 60 per cent.,[1] it would be fair to assume that the efficiency of an electric furnace, such as shown in Fig. 2, would be 20 per cent. greater than that of the ordinary blast furnace, and hence we will say it is 70 per cent. If this be true, then the theoretical 280 kw. hr. needed to smelt one ton of charge would have to be increased 1.42 times (280×1.42 = 397) or say in round numbers, 400 kw. hr. would be required to smelt one ton of charge. Considering that part of the heat in this case is supplied by combustion of sulphide the figures agree closely with the 480 kilowatt hours calculated from the experiments on melting only. As we are to smelt 10 tons of charge per hour, 400×16 = 6400 kw. hr. would be the constant load on the furnace, and, as the power supply would doubtless be three-phase, this would mean that the electrical equipment should consist of:

Three 2500-kw. variable voltage transformers, to allow ample margin for sudden overloads, etc.; instruments, bus-bars, cables, etc.

As to their costs at the plant, that would, of course, depend upon the locality, and would be the factory costs plus transportation charges. To partially offset this cost, the blower capacity would not need to be so large as in the case of the regular blast furnace, because less air is blown.

d.—Cost of Smelting.

We may say that the costs of coke and electrical energy are about on a par when coke costs $7 a ton, and electrical energy 0.15 cents per kilowatt hour, or $13 per kilowatt year,—or say in the ratio of 1: 1.8. Although there may be advantages in the use of an electrical blast furnace instead of an ordinary blast furnace, these will not be taken into consideration in this connection, and we will assume that the cost of electrodes and of the electricity on the one hand, and its equivalent in heating value

[1] *Revue de Métallurgie*, vol. IX, p. 362 (1912).

of coke on the other, would be the same; in other words, that the cost of electrodes and electricity would balance the cost of the amount of coke that would be necessary to do the same amount of work. We will not take into consideration the saving in blowing by reason of not having to furnish air for burning the coke.

e.—Cost of Electrodes.

The average consumption of electrodes per ton of iron produced at Trollhattan, Sweden, is about 10 lb. Assuming 1 ton of iron is equivalent to 2 tons of charge, the consumption of electrodes per ton of charge smelted in a furnace, such as the one we have been discussing, would not be over 5 lb. per ton. If we assume that the electrodes do not cost over 6 cents per pound, which allows a fair margin for transportation over their cost at the factory, the cost of electrodes would be about 30 cents per ton of charge smelted. The consumption would be probably considerably less than 5 lb. per ton, as was shown in an experiment performed by the writers in which air was blown into the furnace, and in which the electric consumption was only 2 lb. per ton of ore treated. Such being the case, if our combined electrical energy and electrode cost is not to exceed our equivalent coke cost, with coke at $9 a ton, the cost of electrical energy would be $9 × 1.8 = $16.20 a kilowatt year, minus the cost of electrodes, or about $16.00. Compared on the basis of calories, if the coke contains 80.24 per cent. fixed carbon, it would contain 1604.8 lb. of carbon per ton of 2000 lb., and this carbon would, if completely burned to CO_2 give a calorific value of about 13,000,000 calories. As one kilowatt year of energy is equal to 16,616,000 cal., it would require something like 1.2 tons of coke, containing 80.24 per cent. fixed carbon, to equal in calorific value the kilowatt year, but it is to be remembered that this assumes that all the carbon of the coke is completely burned to carbon dioxide, which rarely happens in practice, and so, as has been determined from actual practice, it requires from 1.5 to 1.8 tons of coke to yield the same number of calories as may be obtained from 1 kilowatt year of electrical energy.

XIII.—The Use of the Electric Blast Furnace in the Smelting of Pyritic Ores.

As previously stated, in all true pyritic smelting, fuel (usually coke) in varying amounts is added to the charge. We will assume for the sake of argument, that the ore being treated is entirely suited to pyritic smelting, and that it is not necessary to use more than 0.5 per cent. of the weight of the charge in coke. Could such an ore be treated pure as well

in an electric blast furnace as in an ordinary blast furnace, assuming that the costs would be the same in each case? As what we have had to say in regard to the construction of the furnace and the costs of applying the process is as applicable to true pyritic as it is to semi-pyritic smelting, we will only take up the subject from the metallurgical and chemical standpoint.

In the first place let us consider the nature of the atmosphere in a pyrite furnace. As the air enters the tuyeres, practically all of it enters into immediate combination, forming SO_2 and FeO. Although some oxygen escapes combination, it is not sufficient to support combustion within the furnace, as has been shown by Sticht and others. Now let us briefly consider the changes which take place in the charge as it descends from the charging door to the fusion zone. They are few, and may be briefly stated as follows:

(1) The iron sulphide loses one-half of its sulphur as elemental sulphur, and becomes FeS.

(2) Chalcopyrite. It loses about one-fourth of its sulphur and becomes practically a mixture of Cu_2S and FeS.

(3) The silica is unchanged.

(4) The coke is for the most part consumed in the upper zone of the furnace, but not by oxygen, as no free oxygen exists in that part of the shaft, but by the O from SO_2, that is, the carbon of the coke reduces the SO_2 giving S and CO, and this CO reacts with more SO_2, forming S and CO_2. Aside from these reactions, which are of no great importance in the chemistry of the process, the chief office of the coke as pointed out by Sticht,[1] "Is apparently to heat up the sulphides and the quartz in preparation for their active oxidation deeper in the furnace." If, therefore, coke was omitted from the charge we would not have this preparation of the sulphides and the quartz as stated by Sticht. Such being the case, how detrimental to the process would this lack of preparation be?

In order to answer this question, it must be borne in mind that in true pyritic smelting, there is "no heat to spare," and that although, as pointed out by Sticht, the heat evolved by the combustion of the coke by the oxygen of sulphur dioxide is only one-third as much as it would be if the oxygen were furnished direct by the blast, and that it is given off at a point considerably higher than the focus of the furnace, it nevertheless "assists in preparing the charge for the reactions which take place in the focus of the furnace, for with 1 per cent. of coke in the charge enough heat is generated to supply one-third of the heat required to melt the entire pyrrhotite contents of the charge." This extra heat "doubtless furnishes just the necessary aid to bridge the operation over some critical

[1] *Principles of Copper Smelting*, by E. D. Peters, p. 227 (1907).

point." From this we see that the office of the coke then is solely for the purpose of furnishing heat. Inasmuch as the nature of the charge in true pyritic smelting is such as to demand the oxidation and removal of a large excess of iron as silicate, and as it is necessary to cause the silica present to unite with as much FeO as possible (due to the fact that generally silica-bearing materials have to be added for slag-forming purposes, and often these are barren and hard to obtain), and as the saturation point of silica with iron oxide is greater the higher the temperature attainable in the pyritic furnace, it is quite necessary that the reactions in the zone of the furnace proceed as energetically as possible, in order to meet the requirements stated above, and to provide enough heat to keep the process going. If, therefore, pyritic smelting be attempted in an electric blast furnace, it would be necessary to supply the heat from the electric current in such a manner as to insure there being enough heat at the focus to carry on the reactions at that point in the same manner as if coke had been added to the charge. The writers are of the opinion that it would be possible and perfectly feasible to do this in the furnace of the type shown in Fig. 3. Moreover, that the use of the electric furnace would permit of a more uniform operation of the furnace than is now possible, for, with the electric current, it would be an easy matter to quickly increase or decrease the heat required for the successful operation of the furnace. In this way we could maintain the "degree of concentration at a constant," which, as pointed out by Sticht, is very important, but which is extremely difficult to do. Also, we could avoid the troubles arising from the "constant fluctuations" which are bound to occur, and which at present demand unbroken attention and frequent changes in the charge, especially in its proportion of silica "due to the fact that in true pyritic smelting there is no heat to spare," whereas, with the electric blast furnace, it would be possible, as just stated, to increase or decrease the heat as necessary in order to meet these fluctuations.

Summary.

Summarized then we may say that the electric smelting of copper ores is nothing more than the substitution of electric heat for the heat derived from the combustion of carbon. Inasmuch as the carbon which is used either in the reverberatory furnace or in the blast furnace plays no important part in the necessary reactions which take place in these furnaces, there is no reason, metallurgically, why electric heat may not be substituted for the heat derived from the combustion of carbon. In fact, as we have tried to point out, in some cases the reactions would take place to better advantage in the neutral atmosphere of the electric furnace than in the reducing or partially reducing atmosphere of the com-

bustion furnace. Therefore, as to whether the electric furnace would be used for the smelting of copper ores would largely depend, so far as we are able to make out at the present time, upon the relative cost of coke and electric power. As the use of the electric furnace is not advocated as a competitor of the combustion furnace, but as a substitute for it in those localities where it is not advisable because of the high cost of fuel, we see no reason why the electric furnace may not be developed as a substitute for the combustion furnace, where the conditions are such as to warrant its use, especially in the treatment of copper-bearing ores. In this connection it is to be remembered that the reason why the electric furnace was developed in the iron industry, for the reduction of iron from its ores was due to necessity. As a matter of fact the field for the electric furnace in the reduction of iron from its ores is a limited one. Perhaps the same is true as regards the possible application of the electric furnace to the treatment of copper ores, but, judging from the comparative costs as shown in the preceding pages, it would seem that the chances in favor of the electric furnace for the treatment of copper ores are greater than those for the treatment of iron ores, because there is not so great a difference in the cost of coke and electric power in copper mining districts as in iron smelting centers. Also the cost of electric power is constantly becoming less, due to improvements in gas engines and steam turbines, so that, in districts where water power is not plentiful but cheap fuels unsuited to coking purposes are available, it may be found more advantageous to use electric heat than the heat derived from the combustion of coke.

It is sincerely hoped by the writers, that although it is possible at this time to present but few facts, that the comparative study herewith presented may serve to stimulate interest in the subject of the electric smelting of copper ores, and to cause others to attempt a further investigation of it. As a result of their investigations, the writers are convinced that experimental work on a larger scale should lead to the development of an electric blast furnace, which in some cases could be used to better advantage for smelting sulphide ores of copper than the combustion blast furnace.

Discussion.

C. D. Woodward, Great Falls, Mont.:—We constructed a small blast furnace, and used first carbon electrodes and an alternating current of about 50 to 75 volts. We were able to smelt the material which was fed to the furnace, but we did not carry on experiments far enough to see just exactly what the power consumption was and compare it with the amount of material treated. We found that the blast seemed to help materially in reducing our charge and cutting down our power consumption.

There were several other experiments tried at the time, and I do not call to mind now just what they were. We were trying to do away with the carbon electrode and replace it with an iron one, and it also seems to me we tried to use the ore for an electrode; but I believe that the electric furnace in the reducing end is simply a question of British thermal units per kilowatt-hour compared with British thermal units in a pound of coke. Now, if a pound of coke costs us 0.5 c. and a kilowatt-hour costs us in the neighborhood of 1.5 c., I think there is a big advantage in using coke instead of the electrical current for copper reducing.

We tried steam, and a number of other methods, to throw copper down, but it has been some time ago and I am not very familiar with them now. The outlook of the experiments did not seem to be very promising, and we took it more as a thermal problem than an electrical one.

PROF. JOSEPH W. RICHARDS, South Bethlehem, Pa.:—Mr. Woodward has spoken of the question as a thermal problem. I would call attention to the fact that electric furnaces usually utilize the heat of the electric current at from four to ten times the efficiency with which the heat generated by fuel is utilized; so it will not do simply to compare by thermal units generated. Your total thermal units may cost much less from coke than from electricity, yet you will find the electrically generated thermal units utilized in a properly designed furnace from three to ten times more efficiently than in the combustion furnace. You must make that allowance is comparing the cost of thermal units generated electrically and thermal units from coke.

With regard to the melting of copper concentrates, which Mr. Lyon has spoken of, I think it will be found advisable to use the resistance electric furnace for this purpose rather than the arc furnace or the induction furnace. The arc furnace is the one most in use for steel, and the one which Mr. Lyon used, but there you are troubled with the volatility of copper. I think many difficulties can be overcome if electro-metallurgists will design a proper resistance furnace for copper which utilizes only the resistance of the charge and uses no electrodes and no arc. By using no arc you get away from the high temperatures which cause volatilization, and using no electrodes you save money. I think the resistance furnace will be found the proper type for most electric copper furnaces.

DORSEY A. LYON:—In that connection I will say we operated the furnace as a resistance furnace; we never allowed it to strike an arc. I quite agree with you that where you have an electrode you have a local high temperature. If you notice the design given, the idea is to distribute the current as much as possible; to use large electrodes to avoid an arc. If you could design a furnace to have no electrodes it would be a great improvement. So far we have not been able to do that.

C. D. WOODWARD:—That brings to mind now that we did try a resistance furnace, and we used iron electrodes, and, as the resistance of our ore varied according to the temperature, the heat seemed to be uncontrollable in the furnace; that is, it would have required perhaps a regulating transformer in order to hold the furnace down to some heat that would not explode the machine after we got a certain temperature on it. The furnace seemed to have about the same characteristics as a Nernst lamp, and, as the sulphur was driven from the ores, the resistance seemed to decrease and the flow of current evaporated until either the temperature of the furnace became an incandescent one or the voltage had to be reduced and followed up pretty closely.

DORSEY A. LYON:—I think it has been quite well proved in the iron industry that it is necessary to have variable voltage control. If constant voltage is used, we encounter the trouble you have mentioned.

W. MCA. JOHNSON, Hartford, Conn. (communication to the Secretary *):—The writer's experience has been confined recently to the electric smelting of complex zinc ores. These ores usually carry lead or copper as by-products. So his experience will possibly throw some light on the subject so ably and conservatively treated by Mr. Lyon and also treated in a manner calculated to advance the progress of the art of electric smelting.

The first feature that is startling is the extremely low copper values in the slags made at the Hartford plant of the Continuous Zinc Furnace Co. In the last 14 months there has been made there some 16.9 tons of slag. In our furnace there are two tap-holes, one for slag and another for copper matte and lead. Through the latter some foul slag is tapped. An average (not, however, a weighted average sample) sample of these 104 taps of slag analyzes 0.065 per cent. copper

* Received Sept. 10, 1913.

(electrolytic assay on 5 g.); 0.05 per cent. Pb (wet assay on 5 g.); 0.50 oz. Ag on 2 A. T.; 0.01 oz. Au on 4 A. T. Many of the individual analyses have been checked by outside chemists.

Furthermore, in June, 1913, we made an eight-day run on a charge analyzing 0.43 per cent. Cu and made a matte analyzing 5.33 per cent. Cu with slag analyzing 0.05 per cent. Cu.

The conditions for the removal of copper, whether held in slag as copper silicates by chemical action, dissolved as copper sulphide in the slag by an indefinite chemical action, or held mechanically as particles of matte in physical suspension, are well-nigh ideal and can be briefly summarized as follows:

(1) A thinly liquid superheated slag. Our slags will run at 1,100° to 1,200° C., but are tapped at 1,250° to 1,350° C.

(2) A quiet pool of slag continually titillated by the 60-per-second pulsation of the alternating current in a manner similar to the rapping of the assay crucible by the assayer. Our Mr. E. N. Morrill pointed out to the writer this point.

(3) The intense reduction of the smelting zone that is strong enough to reduce 600 to 1,000 lb. of zinc oxide per ton of charge.

Each of the three makes for a complete separation of the copper from slag, be it held there originally by either chemical or physical forces.

The above is all based on actual experience and on what actually has happened. But if we consider what possibly could happen, let us suppose that we have two copper mixes, one basic and one acid, and power at $0.003 and soft coal at $3 per ton.

Now run the basic mix through a basic-lined preheater fired with soft coal and mechanically operated, and run the acid charge through an acid-lined preheater of the above description and charge this with 1 per cent. of coal into a furnace something like our Hartford furnace for smelting complex sulphides. With the exothermic reaction of FeO and CaO on the SiO_2 and preheating to 1,100° C. and smelting at 1,300°, the power consumption per ton of charge would be less than 300 kw-hr. in a 400-kw. unit. The amount of electrode consumed would be certainly less than 5 lb. per ton of charge, and with an open-flame reverberatory the coal for preheating would not amount to more than 12.5 per cent. of the charge.

Allowing for increased values of copper, silver, and gold recovered and considering the wider range of slags possible, as far as my judgment goes such a procedure would have great commercial worth if operated along several possible lines, such as smelting fine ore. The use of an electric furnace as a settler for hot copper-furnace slags

and making out of them possibly also a low-carbon pig iron presents certain promising potentialities.

F. L. CLERC, Estes Park, Colo. (communication to the Secretary *):—Among a number of very interesting questions raised in this paper, the following, quoted almost literally, but not consecutively or in full, appeal to me as having a wider interest in connection with the pyrometallurgical concentration of metallic values in lean ores.

"Whether electric heat may be used to replace the heat which is derived from the combustion of coke in pyrite and semi-pyrite processes . . . would it be possible and commercially feasible to carry out these processes in an electric furnace using a blast, obtaining as much heat as possible from the oxidation of the sulphur and iron, and supplying by electric energy whatever additional heat might be needed?"

"In true pyritic smelting 'there is no heat to spare.' If, therefore, pyritic smelting be attempted in an electric blast furnace, it would be necessary to supply the heat from the electric current in such a manner as to insure there being enough heat at the focus to carry on the reactions at that point."

Thermo-electric processes are flexible, "for with the electric current it would be an easy matter to quickly increase or decrease the heat required for the successful operation of the furnace."

"This extra heat doubtless furnishes just the necessary aid to bridge the operation over some critical point."

The suggestion I would make is this: Why not apply the electrically generated heat to the blast, and in that manner insure there being enough heat at the focus to carry on the reactions at that point?

These reactions are exothermic, and the attainable temperature depends on the rapidity of the reactions, but the reagents must be heated before the reaction will start. I recommended this procedure more than a year ago, for the igneous concentration of the zinc in mixed sulphide ores. It appears to have many advantages.

The furnace would be freed from the encumbrance of electrodes passing through the arch, and no special form would be required. The electrical stoves would be replaceable units, independent of the furnace structure, and all electrical connections would be readily accessible. There is little difference of opinion about the effect of a superheated blast in pyritic smelting. Superheated is understood to mean hotter than ordinarily used in the same branch of metallurgy. Most of the variations to be observed in practice are due to local

* Received Sept. 27, 1913.

conditions. It may be cheaper to add a little coke and use cold air, and more of it, than to install stoves for heating the blast, or the charge may be too fusible for a high furnace.

The idea of heating the blast at the tuyeres is not a new one; it was developed by W. Lawrence Austin, in connection with pyritic smelting, a dozen or more years ago. He burned petroleum with an air jet, in a cast-iron bulb surrounding the air pipe leading to the tuyeres.

It would appear that an electric heating section could be inserted in each of these pipes, and that, in addition to the heat required for normal working, additional heat could be supplied as desired, to carry the furnace over some critical point, either by increasing the temperature or the volume of the blast, at the particular tuyere where it would do most good, or by keeping in reserve a more intense blow pipe for use in emergencies, to fill the rôle of the "fighting tuyere," which iron furnace masters found useful in the days before a uniform furnace charge was practicable.

Note on the Utilization of the Waste Heat of Regenerative Furnaces.

BY GEORGE C. STONE, NEW YORK, N. Y.

(New York Meeting, October, 1913.)

THE stack gases from regenerative furnaces are very seldom utilized for the production of steam. If the temperature of the gases is not higher than 300° C. (572° F.) there is no economy in their use for this purpose, as the amount of available heat is very small and the temperature is so little above that of the steam that the interchange is too slow. With higher temperatures of the waste gases, however, good results can be obtained.

At the plant of the New Jersey Zinc Co. (of Pa.) at Palmerton there are a number of return tubular boilers, each 6 ft. in diameter and 20 ft. long, with 96 3.5-in. tubes, which are heated by the waste gases from the spelter furnaces. These furnaces have continuous regenerators heating the air only and the stack gases reach the boilers at a temperature of about 450°. The gas producers for one of these furnaces require 14 tons of anthracite (a mixture of Nos. 2 and 3 buckwheat), and the waste heat from the furnace gives a little more than 42 h.p. in steam.

The boilers are not well adapted for the purpose, as they are much too large for the amount of heat going to them and the area of the tubes is not sufficient to take the gas from two furnaces without causing an objectionable amount of back pressure. The plant was built without any idea of using waste-heat boilers, and it was impossible to place the latter as close to the furnaces as they should be. Furnace and boiler are connected by an underground brick flue in which the temperature of the gas drops about 100°, losing 40 per cent. of its available heat. If the boilers were close to the furnaces the horse power of each would be 70 instead of 42.

Even under these unfavorable conditions the boilers have been very advantageous, as one boiler easily supplies the steam necessary for two batteries of producers.

With such low gas temperatures the heating surface of the boilers is, of course, not as efficient and the flue areas need to be considerably larger than for coal firing.

We have found that under our conditions with gases at 450° about 3 h.p. can be obtained for each ton of coal gasified, and that this is increased or decreased by 1 h.p. for a variation of 50° in the temperature of the gas entering the boiler setting. In Germany similar furnaces fired with gas from bituminous coal have given rather better results, as they were equipped with especially designed water-tube boilers. Both here and in Germany the use of the boilers has had no visible effect on the working of the furnaces.

Discussion.

D. S. Jacobus,* New York, N. Y.:—The engineers of the country are waking up to the possibilities of waste-heat boilers. As the cost of fuel increases, and as more attention is given to economies, it is evident that a great deal may be gained by utilizing heat that has ordinarily been wasted. The art in the manufacture of waste-heat boilers has advanced so that it will now pay to install them under certain conditions where it would not have paid to install boilers constructed in accordance with older methods. In using boilers where the gases are at a comparatively low temperature, a great deal depends on the method of baffling the boiler in securing good results. The Babcock & Wilcox Co. has made a careful investigation of the laws of heat transfer and has applied the same to waste-heat boilers, which have been carefully tested under operating conditions, with the result that we now know what to expect with different arrangements of baffles and heating surfaces.

The old idea of installing waste-heat boilers was to provide an arrangement where the draft loss would be a minimum, and little or no attention was paid to the effect of different systems of baffling on the steaming capacity of the boiler. In some instances, to secure a minimum draft loss no baffles were placed in the boilers. After making a most careful study of the subject it developed that this practice was bad, and that to obtain the best commercial economy, in place of endeavoring to obtain a minimum draft loss, an arrangement should be used that would lead to the maximum amount of heat transmission with a comparatively high draft loss. In nearly every case the amount of resistance of the gases passing through the boiler involves the use of an induced-draft fan, but the additional capacity developed by the boiler over that which could be secured with the older designs, through arranging the heating surface properly and using proper baffles, much more than pays for the increase in the steam required to drive the fan.

* Non-member.

In the older practice with gases at, say, 1,100° or 1,200° F., about 18 or 20 sq. ft. of heating surface would be allowed for each boiler horse power developed. By our present methods it is perfectly feasible to develop a boiler horse power with 10 or 12 sq. ft. of heating surface for this gas temperature.

The introduction of an induced-draft fan outfit has been found an advantage in some instances where waste-heat boilers have been applied, as it allows better regulation of the draft acting on the furnace, with the result that the output of the furnace has been increased over what it was with natural draft. There were therefore two gains through installing the waste-heat boiler, one through increasing the capacity of the furnace on which the boiler was applied, and another through utilizing the waste heat.

E. B. CARTER,* Philadelphia, Pa.:—Two points of the subject that do not seem to have been touched are very important to persons considering it. These are, the question of first cost, and the question of cost of horse power development. Naturally the cost of horse power installed runs very much higher than the cost of installing the same horse power in the boiler house, because you have to have the auxiliaries and the small units. In the cost of operation you have additional costs in nearly every item except the cost of fuel, and I would like to inquire of Dr. Jacobus whether they have obtained any data on those subjects. I also understand that from experiments they have made it has not paid to install boilers where the temperatures were much below 500° C.

T. T. READ, New York, N. Y.:—It may be of interest to call attention to the fact that the use of waste-heat boilers has become general practice in the reverberatory smelting of copper ores. At the Steptoe Valley smelter they have a number of oil-fired reverberatory furnaces, and S. S. Sörensen, the Superintendent, has made a series of interesting comparisons between the results with the Stirling type of boiler and the Babcock & Wilcox boiler. As his paper, which has just appeared, is the first comparison of that sort that has come to my attention, it may be well worth while to cite a few figures from it. At the Steptoe plant the Stirling boilers were the first used, and the management was prejudiced in favor of them because of their being so easily cleaned. Later they installed some Babcock & Wilcox boilers and carried out a series of comparative tests, of which Mr. Sörensen has given us the record. The Stirling boiler was of the

* Non-member.

ordinary type of that make; the Babcock & Wilcox was a special type, especially designed for the Steptoe Valley company. The boiler horse power of the Babcock & Wilcox installation was 520 and of the Stirling 375. In a 100-day test the Babcock & Wilcox made a net recovery of $25,000 worth of oil and the Stirling boilers recovered $20,000 worth. Mr. Sörensen's conclusions are given at length in his paper,[1] but may be summarized by saying that the tubes in the Babcock & Wilcox boiler are so arranged as to give greater heat efficiency; and the greater cost for cleaning and repairs is overcome by the greater recovery of heat.

On the question of first cost I might say that at the Copper Queen smelter, at Douglas, Ariz., Erie boilers were installed because of their lower first cost. I have been told that the results show that the efficiency of these boilers is comparatively high, but difficulty is experienced on account of the priming to which the Erie boilers are subject. Waste-heat boilers are typically driven at overload, and under such conditions I understand that the Erie boiler primes badly. It is evident from this that it is well worth while to devote some study to the selection of the proper type of boiler for waste-heat recovery before beginning the construction of a plant.

JOSEPH W. RICHARDS, South Bethlehem, Pa. :—I wish to comment on the inefficiency of the chimney. If you are using a chimney merely to produce draft, you are using one of the most inefficient apparatus that has ever been devised by man. The mechanical efficiency of the chimney for producing draft, calculated upon the heat energy which goes into it, is certainly less than 1 per cent. Now consider most of our regenerative furnaces, which are supposed to save a great deal of the heat from going into the chimney, and yet about 25 per cent. of all the heat that the coal furnishes goes up the chimney. We are using then this 25 per cent. of the calorific power of the coal at an efficiency of less than 1 per cent., if we are utilizing the chimney as a draft producer. In the best regenerative steel furnaces about 25 per cent. of the heating power of the fuel goes into the steel; so you are using up as much heat to run the chimney as you get efficiently into the steel, and that chimney heat is utilized to produce draft at an efficiency of less than 1 per cent. This points to the general conclusion that in every case where it is possible to use the heat that is going to the chimney for any other purpose whatever it should be thus used, and the function of the chimney as a draft producer replaced by the fan. If you can thus reduce the tempera-

[1] *Mining and Scientific Press*, vol. cvii., No 15, p. 576 (Oct. 11, 1913).

ture of the gases down even to the ordinary temperature, so much the better.

I think that the apparatus under discussion could well be provided with feed-water heaters, and thus reduce the temperature of the gases still lower, and replace still further the energy required by the chimney by the mechanical draft. You have at least 99 per cent. of the heat energy in the gas absolutely wasted when you are using the chimney simply as a draft producer.

D. S. JACOBUS:—The first cost of the boilers is greatly reduced through our present practice as compared with the old, as by arranging the boiler and baffles properly a much greater capacity is obtained for a given amount of surface than was possible with the older designs. Whether it will pay to install waste-heat boilers at temperatures lower than 500° C., or somewhat over 900° F., depends on the volume of gas involved. We are installing boilers to run with this temperature of gases where there will be a large return as compared with the capital invested. How much lower temperatures we can handle depends on the individual case at hand, and cannot be stated unless all of the conditions are considered.

There is another feature that was brought up by Professor Richards, namely, that a feed-water heater or economizer might be used to advantage in connection with a waste-heat boiler. An economizer in certain instances would give a material increase in the efficiency, but the economizer problem in connection with a waste-heat boiler is a difficult one, because usually there are times when a large volume of the gases passes through the boiler at a much lower temperature than the average. When this occurs the amount of heat absorbed by the economizer as compared with that absorbed by the boiler increases, with the result that steam may be generated in the economizer. To avoid this it is necessary to employ some sort of an automatic controlling device.

A MEMBER:—Will Mr. Jacobus tell us about how cool it is possible to make the gases as they leave the boiler?

D. S. JACOBUS:—The temperature of the gases leaving the boiler depends upon the arrangement of the boiler surface and baffles, and, in general, the higher the draft resistance the lower will be the final temperature of the gases for a given initial temperature and volume of gases. This temperature may be, say, 500° F., all depending on the particular arrangement which is used, the best arrangement, of

course, depending on the conditions existing at the plant. There are so many features to the problem that it is hard to generalize. Increasing the velocity of the gases over the heating surface increases the heat-transfer rate, and with our present practice the heat-transfer rate is about twice what it was for our older practice. This does not mean that in applying a boiler to a furnace we would necessarily secure double the capacity that we would by our older practice, as there is a given amount of heat in the gases leaving the furnace, and doubling the heat-transfer rate does not therefore double the boiler capacity.

JOSEPH HARTSHORNE, Pottstown, Pa.:—I think that Professor Richards has been rather liberal to the chimney. From my own observation and experience, I can say that I think that it will be generally found that the chimney loss of heat will be much nearer 30 per cent. than 25 per cent.

I recently had occasion to make a rough estimate of the amount of heat which would be available for steam from the flue gases of a 50-ton open-hearth furnace. The data, partly assumed, were as follows: 50-ton heats, taking 10 hr. to finish; 500 lb. of coal, having 14,000 B.t.u. per pound, per ton of steel; stack loss, 30 per cent.; temperature of flue gases, 1,200° F.; and temperature of gases leaving the boiler, 600° F. On this basis, there would be, in round numbers, about 50,000,000 B.t.u. available for making steam during the 10 hr. This shows what can be expected, as far as quantity of heat is concerned.

As to practicability, it is well known that the process is being used in this country, fans being used to draw the waste gases through the boilers. In 1912 I saw it successfully in use at Phœnix-Ruhrort, on two 40-ton furnaces. There was a 200-h.p. water-tube boiler attached to each furnace, to which the waste gases came at a temperature of about 700° C. There was a stack, about 20 ft. high, beyond each boiler, into which a pressure fan blew a stream of air, drawing the gases through the boiler on the ejector principle. The fan was driven by an 80-h.p. motor, which seems an excessive power requirement. The boilers were said to evaporate 22 liters of water per square meter of heating surface (0.54 gal. per square foot).

Drawing the gases through the boiler by means of a fan would appear to be the more economical and efficient method. The gases should be reduced to at least 400° F., which should not be injurious to the fan.

An added advantage is the fact that the furnaces should be more

easily controlled by the damper, atmospheric influences being entirely eliminated.

G. C. STONE:—As to our boilers, the cost of installation is very small. The cost of boiler per horse power is large, as it is necessary to have a large heating surface. Our cost of attendance is extremely small; as our boilers are placed, they are in widely separated parts of the plant and we take more than we should, but the actual labor would be about one man to ten boilers. The repairs to the boilers have been slight and the cost of cleaning has been very small. We are actually running with the gas, as I said, at 450° C., which is about 840° F., and at times it is as low as 300° C. It would not pay to put in boilers if the gases were regularly at that temperature.

JONATHAN SEWELL*, Anaconda, Mont. (communication to the Secretary†):—Referring to the article by S. S. Sörensen, quoted by T. T. Read, I am surprised to find the results given as proving the superiority of the Babcock & Wilcox boiler over the Stirling for the generation of steam from waste heat, for our experience with the Stirling type is much different, as shown by the following figures:

Thirty-two Stirling boiler tests were made by 20 experts, in various parts of the United States, the boilers ranging in rating from 125 to 1,125 h.p., the fuel used being a variety of coals, and in three cases, California crude oil. In 24 tests the boilers ran from 1.7 to 132 per cent. above their rating, an average of over 28 per cent. One boiler of 335 h.p. developed 97.36 per cent. above its rating and evaporated 13.85 lb. of water per pound of crude oil.

In July, 1902, on a test, one of our 300-h.p. boilers on a 50-ft. reverberatory furnace developed 336 h.p. with baffles in; and again in April, 1903, under test, a 300-h.p. boiler developed 346 h.p. with baffles removed. From Jan. 9 to Feb. 10, 1904, tests were made on nine new 375-h.p. boilers, from 399 to 461 h.p. being developed. On a more recent test, a 375-h.p. boiler on our present furnaces developed 470 h.p.

In view of the fact that oil was used as fuel on the furnaces at the Steptoe Valley plant, it would be reasonable to expect much better results from the boilers there than can be obtained here, because of the more uniform temperature of the furnaces. The relatively poor work done by the Stirling boiler at the Steptoe plant I am convinced was due to the setting of the boilers. The setting is highly favorable

* Non-member, Superintendent of Power Houses, Washoe Reduction Works.
† Received Jan. 6, 1914.

to the Babcock & Wilcox boiler, while it would be hard to imagine a more unfavorable setting for the Stirling. The boiler is placed so far above the point where the flue enters the setting that there is a large amount of heating surface in the upper portion of tubes and drums which is practically lost, since it is practically a dead space where there is little or no circulation of the gases. In contrast to this, in our setting the top of the mud drum is about 16 or 18 in. below the floor of the entering flue, and the flue is sufficiently large at that point and far enough away from the boiler to allow time and space for the gases to expand before reaching the tubes, so as to fill completely the boiler setting, impinging on the tubes and drums in their passage through, thus giving the best possible results.

I believe the setting of the Stirling boilers on the reverberatory flues here is such as to leave little if any room for improvement, and the comparatively small cost of repairs on the 16 boilers that have been in use for over 10 years confirms the opinion that I expressed when they were first installed, that, all things considered, the Stirling boiler is one of the best that can be used for the utilization of waste heat, and I feel confident that with our coal-fired furnace, the grates kept reasonably clean and carefully fired, the 375-h.p. boilers will develop 500 h.p. and compare favorably with any type of boilers used for the same purpose. Various approved settings of Stirling boilers for waste heat of reverberatory furnaces are shown in E. P. Mathewson's paper, The Development of the Reverberatory Furnace for Smelting Copper Ores, *Trans.*, xliv., 781.

The Generation of Steam by Waste Heat from Furnaces.

BY F. PETER,* LEOBEN, AUSTRIA.

TRANSLATED BY R. W. RAYMOND, SECRETARY EMERITUS.

(New York Meeting, October, 1913.)

I. General Introduction.

Technical progress takes place in two directions: the improvement of methods, affecting the quality of the product; and increase in the economy of operations, affecting its cost. In the iron-industry, practice is pretty well settled, and revolutionary changes of method are, for the present, not to be expected. Hence, in this industry, the principal endeavor is to reduce costs by the use of labor-saving inventions, such as the machines brought into use during the last decade. But it is not alone necessary to reduce the labor-cost; complete economy requires also a saving in raw material. Under this head we may rank the coal employed as metallurgical fuel; and it follows that progress includes the economic use of fuel, so managed as not to injure quality of product. In the present paper it will be shown how, by suitable utilization of the heat from the furnace, a large portion of it may be made available for other purposes.

Such utilization of waste-gases is important in all industries which employ furnaces (especially heating- and smelting-furnaces); and in the first rank of these stand the iron-, glass-, and porcelain-manufactures. The following pages are devoted chiefly to the waste-gases of smelting-works, and especially to those reverberatory-furnace gases which are not combustible.[1]

The object of metallurgical furnaces is either to fuse the material charged or to bring it to a high temperature required for further mechanical manipulation. Leaving out of consideration for the present the reverberatory fusion-furnaces, we have to do with the heating-furnaces employed in connection with rolls, hammers, or hydraulic presses. The temperature to which the material must be

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[1] For more thorough discussion, reference is made to the work of the present writer, *Die Abhitzkessel*, published in 1913 by W. Knapp at Halle an der Saale.

raised in such furnaces varies, according to quality and use of product, from 800° to 1,200° C., and, of course, the hearth itself must be still hotter, as will be also the gases coming from the hearth (except in the continuous heating-furnace). It follows that the efficiency of such a furnace with respect to the utilization of fuel must be very low, as will appear from the following statement of the matter.

If t_a and t_e be the temperatures at the beginning and the end of the hearth, respectively, η_1, the theoretical ratio of utilizable heat, will be $\eta_1 = \frac{t_a - t_e}{t_a}$. For instance, if the initial temperature be 1,400° C., and the final temperature 1,000° C., the percentage of available heat will be $\frac{1{,}400 - 1{,}000}{1{,}400} = 0.29$ or, roughly, 30 per cent. This calculation makes no allowance for the great loss of heat by radiation and conduction, which will be discussed hereafter.

In the instance given, on the assumption that there was no suction of air through the doors, the combustion-gases have lost, on the way from fire-bridge to flue, say 30 per cent. of their original heat, and there should remain, therefore, some 70 per cent. of the heat generated on the grate, to be made available (if it be possible to cool the gases to 0° C., or to the temperature of the outer air, as the case may be).

The simplest way to utilize this heat would be to prolong the furnace, and move the charge towards the fire, so as to heat it on the counter-current principle. By this method it is possible in the continuous heating-furnace to operate the furnace itself at a lower temperature, thus securing additional fuel-economy. Of course it is not the difference between initial and final temperatures only, but, as already observed, all incidental losses of heat also, which must be considered in maintaining the efficiency of the furnace. Making allowance for the great losses by radiation, which depend upon the temperature of the furnace, one soon arrives at a practical limit of furnace-size, beyond which it is not advantageous to go. The lower the flame-temperature at the end, the greater the efficiency of the furnace.

As to further reduction of chimney-temperature, however, it must be inquired whether the draft may not be thereby diminished. Theoretically, the maximum draft is obtained at about 300° C. of gas-temperature. An example will show how the draft is affected by higher or lower temperatures.

Let us assume that the stack of such a furnace has a draft of

20 mm. water-column, one-half of which is used in overcoming the resistances in the furnace and flue, and on the grate, leaving 10 mm. to affect the speed of the escaping gases. In order to determine the volume or weight of the gases drawn per unit of time through a stack of say 1 sq. m. area, we will consider three cases, assuming the gas-temperature at 200°, 300°, and 400° C. respectively.

Under the conditions assumed, the friction in the stack itself being neglected, we have $v = \sqrt{2gh}$ for the velocity of the gases entering the stack with an effective draft-pressure of 10 mm. water. Substituting for the water-column h_w the corresponding air-column in millimeters, γ being the specific gravity of the chimney-gas and γ_w that of water, we have $\sqrt{2gh_w \frac{\gamma_w}{\gamma}}$ as the velocity of the chimney current. At the given temperatures, the specific gravities of the chimney-gases are approximately:

Temperature. Degrees C.	Value of γ in Kilograms per Cubic Meter.
200	0.7
300	0.6
400	0.5

Calculated upon these figures, the respective velocities of the chimney-gases in the three cases will be roughly 16, 18, and 19 m. per second; and the weight of gas passing per second through the assumed cross-section of 1 sq. m. (no allowance being made for resistance in the stack) will be roughly 12 to 10 kg. at these three temperatures. It appears at first sight that the weight of gas actually moved by the stronger draft is not greater, but, on the contrary, somewhat smaller. But this contradiction is not real; for we have assumed the absence of frictional resistance in the stack, whereas, this resistance not only exists, but may be confidently assumed as greater for the cooler, and smaller for the hotter (*i. e.*, lighter), gas-current. The safe practical conclusion is that, within certain limits, the weight of gas drawn through a chimney remains constant under moderate increase or diminution of temperature. It follows likewise, that a considerable cooling of the gases for utilization of their surplus remaining heat cannot injure the process of combustion, provided the draft in the chimney is strong enough to overcome resistances, and produce the necessary gas-velocity. In most cases, the chimney-draft has to be throttled anyhow; and in cases where blast-pressure (*Unterwind*) is used, there is no room for any fear of disadvantage through reduction of draft.

The temperature of the waste-gases, though varying with the pur-

pose for which the furnace is operated, is usually high enough for the utilization here to be described. According to the method of calculation employed above, the available heat in gases leaving the furnace at 1,000° C. and entering the chimney at 300° C. would be $\frac{1,000 - 300}{1,000}$ or 70 per cent.; that is, under the conditions assumed, about 70 per cent. of the heat of the gases leaving the furnace should be available for other purposes. As a general rule, however, this figure is not reached, because, for reasons of construction and imperative considerations of safety, the boiler which is to be heated by the waste-gases must not be directly a part of the furnace construction, and hence a fall of temperature will take place in the connecting main. Assuming in our example such a fall of 100° C., we have, as the available percentage of heat, $\frac{900 - 300}{1,000}$ or, about 60 per cent.

It is evident at once from this discussion that *the arrangement of furnace and boiler should be such as to involve the smallest possible fall of temperature between the two.* Long mains are therefore to be avoided as far as practicable, since they involve a considerable relative fall of temperature. Fortunately there are many constructions which, in most cases, satisfy the requirements above stated.

The total theoretically available heat-energy of the coal burned on the grate is

$$\eta = \frac{t_a - t_e}{t_a} + \frac{t'_a - t'_e}{t_a}$$

the flame-temperature being t_a at the beginning and t_e at the end of the hearth; t'_a initial, and t'_e final, temperature of the waste-gases. And since (apart from losses between furnace and boiler) $t_e = t'_a$, the formula becomes

$$\eta = \frac{t_a - t'_e}{t_a}$$

In the example given, $t_a = 1,400°$ C.; $t_e = t'_a = 1,000°$ C.; and $t'_e = 300°$ C. Hence,

$$\eta = \frac{1,400 - 300}{1,400} = 0.79$$

whence it appears that by the addition to a heating-furnace of a boiler heated by waste-gases a total theoretical availability of nearly 80 per cent. would be realized, about 30 per cent. being contributed by the furnace and 50 by the boiler. *The theoretically possible utilization of*

heat is thus essentially higher in the boiler than in the furnace therewith connected.

So far we have not considered the various losses in the furnace and at the boiler. But they must necessarily receive attention, since only so can the increase of economy produced by adding to a heating-furnace a suitably constructed waste-gas-heated boiler be placed in the right light.

The modern rolling-mill furnaces, directly fired or with half-gas firing, combine with a maximum of work a utilization of the heat of the gases amounting in certain favorable cases to perhaps 15 or even 20 per cent. Assuming for less productive furnaces an economic degree (*Wirkungsgrad*) of only 10 per cent. (by which is meant the relation between the heat actually utilized in warming the ingot or bloom and the total heat generated on the grate), it appears that of the 30 per cent. available for this work (according to the calculation in our example) only one-third is really brought out. The remaining 20 per cent. of the total heat generated we must regard as lost through radiation and conduction, as well as through incomplete combustion.

The loss by radiation and conduction of a quantity of heat greater than the quantity utilized is an extreme but conceivable case, in view of the high temperatures occurring within the furnace. According to Stefan-Boltzmann,[1] the transfer of heat by radiation varies as the fourth power of the absolute temperature of bodies concerned.

In a boiler of proper dimensions and construction, the conditions are essentially more favorable, since here only the loss by radiation makes itself felt to any considerable extent. Assuming a total boiler-loss of 15 per cent. of the theoretical boiler-efficiency, we shall have, notwithstanding, in our example, an actual realization of 85 per cent. of the available 60 per cent., or about 50 per cent. *Thus, by the addition of a properly constructed boiler to a heating-furnace, the utilization of the fuel-energy may be raised from 10 per cent. to 60 per cent., whence it is again clear that such an addition is imperatively demanded in the interest of furnace-economy.*

Thus far, we have left out of consideration the fuel and the conduct of combustion. But in the operation of the reverberatory, it is, as already observed, necessary not only to maintain the furnace-temperature required by the pieces to be heated, but also, in many cases (particularly in furnaces heating "quality"-steels), to keep the composition of the gases in the furnace such as to exclude, as far as possible, any deleterious action upon the charge, which it must be our

[1] *Stahl und Eisen*, vol. xxxi., No. 40, p. 1640 (Oct. 5, 1911).

aim to protect, not only against overheating, but also against too great oxidation. The flame should carry as little excess of air as possible; so that it seems in many cases better to work with too little than with too much air. In the former case, there would be a loss of unconsumed gases; but this would ordinarily be smaller than the loss occasioned in the latter case by the burning of expensive material in the charge.

The amount of loss from incomplete combustion depends chiefly on the construction of the furnace and the manner of firing. Investigations of this question made by the present writer have given various results, so that (as might, indeed, have been expected after the foregoing statements) no generally applicable figures can be given. In one experiment, no less than 6 per cent. of CO was found in the waste-gases of a furnace heating large blooms. In the very great majority of cases the furnace could be run with, perhaps, from 2 to 3 per cent. of carbon monoxide in the waste-gases, so as to avoid excessive burning of the charge. This loss through imperfect combustion has been omitted from the foregoing discussion because, being so highly dependent upon various working-conditions, it cannot well be expressed in a general formula. That such losses are not exceptional, has been shown by Dr. M. Philips,[3] who found in the waste-gases from a half-gas furnace more than 9 per cent. of carbon monoxide, and also a considerable amount of hydrogen.

Before considering in detail the several kinds of waste-gas boilers, we should inquire why the production of steam is relatively the simplest and easiest, and also commercially the most profitable, way of utilizing the heat of such gases. The other ways which have been proposed are: Preheating the charge; preheating the air for combustion; and preheating the boiler-feed, or superheating the steam, of boilers directly fired.

The preheating of the charge in continuous heating-furnaces has been mentioned already; and it has been shown that there is a practical limit, beyond which the furnace cannot be advantageously lengthened for this purpose.

The heat of the waste-gases is applied satisfactorily to the preheating of air in half-gas furnaces, and of air and gas in gas furnaces.

For advantageous preheating of the boiler-feed, the difference in temperature between the escaping gases and the water to be thus treated is too great. The intensive heating of an ordinary amount of water would absorb but a small quantity of heat, and consequently

[3] *Stahl und Eisen*, vol. xxxii., No. 1, p. 13 (Jan. 4, 1912).

a large quantity of heat would not be utilized, unless much more water were preheated than is called for in such establishments.

It would be easy to superheat steam by means of the waste furnace-gases. Not to mention the fact that the economic result might leave much to be desired, this method of utilizing the hot gases is open to a serious objection, namely, that the operation of the furnace does not uniformly correspond with the consumption of steam. It might happen, for instance, when the engines of the works were using but little steam, that what they received would be too hot; or, on the other hand, when the several engines happened to be all working at once, and thus taking a large aggregate of steam, it might not be sufficiently superheated.

Yet this method is occasionally practiced. At one of the works of the U. S. Steel Corporation [4] a large central superheater was installed, for raising to 240° C. the temperature of 110,000 kg. of steam per hour, at a pressure of 10 atm. This apparatus received the waste-gases from four ingot-heating furnaces and superheated the steam for four roll-trains, requiring a total of 8,000 h-p. Whether such an arrangement can satisfactorily utilize the heat of the chimney-gases may be doubted, especially in view of the fact that in superheating steam, there must be, to secure a satisfactory efficiency per unit of heating-surface, a very great difference of temperature between the heat-giving and the heat-receiving material. If the transfer of heat per square meter of heating-surface is large, then the temperature of the gases leaving the superheater will be high—and *vice versa*.

The heat of waste furnace-gases can be most economically utilized in the production of steam. Steam is used everywhere and always in metallurgical works. Even those which are run throughout by electric power, derived from gas-engines, are provided with steam-boiler plants at least as a reserve, or for use in special operations. By contributing to the general steam-conducting system the steam generated by the heat of waste-gases, the work, and therefore the fuel-consumption, of the directly-fired boilers is diminished. Moreover, the difference of temperature between the hot gases from the furnace and the water under steam-pressure in the boiler is about the same as between the flame and the water in a boiler directly fired, so that the transfer of heat will be as satisfactory in the former case as in the latter.

One objection may be raised. It is well known that the operations of furnace and boiler do not continuously correspond. The times of maximum production of steam by means of the furnace-gases are not

[4] *Scientific American Supplement*, Sept. 9, 1911, p. 169.

always those of the greatest consumption of steam from the boiler. But this circumstance will very seldom be important enough to impair the economy of the method. For, on the one hand, steam is almost always in use somewhere in the plant, and, on the other hand, boilers containing a considerable quantity of water are quite capable of storing temporarily a considerable quantity of heat—especially when (as is almost always the case) the steam-pressure is below the normal.

It may be added that in modern construction the proportion of size between furnace and boiler is very good, so that the two can exist side by side, without making the boiler awkwardly large. A comparison of the heat which can be developed by 1 sq. m. of grate-surface with that which can be taken up by 1 sq. m. of boiler heating-surface, shows that the boiler need be only about as large as the furnace, or, in some cases, may be even smaller.

II. Principles Governing the Choice of a Boiler-System.

The first boilers built for this purpose combined economy of ground-space in the works with the direct upward discharge of the spent gases. The vertical cylinder-boiler stood, as it were, in the enlarged lower portion of a chimney, the hot gases played around it up to a given height, and were there taken up, generally by two diametric sheet-metal flues, at the side of the boiler, and conveyed away, upward. The vertical cylinder-boilers had generally a diameter of from 1 to 1.2 m., and a total length of 12 or even 15 m.

This type, often employed in the '60's of the last century, had two advantages: (1) it occupied, together with the surrounding masonry, only about 6 to 8 sq. m. of ground-space; and (2) its chimney was combined with it—a welcome feature from the standpoint of furnace-management, because it assured the complete independence of each furnace. This form of boiler presents, however, such weighty disadvantages that its use is no longer seriously considered. A very questionable improvement, was the vertical flame-tube boiler of Hall.

Gradually, the consideration of ground-space gave way to that of safety, and horizontal boilers, which could be more easily shut off, were placed behind the furnaces. The so-called Bouilleur boilers (cylinders, with one or more boiling-tubes [*Sieder*]) were first used, and afterwards came fire-tube boilers. These types permitted, through the establishment of a direct connection between furnace and chimney, the operation of the furnace with the boilers cut out of the system; and thus assured a greater safety. The development of heating-surface easily followed, since wide limits were provided for it. The

degree of utilization—in other words, the amount of steam power produced—per kilogram of furnace-coal was greatly advanced by the use of the fire-tubes; the boilers were much more easily served and cleaned than the vertical ones; in short, this would have been recognized as the ideal form for a waste-heat boiler, if it had not demanded such a disproportionate ground-space. While a simple puddling- or heating-furnace occupied, say, from 16 to 20 sq. m., the accompanying fire-tube or Bouillier boiler called for from 20 to 40 sq. m., or almost twice as much as the furnace itself.

The necessary ground-area is smallest when the boiler is placed over the furnace. If it occupies not more, or not much more, horizontal area than the furnace, the question of space retires completely into the background, and the furnace can be designed to suit the conditions of operation with a freedom previously unknown, since furnace and boiler can now be regarded as a coherent whole. This conception seems to have taken tangible form first in America, where water-tube boilers were placed above and behind puddling-furnaces. But in designing such overhead boilers, the special kind and operation of the furnace must be kept in mind. A furnace within which the highest temperatures prevail suffers from these, or through the consequent expansion of its refractory material, certain changes, which the strongest armature cannot prevent. These movements or changes of form must be absolutely kept away from the boilers above; otherwise, the position of the boilers would be altered in the course of time, with a certain lack of safety in operation as a natural result. Nor should it be forgotten that a furnace, according to the demands made upon it, will have to be rebuilt much sooner than a boiler, even when the latter is placed over it. Though furnace and boiler constitute an almost indivisible whole, they must be built separately, so that changes suffered by the furnace may not be communicated to the masonry of the boiler, and thus to the boiler itself. This requirement is satisfied by mounting the boiler upon independent cast- or wrought-iron columns, placed at a certain distance from the furnace-buckstays. The use of these boiler-columns as part of the armature is, for the reasons just given, not permissible. They can be in each case so grouped around the furnace as not to hinder its operation. In many instances the structure thus carrying the boiler may serve as a support for door-levers, etc.

The question of total height for furnace and boiler plays no part in modern plants. Even in extreme cases, the highest part of the boiler would be scarcely 7 m. above the furnace-floor; whereas the traveling-cranes in modern works run on a still higher level.

The relative position of boiler and furnace having been determined, the next question concerns the kind of boiler which will involve the least interference with furnace-operations, while possessing the maximum technical efficiency, and not requiring too much in the way of attention and maintenance. Of the various boilers claiming consideration, we may name the fire-tube, the combined fire- and smoke-tube, the water-tube, and the boilers of locomotive and locomobile form. All of these are more or less suitable for the purpose in view, and it will require special study in each case to decide which should be adopted.

One fundamental difference among them is, that the flame-tube and water-tube boilers require to be set in masonry, while the other types named can do without it, since only their interior surfaces are played upon by the hot gases. In consequence of this masonry setting, boilers of the first-mentioned types sometimes seem somewhat large and clumsy, as compared with the furnaces, whereas the boilers which have interior firing exclusively look much more graceful and small, as indeed they are, since they have, instead of the thick masonry setting, a sheet-metal case encircling them at a small distance. Behind this, the non-conducting material lies upon the mantle of the boiler. By reason of the smaller size of this type of boiler, its use affords some advantages in the better lighting of the works.

III. Recent Waste-Gas Boilers for Directly-Fired Heating-Furnaces.

Fig. 1 shows in section the usual arrangement of a fire-tube boiler above the furnace, as executed by the Deutsche Hüttenbaugesellschaft (the German Furnace-building Company). The hot gases from the furnace traverse first the fire tubes, then a superheating chamber, and then return along the floor and on both sides of the boiler, to be finally conveyed by a sheet-metal flue to the chimney.

A required cutting-off of the furnace from the boiler is effected by the slide *s*, which, in a properly constructed apparatus, could scarcely give occasion for disturbance. When the valve is closed, the hot gases pass from the furnace through the opening *a*, Fig. 1, into a masonry pier, under the sheet-metal flue, and through this to the chimney-flue, which is lined for a short distance with refractory material.

Where the gas or flame enters the fire-tube, the precaution should be taken of providing the first section—say from 1 to 1.5 m.—of the latter with a refractory lining, to avoid injury to the boiler from impinging jets of flame.

Experiments covering more than a month with a furnace of this kind, built for a rolling-mill, gave, according to the above-named furnace-building company, the following results:

Heating-value of the coal-mixture in its original condition	6,192 h. u.
Heating-value of the coal-mixture in air-dried condition	6,763 h. u.
Heating-surface of the boiler	80 sq. m.
Water evaporated by 1 kg. of coal burned on the grate	2.71 kg.
Average evaporation of the boiler per hour for 1 sq. m. surface	12 kg.
Coal-consumption for 100 kg. of pieces, charged cold	10.1 kg.
Total weight of pieces charged per shift	42,000 kg.

In order to determine separately the parts played by furnace and boiler in utilizing the heat from the coal, each must be considered by itself.

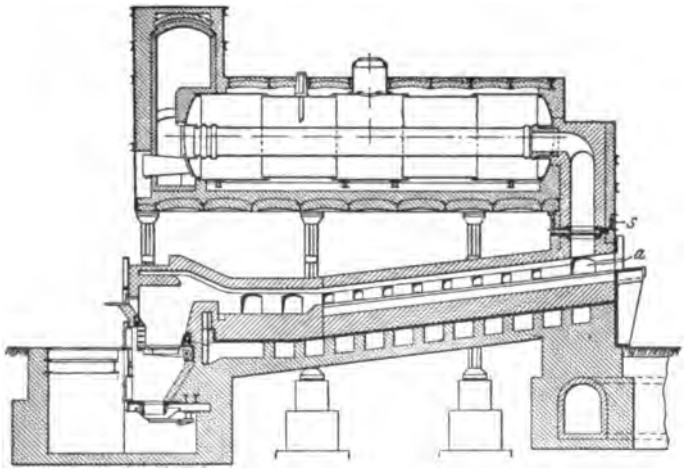


FIG. 1.—SECTION THROUGH FURNACE AND OVERHEAD FLAME-TUBE BOILER OF THE GERMAN FURNACE-BUILDING CO., DÜSSELDORF.

On the assumption of a complete combustion, there were generated about 68,000 h. u., of which, in round numbers, $100 \times 0.168 \times 1{,}100 = 18{,}000$ h. u. were transmitted to the pieces heated. The utilization of heat in the furnace was therefore about 26 per cent., while about 17,000 h. u., or 25 per cent., were consumed in the production of steam. The total heat-utilization was therefore about 51 per cent. These figures have approximate value only, since the temperature of the material as charged and as withdrawn was merely estimated.

If the calculation be pursued further, and 10 per cent. of CO_2 be assumed in the gas at the end of the boiler (after complete combustion), it is found that there is a loss of about 19 per cent. in the gases escaping at about 300° C. from the boiler. Hence the loss by radiation, conduction, and unburned residues is $100 - 51 - 19 = 30$

per cent. That is to say, the loss by radiation, conduction, etc., for both furnace and boiler, is 30 per cent., of which about three-fourths is to be charged to the furnace, and one-fourth to the boiler.

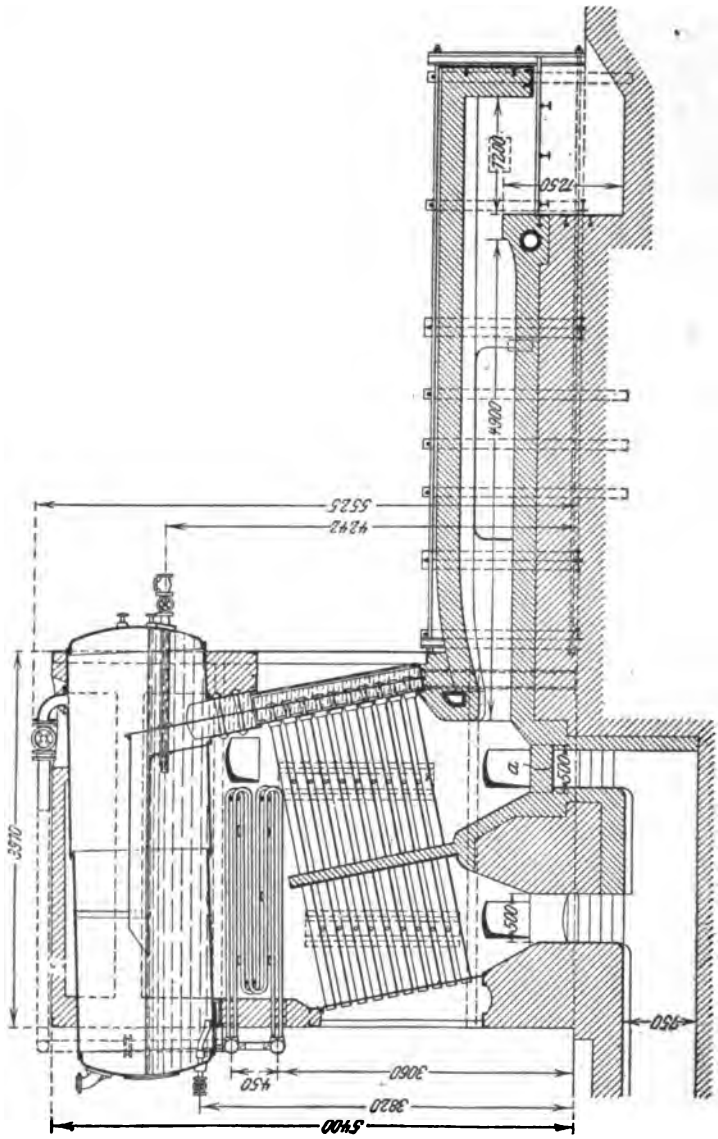


FIG. 2.—JOINT-CONSTRUCTION OF FURNACE WITH WASTE-HEAT BOILER. DÜRR TYPE.

The furnace in Fig. 1 having a long hearth *per se*, there is no discrepancy of size between furnace and boiler; but in the case of a smaller furnace, this boiler (a 2-fire-tube boiler, from 8 to 9 m. long) would seriously over-balance the furnace, or else the boiler would

have to be shortened, with a considerable diminution of heating-surface. But in the presence of a large quantity of gas at high temperature, the heating-surface must be correspondingly large, in order to convey a large amount of heat to the boiler-water. This circumstance has led to the adoption of water-tube boilers, which permit for a given ground-area a great development of heating-surface, and are adapted to the highest steam-pressures.

Fig. 2 shows the combination of a furnace with a Dürr water-tube boiler placed partly over and partly behind it. Since, in this instance, the available space was not sufficient for water-tubes of the normal length of 5 m., it was determined to use a boiler containing several sets of tubes, 3 m. long, and placed over one another, and, moreover, to provide for two passes only, so that the hot gases ascending in the first pass would flow along the superheating-pipes (which were built into the masonry) and the lower part of the boiler-mantle, and then return downward, around the bundle of water-pipes, to the chimney-flue. The possibility of cutting off the boiler from the furnace was, at least partly, secured in this arrangement by building into the wall under the first pass a Schlitz sliding-plate (*a*, Fig. 2), which, during the normal operation of the furnace, is protected by a layer of sand from the direct action of the fire. This does not effect a complete separation of furnace and boiler; but it permits the waste-gases, in case of need, to be drawn, under the first pass, directly into the chimney-main.

Thorough experiments with this boiler occupied 11 days, during which the furnace heated by day the pieces for the rolling-mill (which was not at that time running at night). During the night-shift, the furnace was merely kept warm. The measurements and results were as follows:

Grate-area of the furnace	1.9 sq. m.
Hearth-area (width, 1.8 m.)	8.2 sq. m.
Ratio of grate-area to heating-surface of boiler	1:48
Coal-consumption per sq. m. grate-area for each hour of the working period, about	200 kg.
Average evaporation per sq. m. and hour, as above	13 kg.
Maximum evaporation per sq. m. and hour, as above	17 kg.
Average heating-value of the coal-mixture, about	4,900 h. u.
Average steam-pressure	5 atm.
Average steam temperature	270° C.
Superheating	112° C.
Water evaporated by 1 kg. coal	3.15 kg.
Corresponding heat utilized from 1 kg. coal in evaporation, about	2,170 h. u.
Efficiency of the boiler, about	44 per cent.

As the above figures show, the conditions of operation with this type of boiler-construction were very favorable to a high efficiency. But the union of furnace- and boiler-masonry had a bad effect, causing, in the latter, cracks, dislocations and bulges, which increased the frequency of necessary repairs. Moreover, the longitudinal buckstays of the furnace left much to be desired.

Fig. 3 shows a Dürr boiler, with 120 sq. m. of boiler heating-surface, and 24 sq. m. of heating-surface in the superheater, the whole supported, upon a separate frame of rolled iron, above a large ingot-heating furnace, the hearth-area of which is about 10 sq. m.

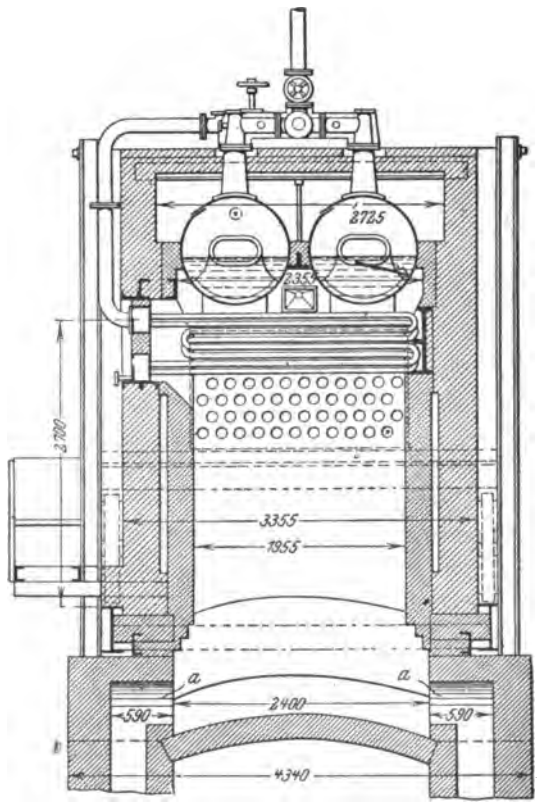


FIG. 3.—WASTE-HEAT BOILER, DÜRR TYPE, OVER LARGE INGOT-HEATING FURNACE.

These boilers, which have been running satisfactorily for about six years, produce per hour per square meter of heating-surface about 11 kg. of steam at 280° C. The furnace-gases enter through two openings (*a*, *a*, Fig. 3) to the boiler, go in three passes over the water-tubes, and then escape into the atmosphere through a sheet-metal

pipe placed between two boilers and carried by the boiler-masonry. Any necessary shutting-off of boiler from furnace is made possible by a fire-proof chimney-flue under the floor of the works, with which the openings *a*, *a*, are connected, and which is ordinarily closed by a slide, so as to permit the normal circulation of the gases to the boiler.

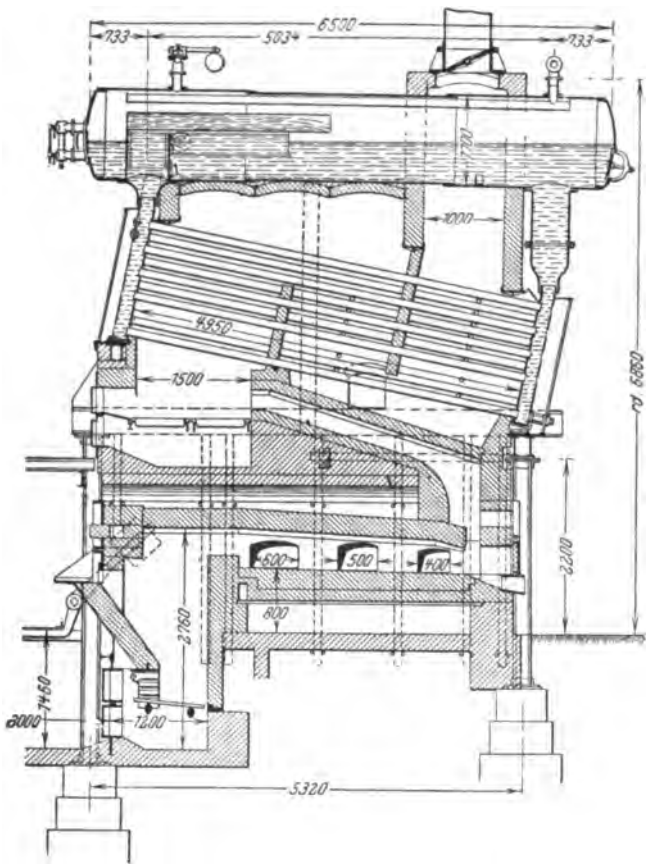


FIG. 4.—WASTE-HEAT BOILER, DÜRR TYPE, OVER SMALL HEATING-FURNACE.

Fig. 4 represents a Steinmüller boiler, with 82.5 sq. m. of heating-surface, and provision for auxiliary direct firing, carried upon a frame over a small heating-furnace. According to the builders, this boiler, heated with the waste-gases only, produces up to 15 kg. of steam per square meter of heating-surface, per hour. With the aid of the auxiliary grate, the product is from 20 to 25 kg. This combination of gas-heat with direct firing is no doubt due to the desire to get as much steam as possible out of the boiler, incidentally utilizing the heat of the waste-gases, and also diminishing the cost of the in-

stallalion per unit of steam-production. But the objection cannot be disregarded, that a separately located and separately heated boiler costs considerably more for attendance than one which constitutes a unit in a battery.

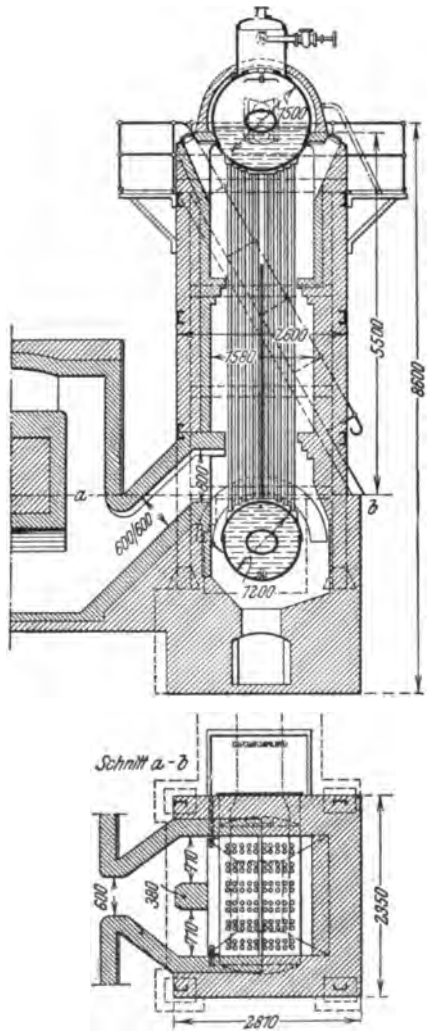


FIG. 5.—GARBE WASTE-HEAT BOILER, COMBINED WITH REVERBERATORY FURNACE.

The steeply inclined or vertical tubular boilers (*Steilrohrkessel*), so much used at present, are well adapted for the utilization of waste-gases from furnaces with working-doors on the side, because in such cases, the boiler can be built directly to the rear end of the furnace—the requirement of floor-space being very small. Fig. 5 shows a

Garbe boiler with 120 sq. m. of heating-surface, attached to a reverberatory furnace, as built by the Düsseldorf-Ratinger tube-boiler works.[5] The boiler shown in Fig. 5 was built without a superheater; but there would be no difficulty in adding one, as, indeed, the same builders have done in other cases.

To meet the most frequent requirement, namely, that the hook of the crane must be brought as near as possible to the furnace-door, so as to facilitate the handling of the heavy ingots, it seems advisable that the outer edge of the boiler over the furnace should at least not project beyond the furnace-armature. Since, for water-tube and fire-tube boilers, the surrounding masonry occupies a large part of the available width, attention is naturally drawn to the saving of space by the use of boilers which do not need masonry. Such tubular boilers approach the locomotive, locomobile, and marine types.

In locomotive boilers, the furnace-flue enters the fire-box from below, so that the heating-gases go directly into the fire-box, and thence into the boiling-tubes. In accordance with the nature of the locomotive boiler, the gases pass through it in one direction, and escape through a sheet-metal chimney, which branches from the smoke-chamber above the firing-door. In this case, the boiler can be shut off from the furnace only when there is a second flue, leading from the main furnace-flue to a reserve chimney-flue. During the cleaning or repair of the boiler, this reserve conduit conveys the gases to a chimney, which may serve several furnaces, if necessary. But all this makes the design costly and complicated. Moreover, it must not be forgotten that the locomotive boiler, by reason of the great frictional resistance encountered by the gases in passing through the usually small heating-tubes, requires a strong chimney draft. It is therefore advisable, in order to avoid this evil, to choose heating-tubes of larger diameter, sacrificing thereby a few square meters of heating-surface.

For welding-furnaces with waste-gases of very high temperature, the locomotive type of boilers is not well adapted, since the pointed flame from the furnace strikes directly upon the tube-walls, and might easily make them leak. This type is likewise unsuitable where only a natural chimney draft is employed, because, when the furnace-doors are opened, the entrance of cold air tends to produce leakiness of the tube-ends.

Fig. 6 shows a boiler once designed by the present writer to be

[5] Such boilers, each with $2 \times 250 = 500$ sq. m. heating-surface, were built in 1912 at the "Phoenix" works, to utilize the waste-gases of Martin open-hearth furnaces. See description in later pages.

heated by furnace-gases. It is a variant of the marine boiler. A short vertical flue (usually 2 m. long) leads the gases from the furnace-flue into a corrugated tube, *b*, lying along one side in the boiler. The first 0.6 m. of this tube are lined with refractory material. The gases pass through the vertical flue *a* and the tube *b*, into a reversing chamber, *c*, in which a superheater, *d*, may be advantageously placed; and from this chamber they pass back through the boiler in the small boiling-tubes, *e*, which, like the corrugated tube, lie along one side. Finally they are received, at the end at which they entered, in a smoke-chamber, *f*, of thin sheet-metal, and are conveyed at a greatly reduced temperature through the vertical flue *g* to the subterranean chimney-flue *h*. To make all parts of the boiler accessible, the superheating coils are placed in the reversing-chamber opposite the corrugated pipe, so that when it is necessary to tighten up the smoke-pipes, or clean them from soot, they can be easily reached from both sides after the corresponding doors have been opened. The inside of the corrugated tube itself is accessible through the door *k*, which has a peep-hole, *i*.

A direct connection of the main flue, *a*, with the flues *g* and *h* is established by breaking through the thin wall *m*, separating *a* from *g*. When this is done, the furnace-gases will be drawn through *g* and *h* into the chimney, without touching the boiler at all. The introduction of valves, etc., for this purpose was avoided, because they would not have endured the high temperature which, at one point in the main flue, often melts the refractory lining. It should be added that, in later constructions of this type, the boiler rests upon a grating of beams, carried by six wrought-iron columns. Since the temperatures in the corrugated tubes are much higher than in the small tubes near it, care must of course be taken that the latter do not become loosened in the tube-walls through expansion and contraction. This consideration affects the design of the corrugated tube and the length adopted for the small tubes. Boilers of this type have been erected in considerable numbers at various works, and have run satisfactorily for several years.

Fig. 7 shows a modification of the type, in which the gases finally escape through a sheet-metal chimney over the boiler. A direct connection between furnace and chimney can be easily effected in this case, as in the preceding. It is only necessary to line both the smoke-chamber and the chimney with refractory material, and then to provide either a sliding cut-off or a removable separating wall, as shown in Fig. 6.

Experiments conducted upon a boiler about like that of Fig. 6,

through 45 days (90 shifts) of the running of the heating-furnace on small forgings, gave the following results:

Grate-area of the heating-furnace	1.2 sq. m.
Hearth-area (width, 1.3 m.)	4.0 sq. m.
Distance from mid-hearth to beginning of fire-tubes	6.5 m.
Length of connecting flue between furnace and fire-tubes	2.5 m.
Total length of furnace	6.2 m.
Total width of furnace	1.8 m.
Heating-surface of boiler	61.0 sq. m.
Heating-surface of superheater in smoke-chamber	5.6 sq. m.
Proportion of furnace grate-area to boiler heating-surface	1:51
Total length of boiler	5.25 m.
Diameter of boiler	1.7 m.
Diameter of flame-tube	0.7 to 0.8 m.
Consumption of coal in 45 days (9 shifts)	234 met. tons.
Consumption of coal per hour, about	217 kg.
Consumption of coal per hour per sq. m. grate-surface, about	181 kg.
Consumption of coal per hour per sq. m. boiler heating-surface, about	3.56 kg.
Amount of water evaporated during experiments	594 met. tons.
Amount of water evaporated per hour	550 kg.
Amount of water evaporated per hour per sq. m. boiler heating-surface, about	9.0 kg.
Amount of water evaporated by 1 kg. coal	2.54 kg.
Average steam-pressure	5.7 atm.
Average temperature of superheated steam	310° C.
Superheating	148° C.
Temperature of boiler feed-water, about	20° C.
Quantity of heat transmitted to 1 kg. superheated steam, about	710 h. u.
Quantity of heat consumed in steam-production from combustion of 1 kg. coal	1,800 h. u.
Average heating-value of coal consumed	5,100 h. u.
Hence, proportion of heating-value utilized in making steam, about	35 per cent.
Temperature of waste-gases escaping from boiler, about	320° C.

In another experiment, lasting only through three shifts, an evaporation of 2.85 kg. water per kilogram of coal was reached. This would be a utilization of nearly 40 per cent. It should be observed that these experiments were made upon a boiler somewhat too small for the furnace, on which account the results are not particularly remarkable. Most of the boilers afterwards erected of this or similar type had about 80 sq. m. of boiler heating-surface, and from 10 to 12 sq. m. of heating-surface in the superheater, with which dimensions a steam-temperature of 400° C. was often attained, under favorable conditions of furnace-operation.

Among the chief advantages of this type of boiler are its small space-requirements, easy adaptation, and total freedom from masonry.

In case of a great fall in temperature but a comparatively small

quantity of heat, which would not warrant the building of a boiler, the heat of the escaping gases can be utilized, if the locality be suitable, and the need sufficient, in a superheater, placed behind or (very easily) over the furnace. The steam-temperature can then be regulated in a highly simple, though not economical, fashion, by admit-

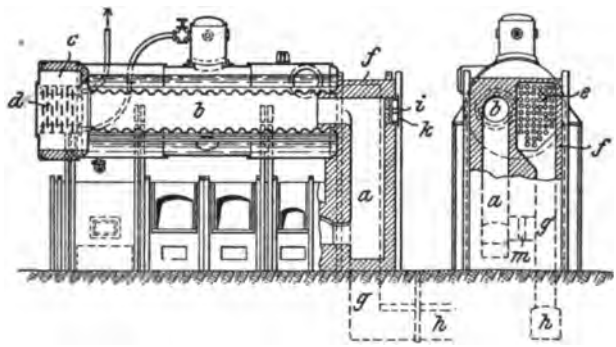


FIG. 6.

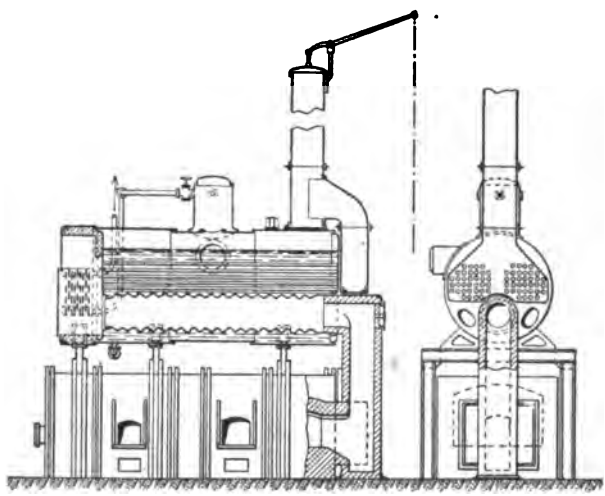


FIG. 7.

FIGS. 6 AND 7.—WASTE-HEAT BOILERS APPROACHING THE MARINE TYPE.

ting air into the superheater through two holes in the masonry. This strongly cools the hot gases, and correspondingly lowers the steam-temperature.

As a general rule, however, when the heating surface has been properly determined, such special means of regulation are not re-

quired, since the quantity rather than the temperature of the chimney-gases is likely to vary.

That, moreover, the utilization of the hot gases in preheaters may be advisable even when their temperature is considerably below 600° C., is shown in Fig. 8, which represents such an arrangement, applied to the waste-gases from the boiler-plant, as well as from four sheet-heating furnaces. This plan works satisfactorily, and up to expectation.

An inquiry addressed to several works using hot waste-gases under boilers established the following:

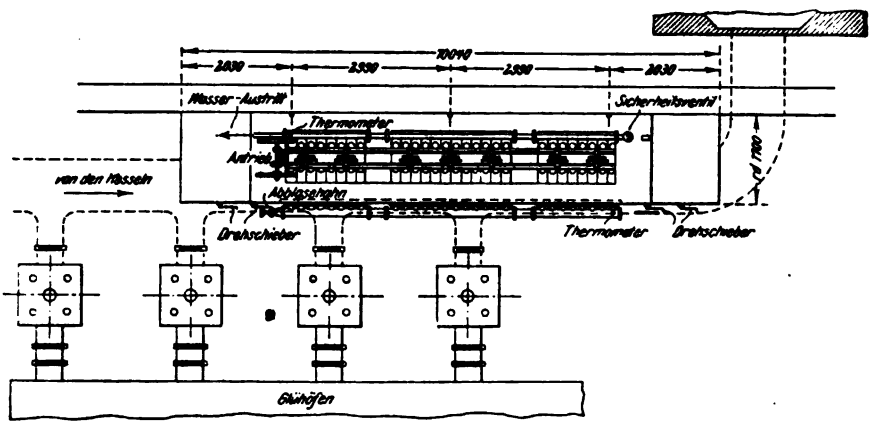


FIG. 8.—PREHEATERS CONNECTED WITH SHEET-HEATING FURNACES.

Precise tests of efficiency had been made almost nowhere, so that only partial reports of the results of practice accumulated in the course of time were available. But these reports were more valuable for the expert operator than the figures obtained by any special tests. They showed in general, that with puddling-furnaces, from 2.5 to 3.5 kg. of steam per kilogram of coal were generated under boilers having from 40 to 80 sq. m. of heating-surface—the quantity of steam produced per square millimeter per hour being from 10 to 15 kg.

With the gases from welding-furnaces, boilers having up to 130 sq. m. of heating-surface produced from 7 to 12 kg. of steam per square meter per hour, representing from 3.5 to 5.5 kg. of steam per kilogram of coal. Of course these figures are subject to great variation, according to the amount of material heated in furnace per unit of time, the nature of this material (ingot-iron or wrought-iron), and its condition (hot or cold) when charged. Upon the production of steam per square meter of heating-surface per hour, the size of the boiler also has an effect.

In the operation of heating-furnaces for hammers, and especially also in furnaces heating thin sheets, it is advisable to introduce fresh air (preferably preheated) into the gases before or in the boiler, in order to effect the combustion of still unburned gas, and thus increase the steam-production.

A similar use of the hot waste-gases from zinc-furnaces has been introduced very recently, and promises to receive a rapid extension, the results so far having been very satisfactory, indicating an increase of from 15 to 20 per cent. in the utilization of the coal. In the cement-industry, the recently adopted revolving tube-furnaces permit the use of boilers, located behind them, for this purpose. The glass-manufacture has long utilized in the same way the waste-heat of its furnaces, even installing, for the preheating of water, apparatus which employs only the radiant heat of the furnace-arch.

The foregoing discussion shows that the utilization of the hot chimney-gases from directly-fired reverberatory furnaces is in very many cases practicable, and in not a few cases highly economical.

IV. The Utilization of Waste Heat from Regenerative Furnaces.

This department of the subject is incomparably more important than the preceding, because the number and the size of the Siemens-Martin furnaces are so much greater than those of the heating-furnaces for rolling-mills and hammer-forges described above.

In regenerative furnaces, there is already a partial utilization of waste heat for preheating gas and air; but its principal purpose is to raise the combustion-temperature in the hearth by raising the temperature of the entering gas and air; the recovery of heat thus effected is not very great, compared with the amount which still escapes unused; and it is therefore worth while to inquire whether a saving of further heat, which would otherwise escape to the chimney, can be effected without injury to the operation of the Martin furnaces.

The quantity of heat escaping at present, for the most part without hindrance, from these furnaces is very large—about 30 per cent. of the total heat employed (according to Professor Mayer's experiments, 29 per cent.; according to Springorum, 32 per cent. These figures come from well-managed works, so that 30 per cent. as an average is not too high).

To these losses, due to the high temperature of the escaping gases, is to be added another, caused by radiation in the furnaces and chambers, which may be estimated at 40 per cent. From these figures we

may deduce approximately the economic effectiveness of such furnaces.

Pfoser in Achern has attempted to utilize in part the radiant heat of a furnace arch, as shown in Fig. 9, which represents such an arrangement for preheating water, placed over the arch of a glass-furnace. This preheater has about 10 sq. m. of heating-surface, and consists of a coil of pipe, over the furnace-arch, through which the water passes. With glass-furnaces, where this method of preheating has been considerably used, it has been found to transfer about 300 to 500 h. u. per square meter of heating-surface. This cannot be called a very great success, but it makes a beginning; and it remains to be seen whether a better way of recovering the radiant heat may not soon be attained. Meanwhile, the placing of a coil full of water on

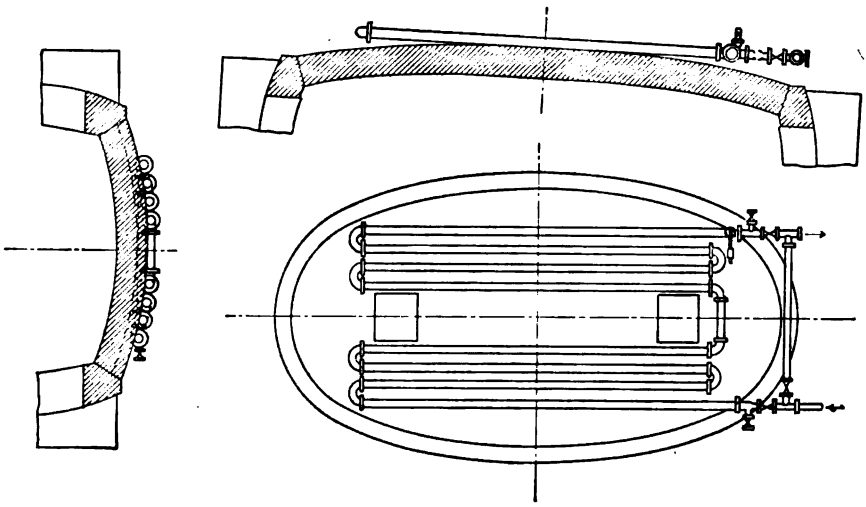


FIG. 9.—PREHEATER FOR WATER, UTILIZING THE RADIATED HEAT OF THE ARCH OF A GLASS-FURNACE.

the top of a furnace-arch is rather a ticklish business; and many a manager would not risk it. Much easier and simpler is the recovery of heat from the waste-gases, as described below.

The average temperature of the escaping gases of the open-hearth, according to numerous determinations at various works, is about 700° C., or from 200° to 300° below that of the heating-furnace gases considered in the earlier parts of this paper. Hence we have to reckon with a somewhat smaller possible recovery, inasmuch as the transfer of heat through heating-surfaces will be smaller. Yet such a recovery is not only practicable but profitable, because the quantities of gas upon which it is carried out are almost always very large.

Prof. Dr. Mayer found in his experiments upon the heat-economy

of the Martin furnace that during the production of 1,000 kg. of steel 667,000 h. u. escape in the hot waste-gases. The investigations of Mackenzie on an entirely different and differently operated furnace gave a loss of 1,000,000 h. u. In both cases the temperature of the waste gases was 700° C.

Of course it would be impossible to recover in any way the whole of this heat. After treatment, the gases would always escape at last at some temperature or other; and, moreover, every transfer of heat involves some loss.

To be perfectly safe, and to reach values which can be, with certainty, verified in practice, we shall use the somewhat unfavorable figures given by Mayer.

But first we must consider certain characteristic conditions of the Martin process itself, in order to avoid the great mistake of proposing any method which would react injuriously upon that process.

The first requirement is that for a given heating surface the boiler should be as small as practicable, in order, on one hand, to permit its installation where available space is limited, and, on the other hand, to reduce to a minimum the loss through radiation and conduction.

Furthermore, the construction and operation of the boiler should be, if possible, such that no interruption for the purpose of cleaning it will be required during the entire furnace-campaign of from 250 to 350 days. This requirement can be met by the use of pure water (surface-condenser water), and by such a construction as will permit outside cleaning of the boiler during operation.

The construction should also be such that occasional explosions, occurring when valves are reversed, will not injure the boiler. This condition is fulfilled by making the boiler-structure of a large number of single elements, with large open areas between them, so that the pressure from an explosion may pass through without doing harm. Moreover, these single elements must be able to resist occasional gas-puffs.

For this, as well as the first-named reason, boilers holding a large amount of water are out of the question.

Finally, the boiler, which is placed in the chimney-flue of the Martin furnace, must not present too great a resistance to the passage of the waste-gases. The reason for this requirement is not alone that the draft might be injured, and the chimney might prove inadequate; for the chimney does not play so very important a rôle when artificial draft is at hand. The chief reason is, that the pressure from a possible explosion should pass between the single members of the boiler-structure, without injury to the boiler.

The obvious requirements, that the boiler should not require too much attendance and maintenance; that safety of operation should be secured; and that the cost of installation should not be too great —need only be mentioned in passing.

All these requirements together can be met by a boiler, the elements of which are made of water-tubes; for, in the first place, such boilers combine large heating-surface with small bulk, and can be operated in a given case with small contents of water; secondly, the outside cleaning of such heating-surfaces is easily performed, if necessary, while the boiler is running; thirdly, the third and even the fourth requirement above stated can be satisfied by a suitable arrangement of the tubes; and fourthly and lastly, such heating-surfaces are comparatively inexpensive, and, when design and shopwork are good and solid, are highly safe against interior and exterior pressure.

In a boiler for the utilization of the hot chimney-gases of Martin furnaces in the production of steam, we distinguish the boiler proper, the superheater, and the preheater.

It is well to place the superheater before the boiler, or at least at the beginning of the draft, and the preheater behind the boiler. In calculating the necessary heating-surfaces, it seems best to begin with the preheater, then to calculate for the superheater, and to end with the calculation for the boiler. In my book, *Die Abhitzkessel*, I have given such calculations for a 30-ton Martin furnace, assuming an hourly steam-production of 2,000 kg. The several heating-surfaces were: preheater, 125 sq. m.; superheater (250° C. steam-temperature), 10 sq. m.; boiler, 200 sq. m.

But since, after utilization, the gases would be so cool that the chimney-draft could not overcome the increased resistances, it is self-evident that such utilization is practicable only when natural draft is not relied upon, but the cooled gases are moved by an exhaust-fan.

This would require in the case named a theoretical expenditure of 6.4 h-p. But in view of the comparatively low efficiency of fans of this size I have assumed for safety as required to draw off the chimney-gases, 40 h-p.—which would represent a fan-efficiency of less than 20 per cent.

The calculation of profit from such an installation, costing 36,000 marks, is as follows:

Average steel production per hour	5,166 kg.
Steam-production from 340 sq. m. aggregate heating-surfaces of attached boiler, in terms of steam at 10 atm. and 250° C.	2,000 kg.
Work of fan per hour	40 h-p.
Cost of plant, including fan	36,000 marks.

Assuming the works to contain six furnaces, of which five are constantly in operation, we shall have:

Cost of the boiler-plant	216,000 marks.
1. Amortization, 10 per cent., and interest, 5 per cent	32,400 marks.
2. Attendance and repairs	7,600 marks.
3. Cost of power, $5 \times 40 = 200$ h-p. at 4 pfennige for 1 kw-hr	50,000 marks.
Total annual costs	90,000 marks.

Since in the five units 10,000 kg. of steam are produced hourly, or 240,000 kg. daily, or, in round numbers, 77,000 metric tons per annum, the cost of this steam is about 1.20 marks per 1,000 kg.

If we now consider the work done with this steam through a steam-turbine, we find that at the rate (including all losses) of 7 kg. of steam per kilowatt-hour, the 10,000 kg. produced hourly will generate about 1,400 kw-hr., of which (150 kw-hr. being deducted for the driving of fans, etc.) 1,250 kw-hr. per hour, or 30,000 per day, or say 9,000,000 per year, remain available, at a cost of 40,000 marks for steam-generation and 40,000 for the turbine-plant, or 80,000 marks total annual cost. In other words this electrical energy is obtained for less than 1 pfennig per kilowatt-hour.

No other way of producing electrical energy can show a lower cost. Even water-power, favorably located and unfailing in adequate supply, is seldom so cheap, if the amortization of the plant be provided for at about the rate assumed in the foregoing example.

It must be noted also, that the generation of electric energy in the works—that is, in the place where it is to be used—is independent of the season or weather. A given production of steel gives a corresponding, and (apart from minute variations) almost uniform quantity of energy in this form.

It is praiseworthy to try to make use of the water-powers of which so many are still neglected; but the *falls of temperature* which occur before our eyes deserve our attention likewise, since they are easier to appropriate, possess more constant strength, do not need long lines of transmission, and can therefore be made available at smaller expense of installation. Moreover, the utilization of these great temperature-falls in large steel-works recovers enormous quantities of heat, otherwise wasted.

Further results of calculation are graphically shown in Fig. 10.

Thus far, only the theoretical determination of the heating-surface areas and the probable costs and profits have been considered. The

records of actual practice with waste-heat boilers attached to Martin furnaces are likewise interesting.

In the Duisburg steel-works of the Phœnix Co., five such boilers were erected last year behind Martin furnaces, and put in operation; and thorough tests were made under the direction of the Imperial

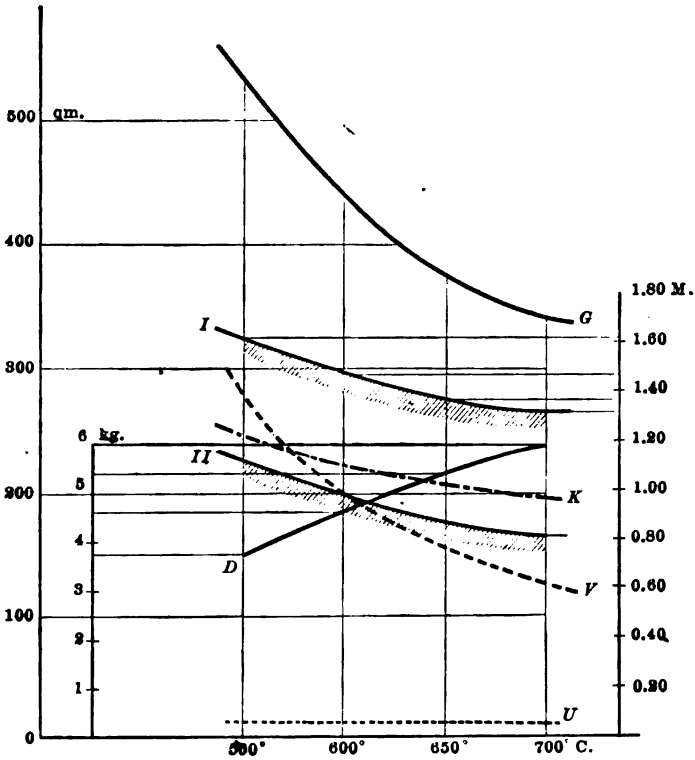


FIG. 10.—DIAGRAM SHOWING HEATING-SURFACE AND COST (INCLUDING INTEREST AND AMORTIZATION) OF PRODUCING 1,000 KG. OF STEAM AT 10 ATM. AND 250° C., FOR VARIOUS TEMPERATURES OF THE ENTERING GAS AND AN ASSUMED GAS-PRODUCTION OF 2,000 KG. PER HOUR.

- - - - - - - - V = Necessary Preheating Surface in sq. m.
—·—·—·— K = Necessary Boiler Heating Surface in sq. m.
..... U = Necessary Superheating Surface in sq. m.
I = Cost of 1,000 kg. steam, with an electric power price of 4 pfennige per kw-hr.
II = Cost of 1,000 kg. steam, when the electric power driving the fans is furnished by the plant itself. This curve shows therefore the steam-cost proper.
———— D = Steam-production per sq. m. of total heating-surface.

German Steel Works. From the report of these tests by Chief Engineer J. Schreiber, which was published this year in *Stahl und Eisen*, I present here only the average figures of a series of experiments, last-

ing through 20 shifts, upon a boiler erected behind a 50-ton furnace (No. 4).

Average temperature of boiler feed-water, about	20° C.
Average temperature of gases entering boiler, about	700° C.
Average temperature of gases leaving boiler, about	350° C.
Temperature-fall of gases in boiler, about	350° C.
Average steam-pressure in boiler	7.5 atm.
Consumption of current in operating fan per hour	57 kw.
Product of steel during experiment	2,000 met. tons.
Product of steel per hour	8.3 met. tons.
Coal-consumption per ton of steel made	234 kg.
Evaporation per sq. m. heating-surface (without preheating)	7 kg.
Steam-production per ton of coal	1,842 kg.
Steel-production per ton of coal	449 kg.
Heating-surface of boiler	500 sq. m.

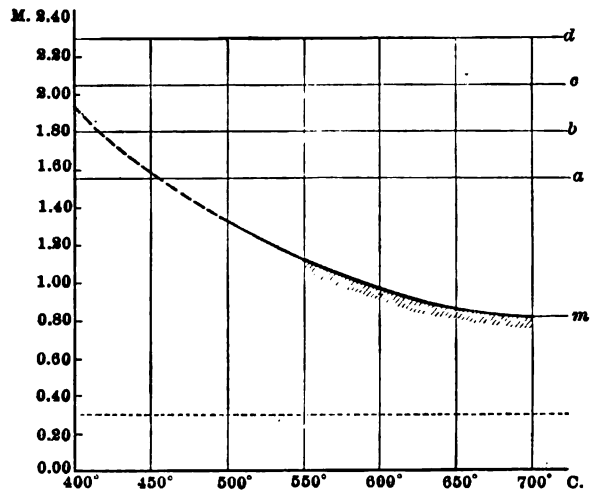


FIG. 11.

The great economy of such a utilization of waste heat appears very clearly from this statement, especially if it be considered that, even in this first undertaking of the kind, not the least injurious effect of the boiler upon the operation of the furnace was noticeable.

Finally, Fig. 11 exhibits graphically a comparison between the cost of 100 kg. of steam produced directly by coal combustion and the corresponding cost of production by means of the waste heat in Martin furnace-gases. In this diagram, the horizontal lines give the cost of 100 kg. of steam from direct combustion, according to the varying prices of coal, while the curve shows the cost of the same quantity of steam produced from waste-gases of different temperatures, as shown along the bottom line. The dotted horizontal line

next above shows the approximate proportion of the total cost required in the use of directly-heated boilers to cover interest, amortization, and labor. The costs are given on the left, in marks and decimals, the interval being 0.20 mark.

The intersections of the horizontal lines by the curve mark the limit of economy in utilizing the waste-gases. It will be seen that this limit is extended in proportion as the temperature of the waste-gases or the price of coal is increased.

The Use of Pulverized Coal as a Fuel for Metallurgical Furnaces.

BY H. R. BARNHURST, ALLENTOWN, PA.

(New York Meeting, October, 1913.)

IT would be a difficult matter to trace from the beginning the very few improvements made in the burning of fuels prior to 1860. Doubtless the crossing of the sticks of wood in building a wood fire early appealed to our savage ancestry as a means of expediting its effectiveness for their crude culinary purposes. Stones and andirons came later in assisting the admission of air to the under part of the fuel bed, while beacons and cressets utilized metal bars as supports for the same purpose. It is probable that Tubal Cain owed his success to the use of bellows making possible the use of mixed fuel and charge upon a hearth. With the development of working in cast iron, grate bars came in, and they are still with us doing duty in every household and almost every art where heat is a factor.

Little was done beyond these crude methods prior to 1860. With the advancement of knowledge came the fuel-gas producer, a natural child of the retorts employed in supplying illuminating gas.

It is only within the last 15 years that the conclusion became general that complicated systems for saving heat would not remedy deficiencies in its primary development, and within that time there has been great advancement in the means of producing initially a fire of high efficiency, so that to-day we are moving rapidly toward both high development and high absorption, with the resultant of high efficiencies.

In studying the subject of the availability of pulverized coal as a fuel, it may occur to ask, why has this not come up earlier? This question would be naturally prompted by recalling the fact that Thomas Crompton's English patents upon his methods were issued to him nearly 40 years ago. A conclusive answer to this, however, is that Crompton at that time had very crude facilities for making what was at best a very imperfect pulverization of the coal, and the expense of preparation was very great. He does not seem to have been aware of the advantage of a thorough drying of the coal in the

effort to reach high efficiencies. In short, while the possibilities of high results were indicated, the means of reaching them was left for later developments.

It was not until the pressing demands of the cement industry for a low-priced fuel of high efficiency became imperative that pulverized coal was found to fulfill almost ideal conditions. In this industry the grinding of cement materials cheaply, to a high degree of fineness, was early found to be a necessity. The finer the grinding the better the cement produced by it. So pulverizing coal as a fuel fell naturally into the hands of men able to powder cheaply a material much less refractory than cement rock or clinker, and to the degree of fineness which would show the best results.

It was early noted that the more thorough the drying and the finer the coal was ground, the higher the results. Of course, there is a limit of practicability.

Coal properly ground will burn thoroughly if 85 per cent. passes through 200 mesh and 95 per cent. through 100 mesh, the measuring screen apertures squaring $\frac{1}{400}$ and $\frac{1}{200}$ in. respectively. I use the term "properly ground" to express the condition that the coal thus pulverized shall contain a high percentage of ultimate fine dust practically unmeasurable.

The author has found in such properly ground coal a percentage above 70 capable of passing through 300-mesh screens, squaring $\frac{1}{600}$ in. on the side. It may be presumed that these percentages progress relatively in the further reductions contained, as the larger screens, of course, pass all such fines without rejections. There appears to be no practicable means of measuring the highly divided particles.

As we can burn all the coal thus prepared, including the rejections when the percentages named pass the 200-mesh and 100-mesh screens, there seems to be no good reason for pushing pulverization beyond this point. Coal can be cheaply brought to this condition and the mills able to do this work have large capacity. Higher percentages may be reached by the sacrifice of capacity, and consequently economy. This standard of 85 per cent. through 200 mesh and 95 per cent. through 100 mesh is a practicable commercial standard and should be maintained.

It may not be amiss at this point to describe the conditions contributory to the high success attendant upon the use of pulverized coal in cement kilns. Two objects are in view in this part of the cement-making process: 1, driving off of the carbon element in the carbonate of lime used as a material; 2, the subsequent formation of clinker nodules by partial vitrification.

In an inclined kiln, say 7 ft. internal diameter and 140 ft. long, the cement material is delivered at the top of the incline, which it descends by the rotation of the kiln. The carbon of the lime is driven off at a comparatively low heat, after which the charge approaches the zone of greatest thermal activity and is there "clinkered," or partly vitrified, at a very high heat. The fuel is fired at the lower end of the kiln and passes up in reverse direction to the progressing charge coming down. The walls of the kiln, as well as the charge, absorb the heat of the passing gases and communicate it in degree to the charge when by rotation the hot walls pass under it. The hot zone of greatest activity is some 10 to 30 ft. from the point of fuel admission.

The lower end of the kiln, immediately adjacent to the fuel admission, receives but little heat directly from the fuel, but it is nevertheless very hot from the mass of nodulized clinker coming at high heat from the hot zone and heating the kiln walls by transmission. The drop in temperature is estimated to be from 2,900° in the hot zone to 2,100° at discharge.

The clinker cascades as it advances through the rotating kiln. This forms at the lower end of the kiln a heated chamber into which the fuel is injected, so that deflagration takes place quickly.

This hot chamber is a condition that must be imitated in an attempt to use the fuel in other conditions. Cement burners, however, do not find it necessary to deflagrate the coal close to the nozzle of the entering pipe. Their control of the admission of air is not complete, hence they rely upon the heating of the air coming in mainly at the discharge opening, passing there over the clinker and subsequently through 10 or 15 ft. of the heated kiln. This assists in cooling the clinker somewhat and in preheating the admitted air which subsequently supplies combustion.

Cement burners make little attempt to control the volume of the air supply. It is difficult under the existing conditions. There is no doubt that better results would attend such control.

As a justification for this extended description of the process of cement burning, the author submits that of the 90,000,000 barrels of cement which will probably be produced in 1913 in the United States, at least 72,000,000 barrels are being burned with pulverized coal as the fuel. As the average quantity of coal burned is at least 100 lb. per barrel, or 20 barrels burned with 2,000 lb. of coal, the total use of pulverized coal in this industry amounts to at least 10,000 short tons per 24 hr. This, coupled with the fact that it is not unusual for rotary cement kilns to be in continuous operation for from 12 to 15

months, would certainly establish confidence and remove doubt as to the reliability of this use of coal.

Undoubtedly the proper method of firing pulverized coal is to admit with the fuel the exact quantity of air necessary to the result to be attained, as shown by observation, and to maintain the relationship between the fuel and air quantities as long as conditions desired are being fulfilled.

This matter of complete control of the two factors, fuel and air, is and will be at the root of all success with pulverized or sprayed fuel in metallurgical processes.

It is unfortunate in the present state of our arts that it is difficult to obtain very exact readings of the temperatures attainable in the burning of fuel. We do know, however, that a certain quantity of air will deliver the oxygen required to give the highest attainable heat from a given fuel. With a knowledge of the components of a fuel, the laws of thermo-chemistry inform us not only of the quantity of oxygen we must have, but the theoretically attainable temperatures.

Applying these laws further, we learn that any air or oxygen supplied in excess of these requirements simply dilutes the products and lowers the temperature; also, that insufficient air and oxygen will burn part of the fuel to CO_2 and part of it to CO, and with the air supply halved we obtain only the poisonous and inflammable CO. However short we may be of pyrometers, there is in the eye of the intelligent operator a gauge which tells him at a glance whether the heat he has is serving his purpose. Pulverized fuel has a great advantage in this respect.

It need not be supposed that an operator must be perpetually adjusting his apparatus. If we find that with the air-gate fixed at a certain opening the fire is too hot, a simple reduction in the quantity of fuel admitted lessens the source of heat and changes the ratio of the air to the fuel. If not hot enough, more fuel gives more heat units entering and a lessened excess of air, resulting in a heightened temperature.

In all probability, it will be found that excess air must be admitted constantly to keep the heat from reaching destructive limits. With control of both the quantity and quality of heat, this danger is negligible.

If we consider the burning of coal as shoveled or fed in bulk, it must be conceded that a certain degree of comminution or pulverization takes place in the fire as a necessity of combustion. Coal does not burn in lumps, but its ash comes away pulverized, and this gradual pulverization occurs in the fire at the expense of some of the heat

units in the work done. As this is done slowly, it is often necessary to supply large grate area so that the collective surfaces exposed for disengagement of heat shall be sufficient for the purpose for which the fire is used.

To be classed as a fuel a material must be able to give out more heat than it receives. No fuel will burn until its particles are brought to this self-supporting condition by the heat absorbed from some particle previously burned. Not only this, but the oxygen must be heated likewise to a combining temperature. This involves equally heating the accompanying nitrogen. This heat must be passed from substance to substance in increments small in themselves but collectively as large as the occasion demands.

In the use of pulverized coal, therefore, we have best prepared this fuel for the absorption and evolution of heat, but in addition we prepare the air by practically a similar subdivision for joining in the process.

The sequence of events in combustion is now conceived to be as follows: The volatile carbonaceous elements of the fuel are first disengaged, producing highly combustible hydrocarbons, and these in turn combine with the oxygen, burning to CO_2 and H_2O, disengaging heat enough to cause the same sequence of happenings to the fixed carbon components. This reasoning seems to be credible, and is based upon the heating not only of the substances of the fuel but of the oxygen, all of which must be raised in temperature to the point from which heat development can proceed in excess of heat absorption.

It must be evident that comparatively large masses of fuel supplied by large volumes of air will, for reasons simply mechanical, fail in efficiency. This is more particularly so where a large content of volatile matter is set free by contact with another mass of incandescent fuel and heated surroundings. Under such conditions it would seem impossible to get the best results from any fuel. The sweeping off of volumes of volatile gases by large volumes of insufficiently heated air produces smoke. This smoke represents but a small weight of carbon unburned, but indicates a condition in which gases of large quantity pass off uncombined. A heavy draft pressure accentuates this condition, and records are plentiful of a large passage through fires of oxygen which has failed of its duty from lack of heat as preparatory to combination. Carbon monoxide is necessarily liberated into an atmosphere of incombustible gases deficient in oxygen, rendering difficult the further establishment of active combustion.

It does not require many moments' thought to perceive that a pulverized fuel, the particles of which are each surrounded by a minute

envelope of air, sufficient to burn it thoroughly, is an ideal fuel under ideal conditions. In projecting thus a cloud of fuel into a highly heated chamber, each particle because of its opacity becomes an absorbent of heat radiating not only from the chamber walls, but from each neighboring particle as it inflames. This inflammation progresses with rapidity almost inconceivable. Pulverized fuel injected thus with its air supply at a speed of several thousand feet per minute inflames right up against the delivery nozzle, the flame playing about its mouth. This is best accomplished by avoiding high pressure for projecting the fuel. The final mixture of air and fuel is at the instant of projection into the furnace.

It must not be forgotten that an expansion of volume takes place in the air carrying the fuel as soon as it is heated. This expansion is of course based upon the related absolute temperatures and explains the large volume assumed by the flame on leaving the point of entrance. Excess air so carried partakes also of the expansion.

The temperatures usable cover a range of nearly 2,000°, or from about 2,000° to 4,000°. By ordinary manipulation as described, the temperature and quantity of fire can be changed as easily as the turning on or off of a gas jet. The response is instantaneous. This particular feature renders the use of pulverized fuel particularly suitable in metallurgical furnaces. It has been found so in steel and iron working, being to-day in the following uses: ore roasting, flue-dust nodulizing, open-hearth furnaces, puddling furnaces, busheling furnaces, heating furnaces, forge furnaces.

The main difficulties in the earlier and experimental stages were caused by: 1, not drying the coal; 2, poor pulverization; 3, the carrying of too high temperatures; 4, the use of ports too small, giving the gases too high velocity.

With a knowledge of how much air is required with a given amount of fuel to produce a given temperature, and a knowledge of the volume of gases so produced, it is easy to proportion the ports both of inlet and outlet so that a scouring blow-pipe effect may be avoided. The excellent practice already attained is undoubtedly due to the application of this knowledge.

Aside from the advantages attainable in the higher efficiency of the fuel, there are a number of things which in actual service contribute to the profitableness of its use.

The furnace begins its work almost instantly and with whatever degree of intensity may be desired. There are no periods of lowered temperature due to firing cold fuel. There is no cleansing of fires for puddling or heating, so that operation is practically continuous. There

is some cinder formed in puddling and heating furnaces, which is disposed of in the usual way. The most of the ash passes out of the chimney and floats away lightly. The neutral ash content within reasonable limits does not affect the fire appreciably.

It has been somewhat difficult to obtain from large users close data concerning the performances of the various furnaces. Perhaps the best evidence of success is the continuance of use and the enlargement of plants now going on in extension of business upon the same lines. The author is able, however, to submit the following authentic data:

In roasting carbonate ores of high sulphur content, the carbon has been driven off and the sulphur reduced within permissible limits by the use of less than 7.5 per cent. of fuel upon the weight of the charge. This problem involves the maintenance of a low temperature, about 2,100° F., to prevent the agglomeration of the ore fines into masses. The same practice obtains in the roasting and nodulizing of ores and flue dust, where the heat is maintained sufficient to permit the ore to form nodules or balls, but not to stick to the walls of the roasting kiln.

In open-hearth practice with pulverized coal, steel is being made with this fuel at the rate of from 450 to 500 lb. per net ton of product. This is an average of 45 heats, the fuel and product being carefully weighed.

These figures were obtained during a continuous run of six weeks. The furnace was operating beautifully when visited by the author, and no mechanical difficulty had been experienced. The melts were obtained in slightly less time than with oil, which had been used previously. An analysis of the slags produced with each fuel is as follows:

	Oil.	Coal.
SiO_2	16	16.5
FeO	22	18.2
MnO	7.4	6.7
P_2O_5	1.7	1.9

Final analysis of the steel:

Sulphur	0.025 to 0.035	0.035 to 0.04

Sulphur in coal, 1 to 1.15 per cent.

There appears here to be no more difference than would occur daily in the variations of the charge and of the fuel.

In puddling furnaces the fuel supply varies with the season, the cool weather of spring and fall permitting a larger output than when intensely hot weather affects the men tending the furnace. It is safe to say that iron can be puddled at an average expense of 1,200 lb. of

pulverized fuel per gross ton of muck bar produced; in fact, less than 1,000 lb. of coal per gross ton of bars has been shown in practice during periods when favorable temperatures and continuity of work conduced to high economy.

In heating furnaces and busheling furnaces there is some latitude of performance, due to varied charges placed in the furnaces and to the size of mill served by them. The average consumption of pulverized fuel in heating furnaces seems to be from 500 to 550 lb. of fuel per gross ton. The busheling furnaces require from 550 to 600 lb.

To obtain such results, however, the furnaces must be properly proportioned and equipped and in good condition. It must not be expected that the results obtained by simply squirting coal of greater or less degree of pulverization into a furnace, with an unmeasured jet of air, will equal the practice here shown. The figures given are authentic and may be relied upon. Success insists upon dry coal, fine pulverization, proper air supply.

Another factor is that the attendants should be interested in the production of high results. Men of a good order of intelligence operating mechanism which displaces the shoveler and the wheelbarrow man, and who are constantly on the "firing line," both practically and metaphorically, are extremely valuable in this art. The operations, however, are simple and the manipulations few and rational in their nature. With such men further advances may surely be looked for.

The procedure followed in the proper preparation of pulverized fuel, as well as delivering it to the furnace, is as follows:

The coal is received in the pit of an elevator, into which it is dumped from the cars. The elevator carries it to the hopper of a pair of crushing rolls. The coal after passing through these rolls may be weighed by automatic recording scales.

The coal should also pass over a magnetic separator at this stage to remove iron or steel scrap in the shape of nuts, bolts, pick points, wedges, and such foreign matter, which would interfere with the pulverization.

The coal is next introduced into a drier to expel the moisture. A good drier of approved design will remove 6 lb. of moisture per pound of fuel used in firing the drier and the product will ordinarily carry less than 1 per cent. of moisture. From the pit into which the dried coal falls from the drier, it is elevated to bins above, from which it is evenly fed by spouts and feeders to the pulverizing mills. These mills, if of proper construction, grind the coal rapidly to the degree of fineness required.

The pulverized coal is led to the pit of an elevator, which carries it aloft to a conveyor which distributes it to the coal bins, from which it is delivered by gravity to the pipes leading to the burners.

It is well to note at this point that pulverized coal should always be handled as a solid and not as a dust-cloud. To convey it by a plenum of air insures the existence of a mixture of air with the coal, the only state in which it is dangerous, while if leaks in the apparatus exist the surroundings become insupportably dirty. The delivery of the coal in this form also makes necessary some form of settling or concentrating chamber wherein the coal may reappear as a solid. The requirement of velocity in the air current sufficient to float the coal along causes an expenditure of power greatly in excess of that required by properly constructed elevators or conveyors.

The bins for holding the coal are proportioned to carry sufficient fuel to serve the furnace during intervals in which the mills may not run; as, for instance, coal may be ground and stored for 24 hr. continuous service by running the mills for 10 hr.

The coal is fed from the bottom of the bin by a worm feed screw provided with a variable-speed drive, so that the furnace may receive the coal as desired. The coal falls freely from the feed screw delivery through a closed pipe, mixing with air in its descent in preparation for entering the burner pipe.

The burner pipe is so formed that the air passing through it from a fan not only projects the fuel into the furnace, but also, while doing so, acts as an injector in drawing with it the descending column of air containing the entrained coal from bins above. The fuel therefore is completely mixed with the ultimate column of air entering the furnace. The speed and volume necessary for the proper furnace performance are predetermined from known data.

The air is controlled by the fan speed or by gates, or by both, and the coal by the number of revolutions of the feed screw per minute. The operator adjusts these factors to the quantity and intensity of fire desired, and by inspection at times sees that the conditions remain constant. The construction of the furnace is not materially changed. The operating cost in the furnace room is very low indeed, as one man can oversee a number of furnaces. The furnaces are so varied in construction and operation that it is not fairly in the scope of this paper to discuss them. It must suffice to state that any solid fuel which can be dried and pulverized can reach its highest efficiency in that form, and for this reason fuels hitherto deemed unavailable, such as coke breeze, lignite culm, and anthracite culm, may be now

looked upon favorably as responsive to the call for cheapness in the source of heat.

Regarding the actual practice in the use of pulverized fuel, the ease with which it is burned has been to a certain extent a drawback rather than an advantage. This is a statement paradoxical in its nature, needing explanation. The novelty of the method is so attractive that those experimenting with it are at first satisfied with producing a good fire with simple apparatus in which may exist no such means of control as are necessary to the accomplishment of the best economy.

It is no success to use twice as much fuel as the work at the time may require, nor is it a success to drive a small fire to a destructive intensity to make good defective proportioning.

The proportioning involves knowledge of the heat requirements of the job. The amount of fuel necessary may be ascertained and the volume and velocity of the air supply computed. With this comes necessarily a prescription of the volume of the furnace, that combustion may have time for its completion. The proper size of ports taking off the gases, the size of the chimney, and the velocities of the gases, should all be as carefully determined for pulverized fuel as for gas or oil. A simple experimental trying out, with a defective link in the chain of conditions, should not be regarded as conclusive.

With proper predetermined proportioning of the apparatus, the operation will be elastic or adjustable to a wide range of performance under a very nearly constant percentage of efficiency. This is unattainable in an installation which may or may not be proportioned to the production of high efficiency at any particular duty. For this reason the ease with which pulverized fuel may simply be burned does not assure that the best results are being obtained.

Discussion.

H. R. Barnhurst:—I would say that in that experimental furnace we were trying to adopt the proper form for burning low-volatile coal. I was enabled on one occasion to burn coke having 15 per cent. ash. Tailings, or washings from the preparation of coal for by-product coke ovens, which came to me 52 per cent. ash, were burned without any trouble. So that ash really might be regarded as not burning, but simply diluting the fuel that was there, without doing any particular harm except that we had to put in more of the material to get the same quantity of heat. With anthracite, I burned coal right along with from 24 to 27 per cent. ash, but it took a furnace of peculiar form in which the products of combustion had to be

diverted to the point of entrance of the fuel. With an ordinary furnace the fuel is deflagrated by its surroundings as soon as it enters the furnace. In a cement furnace this action is secured by the hot clinker passing down, and the walls of the kiln are heated upon contact with it, revolving as they do, so that deflagration occurs very quickly in a cement kiln. Deflagration would occur very quickly in an open-hearth furnace also. There is no trouble in burning anthracite coal under those conditions for three or four days successively. It should be very well dried and thoroughly pulverized. Aside from that, there is no trouble.

WILLIAM A. EVANS,* Boston, Mass.:—In a 52 per cent. ash, or any high-ash coal, what disposition is made of the ash?

MR. BARNHURST:—Most of it went out through the flues. Some of it remained in the boiler and a quantity remained in the furnace. We raked it out and carried it away.

MR. EVANS:—Do you experience the forming of slags in flues with such coal as that?

MR. BARNHURST:—With perfect combustion I do not think there is any difficulty about slag forming. If combustion is interrupted, then it will slag on most any surface, but if the ash is removed promptly, say within half or three-quarters of an hour or an hour, the anthracite coal will pass off as a hot dust; but if it stays there longer, it gradually cakes and at the end of three or four hours will become troublesome.

MR. EVANS:—At what temperature will it slag?

MR. BARNHURST:—I cannot say exactly.

MR. EVANS:—I thought the forming of slag over beyond the hearth of the reverberatory furnace would not apply because of the high temperature carrying it beyond the hearth and the forming of a slag with a very high ash coal.

MR. BARNHURST:—I think the slags would vary according to their composition in the furnace. In the furnace I used, the slags did not form at all. As the heat was prolonged the tendency, however, was more manifest.

* Non-member.

H. A. PROSSER, New York, N. Y.:—This trouble of slag from the ash choking the flue occurred at Cananea some time ago when they were experimenting with powdered coal. It also occurred at the old Highland Boy smelter, where similar experiments were made. The furnace at Cananea started out very well indeed, and then the tonnage diminished every day, until at the end of three or four days the flue was so filled up that they had to stop the operating.

MR. BARNHURST.—I think a little prevision might have avoided that. The last time I was in the western part of the State I visited an open-hearth furnace that had been running six or eight weeks continuously and had not stopped at all. There was no accumulation that was detrimental at that time.

MR. PROSSER:—The gentlemen in charge of both of these plants expressed the opinion that these troubles could be overcome, and since then there have been built two reverberatory furnaces at Copper Cliff which are burning powdered coal; these furnaces have very large flues, and I am told they are running very well indeed. I would like to ask if waste-heat boilers can be used with powdered coal, or whether the dust causes any trouble in the waste-heat boilers of reverberatory furnaces.

MR. BARNHURST:—I see no reason why the dust would not blow out just as freely as in the ordinary type of furnace.

MR. PROSSER:—In the ordinary waste-heat boilers on oil-fired copper-smelting reverberatory furnaces the question of blowing the dust off the tubes is quite important; if this is not attended to, the boilers will not perform proper work, and with this powdered coal fuel I suppose you get a great deal more dust in the boilers, so there will be more blowing off to be done. It is rather hard to get the men to do it properly.

MR. BARNHURST:—In the Babock & Wilcox boilers there is generally provision for blowing the boilers off. My experiments were conducted mainly on a Rust vertical-tube boiler and there was no deposit there, nor was there any deposit at the base of the stack. With the fine pulverized coal nearly all of the ash went out. The grosser particles of the coal, those that would not probably pass 100 mesh, would possibly stay in the furnace, and to that is charged the ash that remains there. In burning a ton an hour under a boiler, we did not get more than 30 or 40 lb. of ash, and we could rest for three or

four hours and not do anything, but then we found it began to cake and we had to take it out. There was about a wheelbarrow full. We find the best way to get a good constant product is to take it out every hour.

T. T. READ, New York, N. Y.:—In the field of copper smelting the metallurgist is confronted by undesirable alternatives. If the coal ash is infusible it will give less trouble through sticking to boiler tubes and flue walls, but it then makes an undesirable addition to the smelting charge. Coal ash from bituminous coal averages about 20 per cent. Fe_2O_3 and 35 per cent. SiO_2, so that its infusibility is largely due to its high content of Al_2O_3 (15 per cent.). The reason why no trouble is experienced on this score at Copper Cliff is because conditions are unusual and the charge is very basic, the slag ranging from 28 to 30 per cent. SiO_2. In ordinary copper smelting the metallurgist is between the two horns of the dilemma, and is apt to be impaled on one or the other.

H. A. PROSSER:—When slime is used for fettling in copper smelting it very often forms what is called a siliceous blanket over the furnace, and this reduces the tonnage smelted. Usually if you have proper combustion and high enough temperature in the furnace, you can melt this scum. I imagine this trouble they have mentioned, due to the ash falling on the charge, is something of that nature. This trouble was experienced by Sörensen at the Highland Boy, and by Shelby at Cananea, when firing with powdered coal, but they both expressed the opinion that the ultimate result would be satisfactory if they were permitted to continue to experiment, which they were not.

P. H. GRIFFIN, New York, N. Y.:—I would like to ask Mr. Barnhurst what experience he had in using checker work.

MR. BARNHURST:—My only experience in that has been the observation of two or three open-hearth furnaces where the checker work is used in the ordinary way. No change was made in that. Most of the trouble in using pulverized coal has been from the excessive heat obtained in the beginning, and it is only by providing some way to adjust the air supply carefully that the heat can be kept down. If you merely put in air enough to burn fuel, you will have an excessive heat, giving you trouble.

MR. GRIFFIN:—I asked the question because, of course, it is well known that in the old days when they used cold blast in the blast

furnace they got the best quality of cast iron. To-day they are making it in the same way. Now, what would be the effect on the product of the furnace operated in that way if you used the hot blast as compared with the cold blast?

MR. BARNHURST:—I must refer you to Professor Richards's paper.

ALFRED STANSFIELD, Montreal, Canada:—I have recently seen the furnaces at Sudbury, and they were working very well with powdered coal. The ores are not high in silica, and therefore there is not the trouble that has been experienced elsewhere from ashes covering up the slag and making an infusible scum. At the time of my visit, in the spring of this year, boilers were used to recover the waste heat from the reverberatory furnaces fired with powdered coal, and I understand that they were giving satisfaction.

T. T. READ:—I asked the people at Sudbury, and I understood them to say that they had tried waste-heat boilers there and found they cut down the draft too much. It was merely a question of draft, as I understood the man in charge of the plant.

CHAIRMAN RICHARDS:—I have heard from some direction that the use at Sudbury is so successful that the Anaconda Company are going to adopt the same system as that in use at Sudbury in their large reverberatories at Anaconda, where they have boilers for utilizing the waste heat. Therefore their engineers must be convinced that the system will work with their furnaces.

KARL NIBECKER,* Youngstown, Ohio:—Mr. Barnhurst said that he had burnt coal with a high ash in boilers. I would like to ask whether he has found excessive erosion and wear of boiler and setting with this high ash. Can he tell us the air pressure at which he burned this powdered coal, and what the percentage of ash was?

MR. BARNHURST:—I found that when the blast was brought down to very low pressure and the air blast was but enough to produce the heat desired there was very little trouble.

A MEMBER:—What was the practice observed in open-hearth furnaces?

MR. BARNHURST:—The coal was projected by an 80-lb. air blast through a ⅝-in. pipe. This pipe was surrounded by a pipe 3 in. in

* Non-member.

diameter through which coal was introduced with air at 1 lb. pressure. That was only putting in about 15 or 20 per cent. of the total air required for the furnace. Using those high pressures you get the long flame that you were speaking of a few moments ago. By projecting the fuel very rapidly and giving it insufficient air at first, you will get a longer flame, because additional air must be taken, as that coal in the core comes to the surface, and is exposed to the oxidizing effect of the air that is coming in. In that way a long flame can be made; but coal mixed with the proper amount of air will burn right back to the nozzle and you will have a short flame and more perfect combustion.

H. A. PROSSER:—In relation to the comparison of fuel oil and coal, the figures were given that 11 gal. of oil are equivalent to 110 lb. of powdered coal. This figures out that from 2,000 lb. of coal you get the same heat effect that you would get from 5 barrels of oil, approximately, and we know, as Mr. Stoughton has said, that in ordinary non-powdered coal practice that figure is from 3 to 3½ barrels of oil equivalent to 1 ton of coal; this seems to bring out the fact that we get much less efficiency from powdered coal.

R. C. CARPENTER, Ithaca, N. Y. (communication to the Secretary*):—I think that Mr. Barnhurst would confer a favor on the members of the Institute by inserting drawings showing a standard layout of grinding machinery, coal bins, and burners for supplying pulverized coal to a cement kiln. These drawings would give a much clearer idea of the construction than the description.

There is no direct statement in Mr. Barnhurst's paper respecting the explosive nature of coal dust, but in one of his paragraphs a statement is made which has a bearing on this point. Mr. Barnhurst states that pulverized coal should always be handled as a solid and not as a dust cloud. This caution is a very important one to be observed in construction, since coal dust when suspended in air is an explosive mixture which is readily fired, and under such conditions is nearly as dangerous as gunpowder. In the early days of coal-dust burning in the cement industry there were many disastrous explosions, which caused on the whole an extensive loss of life. If the coal dust can be conveyed in such a way that it will not be mixed with air, or at least so that sufficient air for combustion is not mixed with it, there is little or no danger of an explosion. Such conditions will give a slow burning rather than explosive burning and little or no damage is likely to be done.

* Received Nov. 19, 1913.

It strikes me that Mr. Barnhurst's paper does not set forth very clearly the early difficulties that were experienced in coal burning in the cement industry. I spent a great deal of time in developing successful processes of burning pulverized coal in its first application for use in cement kilns. The difficulties were due to many other things than improper grinding, and in addition to developing the proper apparatus quite a long period of education was required to produce skilled men capable of getting results.

The burning of pulverized coal proved to be very different practically from the burning of oil, which had previously been used extensively in the cement industry.

It is interesting to note that up to the present time pulverized coal has not been successfully burned under boiler furnaces, although many thousands of dollars have been spent in the attempt. This calls attention to the fact that the successful burning of powdered fuel needs favorable conditions. The actual combustion must take place with the coal particle in suspension and there must be no impingement of the particle against solid materials, like parts of the setting, until after the combustion is completed. This latter condition is nearly impossible to meet in boiler furnaces. In metallurgical furnaces I believe the success will depend upon conditions. Where the furnace is of such a form that the coal particle can be completely burned while in suspension and before impinging on any portion of the furnace, success may be expected. On the other hand, where the metallurgical furnace is contracted so that solid parts will be impinged on by the flame, pulverized coal will probably not be efficient or successful.

In the cement industry pulverized coal is generally burned with a minimum supply of air; as a rule, the air supply for cement kilns is not regulated but the amount of fuel is varied so as to maintain the highest temperature. Maximum efficiency in cement kilns is usually obtained when a small amount of carbon monoxide appears in the discharge gases, and in many of the cement plants the combustion is regulated to maintain such conditions. This action has the same effect as the regulation of the air, although it can be useful only where there is a demand for all the heat that can be produced.

The Scoria Process for the Manufacture of Fine-Ore Briquettes, Flue-Dust Briquettes, and Slag Brick for Building Purposes.

BY ERNEST STÜTZ, NEW YORK, N. Y.

(New York Meeting, October, 1913.)

THE problem of increasing blast-furnace efficiency through diminution of flue-dust production while operating with burdens consisting largely of fine ores has of recent years attracted the attention of metallurgists, whether directly or indirectly interested in the economies of iron smelting, and the various methods evolved from theory or practice have been submitted to wide discussion. After the very thorough and, at that time, exhaustive view of the whole subject, which was afforded to the industry by the meeting of this Institute in February, 1912, the subject was again taken up at the meeting of the Verein deutscher Eisenhüttenleute at Düsseldorf in December, 1912. While American and German conditions are not entirely identical, it may be of interest to give here the words of the gentleman summing up the discussion at the Düsseldorf meeting:

"The chief advantage offered by the use of briquettes lies in the fact that disturbances in the furnace, caused mostly by the presence of fine ores in the burden, are avoided, and that blast pressure can be considerably reduced. The resulting economies cannot be overrated, as, with lower blast consumption per ton of product, the installation will be able to take care of increased production. In consequence of lower pressure and speed of blast, heat is more efficiently utilized, flue gas temperature is lowered, and coke particles are less liable to be carried along with the flue dust, of which considerably less is produced. Finally, by increased regularity of operation and higher temperature in the lower parts of the furnace, the quality of the product is improved. The question of briquetting iron ores has grown to be one of the most important in the iron industry, and it is not any more a question of what is the cost of briquetting a ton of ore, but what, in using ore briquettes, is the cost of production of a ton of iron."

Dr. A. Weiskopf, of Hannover, admirably condenses in these words

the reasons for the interest shown by the industry in the discussion of the most efficient means for turning the wasteful expense of handling and recharging flue dust into a productive factor in the general improvement of blast-furnace practice.

The methods which have been developed for that purpose are fairly numerous; in Table I. their characteristics and comparative costs are reproduced in American values from a table published in *Stahl und Eisen* of Feb. 13, 1913. Most of them have been mentioned or described by Felix A. Vogel in a paper read before this Institute. He was mistaken, however, in stating in the subsequent discussion that the Scoria process "had not been in use for some time past." For more than two years the Scoria process had then been in active operation on a scale of a daily capacity of 200 tons at the Rheinhausen works of Friedrich Krupp A. G., which, on the strength of the results then obtained, has now doubled its installation.

This makes it appropriate to give here a short account of this simple method, which, as its name implies, uses slag—scoria—in preparing the valuable materials for smelting in the blast furnace. In other words, it uses a binder, but with the distinction that, between manufacture and use, the material so employed undergoes an essential transformation, being hydraulic until it passes into the furnace and being there turned automatically into a fusion binder. The metallic oxides will therefore be free to react during their passage through the region of the top gases, and the binder gives the briquettes the necessary consistency to support the weight of the stock pile.

Figs. 1 and 2 show a plan and longitudinal and cross sections of an installation of four briquetting presses, each capable of turning out from 1,800 to 2,000 briquettes per hour, or about 10,800,000 to 12,000,000 in 300 working days of 20 hr. Taking the weight per briquette at 8.8 lb., this gives about 144 to 160 tons per 20-hr. working day, or about 43,200 to 48,000 tons of briquettes per press per year.

To summarize the process of manufacture, the granulated slag and the lime are mixed with the valuable materials and made hydraulic in rotating steaming drums, where they are exposed to moderate-tension steam. When thoroughly mixed, sifted, and ground, the powdery mass is conveyed to the presses to be formed into bricks, which are carried in trainloads of trolleys to cylindrical kilns, in which they are hardened by being exposed to the action of high-tension steam for from 8 to 10 hours. The trainload of hardened bricks goes straight from the kiln to the blast furnace and is there

TABLE I.—*Manufacturing Cost for Various Briquetting and Submitted by the*

(German values translated

Name of Process.	1. Material.	2. Daily or Yearly Production	3. Cost of Installation.	4. Additions. Quantity and Kind.	4. Additions. Value Per Ton of Product.	5. Depreciation. A.	5. Depreciation. B. Per Ton of Product.	5. Depreciation. C.	5. Depreciation. D. Per Ton of Product.
		Tons.			Cents.	Per Cent.	Cents.	Per Cent.	
Ronay.	Flue dust.	100 30,000	$28,500	.....	.....	15 mach. 10 bldg.	13.5	10	9.5
Schumacher, ground quartz lime.	Flue dust and fine ores.	200 60,000	50,000	10 per cent. lime, 5 per cent. quartz flour.	27	10 mach. 2 bldg.	7.5	10	9
Schumacher, magnesium chloride.	Flue dust.	100–120 30,000–36,000	16,000	Magnesium chloride.	12.5	10	4.5	10	4.5
Scoria.	Fine ore and flue dust.	200 60,000	25,000	4 per cent. blast furnace slag. 4 per cent. lime.	10	10	4	10	4
German Briquetting Gesellsch.	Fine ore and flue dust.	160 48,000	11,000	10 per cent. lime and cement.	25	10	2.5	10	2.5
Dahl.	Fine ore and flue dust.	500 150,000	.....	8 per cent. lime, 1 per cent. blast furnace slag.	.....	10	12.5	10	12.5
Grōndal.	Magnetite.	1 furnace 43 12,900	17,500	.....	.....	10	13.5	10	13.5
Agglomeration, Fellner & Ziegler.	Fine ore.	150 45,000	38,000	.....	.....	10	8	10	8
Dellwick-Fleischer water-gas.	Fine ore.	100 30,000	36,000	.....	.....	10	7.5	10	7.5
Converter sintering.	Purple ore.	1 Converter 10–30 3,000–9,000	4,800	.....	.....	10	6.5	.....	.....
Zellpech.	Flue dust and ore.	400 120,000	36,000	4 per cent. organic binder.	43	10	2.5	10	3.0
Crusius.	Fine ore and flue dust.	600 [a]18,000	.....	4 per cent. prepared bitumen.	.....	10	5	10	5

[a] So given in *Stahl und Eisen.*

The different columns give the following information :

Column 2 gives daily and yearly capacity (a year reckoned at 300 working days). The figures in the following columns are based on this.

Column 5 : A and B give depreciation according to figures furnished by constructing companies ; C and D, their reduction to a uniform standard of 10 per cent. to facilitate comparison.

Agglomeration Processes Tabulated According to Cost Estimates Constructing Companies.

into dollars and cents.)

6. Coal Consumption per Ton. of Product.			7. Wages Per Ton. of Product.		8. Power, Steam, Repairs, etc.	9. Remarks.	10. Total Manufacturing Cost.	
A.	B.	C.	A.	B.			A.	B.
Per Cent.	Cents.	Cents.	Cents.	Cents.	Cents.		Cents.	Cents.
.....	.....	.....	10	11.5	18	.....	41	38
.....	.....	.....	15	18	18	See *Stahl und Eisen*, Mar. 4, 1908, p. 322.	67	72
.....	.....	.....	16	16	8.5	See *Stahl und Eisen*, June 22, 1910, p. 1061.	41	.....
.....	.....	.....	10	11	9	See *Stahl und Eisen*, Mar. 4, 1908, pp. 324-5.	32	34
.....	.....	.....	4	13	6.5	Wages for 11 workmen per shift are given; installation cost is given too low, the company itself giving cost of product at 55 to 80 cents.	37	55
.....	.....	.....	16.5		11.5	....	63	.....
8	43	25	30	40	25	In Sweden cost of coal — $5.30 per ton; wages, 80 cents per shift.	$1.10	$1.00
9	27	27	6.5	7	18	See *Stahl und Eisen*, May 4, 1910, p. 759.	60	60.5
10	29	29	16.5			.....	52.2	52.5
b	7.5	.....	15 to 23	16.5	+ Depret'n. — Power. 5, 6, 7	See *Stahl und Eisen*, Feb. 9, 1911, p. 245.	30 to 50	.....
c	7.5		15.5					
.....	.....	.....	2.5	2.5	6.5	See *Stahl und Eisen*, Aug. 19, 1908, pp. 1196-9.	53	.....
.....	.....	.....	16.5		23	.....	88	.....

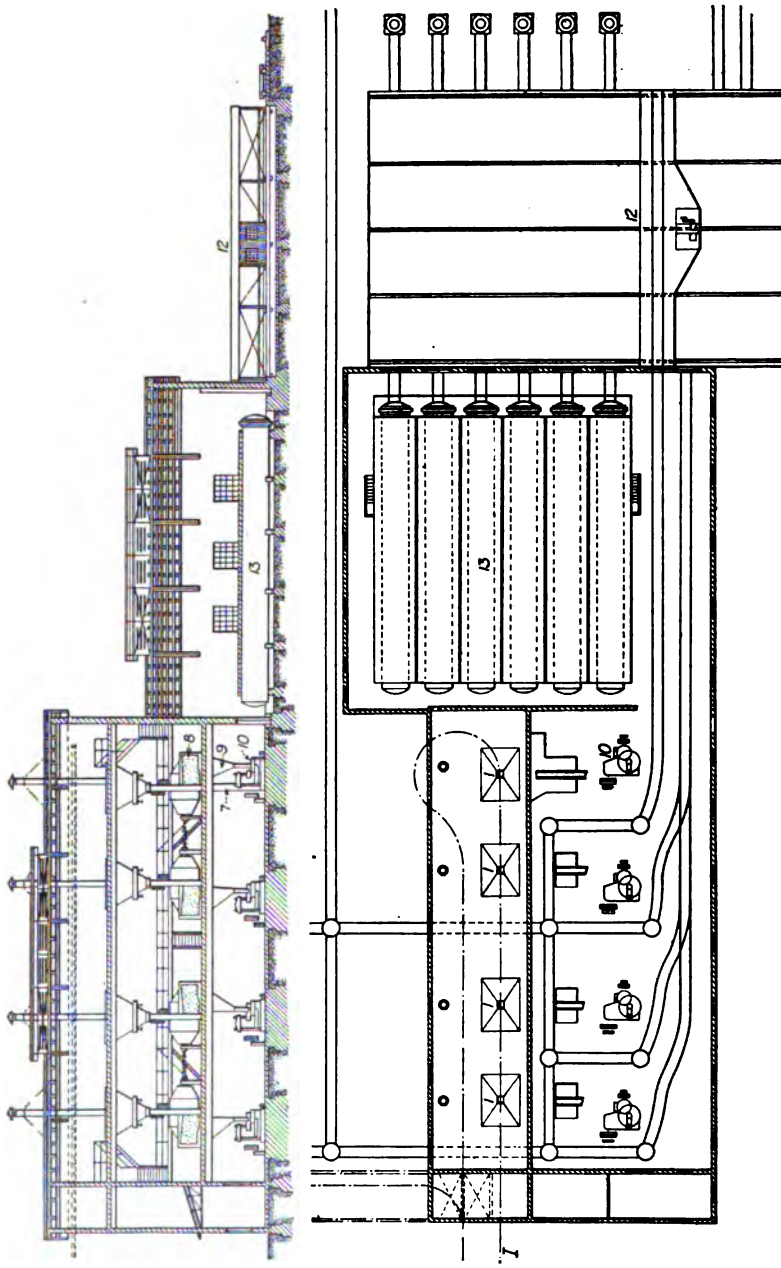
b 6 per cent. of coal for ignition.

c 6 per cent. of coke breeze or 10 per cent. of low-grade fuel.

Column 6: Coal consumption in this column refers only to that of sintering processes. Coal consumption for steam and power for other processes are included in Column 8. The figures under C are reduced to a common price of $2.85 per ton.

Column 7 gives wages: under A, according to constructing works figures; under B, under the assumption of an average daily wage per workman of $1.38.

Column 10 gives cost of production: under A, according to constructing works figures; under B, comparative figures of values reduced to a common basis.



I, Conveyor from outside storage; 1, Storage bin; 2, Conveyor-hopper; 3, Steaming drum for moderate-tension steam to develop hydraulic qualities; 4, Storage bin; 5, Scraper plate to remove larger particles; 6, Grinding and mixing trough (*Kollergang*); 7, Bucket conveyor to 8, Sieve drum where foreign bodies, iron, etc., are thrown aside; 9, Storage bin for 10, Briquetting presses; 11, Trolleys, which are formed into trains and by means of 12, Traveling platform, are run into 13, Hardening kilns, where they remain for from 8 to 10 hr. exposed to high-tension steam of about 120 to 150 lb. pressure.

FIG. 1.—PLAN AND LONGITUDINAL SECTION OF BRIQUETTING PLANT USING THE SCORIA PROCESS.

dumped, unless required for immediate use. In the installation illustrated, particular attention has been paid to an arrangement by which the switching operations, by means of a traveling platform, can be carried out in such a manner that the trolley cars can be moved only in entire trains.

It will be noticed that the manufacture is almost automatic. If the necessary amount of steam is provided for, there is nothing in the process itself that requires supervision. The transportation is practically all mechanical, the actual handling of the briquettes being necessary only once: namely, in transferring them from the press to the trolleys before they are hardened. The labor required for an installation of two presses consists of a foreman and 14 men, including labor for steaming drums, grinding and conveying machinery, presses, hardening kilns, and trolley transportation.

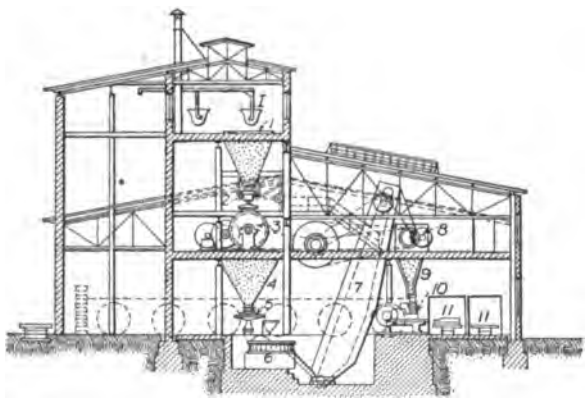


FIG. 2.—CROSS-SECTION OF BRIQUETTING PLANT.

The fact that in the Scoria process no air drying is necessary deserves particular attention, as it is thereby distinguished from all other briquetting processes; and the saving in space and handling is a considerable factor in its economical advantages.

The running of the plant requires nothing in the way of special experience or skill, and the work is such that the labor regularly available at the plant can readily undertake it without special supervision. Raw materials are always at hand at the plant and have not to be supplied from outside, and the success of briquetting does not depend on special conditions of the flue dust.

When the American Institute of Mining Engineers at its meeting in February, 1912, devoted considerable time to the discussion of the merits of briquetting and sintering processes, the Scoria process was

unfortunately not represented. I may be permitted, therefore, in this place to add in its behalf something to that discussion and to inquire into some of the reasons which may have induced a works so highly organized as Krupp's to discard another already installed process in its favor. Of course, low cost counts for something, and the accompanying table shows that in this respect the Scoria process leaves nothing to be desired in comparison to others. The administrative advantages have already been pointed out. But both these combined would hardly have given the Scoria a marked advantage over others, if the result of using Scoria briquettes on blast-furnace operation as a whole had not been signally salutary. For instance, in a trial run of several days' duration, a proportion of 43 per cent. of the charge (burden plus limestone) was added in the shape of Scoria briquettes without in any way interfering with the regular running of the furnace.

The fact is, the hydraulic binder gives the body the necessary consistency at a minimum consumption of binding material, and, being in the nature of a hydrosilicate, leaves the oxides freely accessible to the top gases. It is idle to pretend that in the case of the Scoria binder it would be necessary "to have enormous pressure on one side of the briquette to produce an actual flow of gas through the structure." The force of the human lung is sufficient for that purpose. The objection may be all right with binders of a clayey nature, but with the Scoria binder it is theoretically unsound and practically disproved. On the other hand, the temperature necessary for the destruction of the hydrosilicates is above that where sintering commences, so that there is no possibility of the briquettes crumbling away prematurely.

Another objection that has been brought against briquetting processes is that the coke that has been wasted through the top of the furnace becomes of no value in the briquetting process while it is utilized in one or two of the sintering processes. True, the sintering process does use the coke contained in the flue dust, in order, as it is said, to predigest the food for the furnace. But is the furnace naturally a dyspeptic, that it should pay for the service of "predigestion" when it can get its food in its natural form? The Scoria process returns all the coke blown out with the flue dust in undiminished quantity and in the most suitable condition for assimilation in the monster's giant stomach. In that it is more economical than any of its competitors.

If, therefore, the use of this binder is not a disadvantage as compared to sintering processes, the only remaining criticism would bear upon the question whether the remelting of the slight quantity of

slag in the furnace is a wasteful proceeding. Even were that so, the cost of melting, say, 4 or 5 per cent. of the weight of the briquettes cannot be compared with the cost of coke wasted through the tendency of fine material to scaffold, quite apart from the fact that there are many occasions when the slag is positively beneficial, as in the utilization of ferro-manganese slag in order to increase the manganese content of basic pig iron.

Actually a very important coke economy will result from the use of the Scoria process. Working with a homogeneous and naturally piled stock column, the gases will circulate and pass up under equal pressure, the combustion will be far more complete than is otherwise possible, the production of carbon monoxide will be lessened; and under those circumstances it has been proved by actual operation in Germany that a saving of from 15 to 20 per cent. of coke has been effected. This could be still further increased if the burden were to consist of only lump ore more than 1.25 in. in size and briquettes. Under such conditions blast-furnace operation would be so accelerated that the output of the furnace could be enormously increased. The inventor of the process feels confident that it is well within the means of possibility to so perfect operating conditions as to produce one ton of iron in a day for every cubic meter (35 cu. ft.) of blast-furnace volume.

The Scoria process, based on sound theoretical considerations, has been carefully worked out scientifically, and it has stood the test of actual operations on a large scale. It has come to fruition more slowly than some others, but for that reason it comes forward now with more matured experience. It may appear more complicated than the use of sintered material, but when it is remembered that its success does not depend on the "maintenance of absolutely uniform conditions of adjustment as to mixture, . . . size of material, compression, moisture, air blast, and heat treatment,"[1] but is much more of a humdrum workaday nature, it is evident that appearances in that respect are misleading.

There yet remains to be mentioned, however, an advantage of the Scoria process which makes it particularly valuable to smaller blast-furnace plants or those where the disposal of the slag is a serious problem.

The Scoria plant may be used to produce excellent slag building brick, and can alternate their manufacture with the production of flue-dust or fine-ore briquettes, according to requirements, at a mo-

[1] *Metallurgical and Chemical Engineering*, vol. x., No. 9A, p. 596 (Sept. 12, 1912).

ment's notice. The operations are almost identically the same, except that the flue dust or fine ore is left out of the mixture. Such bricks have shown ample strength for all purposes, not only under severe laboratory tests, but also in actual use for building purposes during a number of years. The cost of production, of course not counting the slag as having a value, in Germany, on a two-press installation capable of furnishing 24,000,000 brick per year is 5.30 marks, or about $1.25, per thousand.

Such bricks were tested at the Royal Testing Laboratory, Grosslichterfelde, by steeping in water and exposing to frost at 0° F. (—17° C.) in 25 successive operations, without showing an appreciable loss of strength.

As a further proof of the growing favor this process meets with abroad, I may mention the completion in April of a plant of a capacity of 400 tons per 20-hr. day for the Aktien Gesellschalt für Hüttenbetrieb, Duisburg-Meiderich, the trial operation of which has turned out so satisfactory that a doubling of the installation is contemplated for the near future.

Discussion.

H. O. Hofman, Boston, Mass.:—I have hunted in vain for a figure which gives a dehydration temperature of cement. I was wondering if Mr. Stütz had a figure of that kind.

E. Stütz:—No, I am afraid I have not.

H. O. Hofman:—I believe some figures have been published that cement disintegrates at 400° or 500° C. It seems to me a very low temperature. Mr. Stütz just made the statement that the disintegration of this briquette begins at the temperature at which the briquette begins to sinter.

E. Stütz:—Above the sintering point.

H. O. Hofman:—The sintering point would be about 1,200° C.

E. Stütz:—Yes, roughly speaking. I am afraid I have no information about the exact degree of dehydration.

H. O. Hofman:—I have looked a great deal for such a figure and I have not been able to obtain it. Professor Howe says I might have tried myself. But my interests were directed in another line; although that is, perhaps, a good suggestion.

CHAIRMAN RICHARDS:—Is not Portland cement sometimes used for the test in a cupellation furnace and a drying-out furnace?

H. O. HOFMAN:—Yes, it is used, but that is a higher temperature, of course. But I have been trying to get a figure as to where it loses all its water. I understand it begins to lose water at 500° C. But there are different silicates or hydrosilicates in the cement, so that one would have four or five or six figures to show the different stages at which the cement loses its water.

CHAIRMAN RICHARDS:—I would ask whether the use of this material in blast furnaces has shown increased capacity of furnace or decreased consumption of fuel.

E. STÜTZ:—It has shown decreased consumption of fuel. With 43 per cent. (calculated on the burden plus limestone) addition in the shape of Scoria briquettes furnace operation was absolutely regular. I have no definite figures on coke consumption.

FELIX A. VOGEL, New York, N. Y.:—I want to call attention to the statement of costs published by Mr. Stütz in his paper, which was taken from an article published by Dr. Weiskopf some time ago. In this article the cost of production by the Scoria process is given as 32 c., while by Dr. Schumacher's process it is given as 41 c. I would like very much to know whether the 32 c. represents the actual cost as obtained in every-day practice. We have a plant in Austria, producing 200 tons daily, which actually makes briquettes for 25 c. in American money per ton, besides two plants in Germany which are operated for less than 30 c., including amortization. The figure given by Dr. Weiskopf of 41 c. was obtained, not from an operating plant, but from the estimated operating cost of a one-press plant furnished by the press manufacturers, and this cost is very conservative and away within actual operating costs. At a plant at the works of the Lackawanna Steel Co., which is an experimental plant only, and where no labor-saving devices have been provided for briquette handling, and where the flue dust is screened very roughly through a grizzly at considerable expense, the actual operating cost has been around 59 c. for several months.

It would be very interesting to find out whether the Scoria process can actually make briquettes for 32 c., in view of the fact that the process is much more complicated than is the Schumacher process, in which either pickling liquor or a cheap sulphate of iron is the only substance added to the dust, whereas in the Scoria process some 8 to

10 per cent. of slag must be added, and the slag must be ground to the fineness of cement.

E. STÜTZ:—The figures in my paper were taken from *Stahl und Eisen* and translated into dollars and cents and pounds. As those given in *Stahl und Eisen* were not questioned at the meeting of the Verein deutscher Hüttenleute, where they were presented, there was no need for me to inquire into their origin. I have no operating figures of a plant in the United States. In regard to the expense of preparing the slag, certainly slag has to be taken in the granular state, but the grinding is not a very expensive process. Of course, the material is first hydrated, and in that way is made very soft, and the grinding is more of a grinding and mixing operation.

FELIX A. VOGEL:—As far as the hydration of slag is concerned, there seems to be little foundation for the claims made. After the so-called hydration, which purposes in itself a disassociation of the slag into minute particles, the slag must yet be ground in Chilean mills. The claim of the Scoria patent, as we understand it, is that the slag, when it is submitted to superheated steam, will hydrate, and, furthermore, disintegrate. This, however, does not seem to be the case, necessitating a second operation. You have then two operations: the first one, of submitting granulated slag to superheated steam in mixing apparatus, and the second one, of subsequently grinding the hydrated slag in Chilean mills.

Dr. Schumacher controls a process which is quite similar to the Scoria process. However, the slag is ground fine without preliminary hydration in steam cylinders, the briquettes being hardened under steam pressure only. This process, using only 3 to 4 per cent. of slag and about 0.5 per cent. of lime, is much simpler than the Scoria process, which uses from 8 to 10 per cent. of slag.

E. STÜTZ:—In regard to Mr. Vogel's statement that there is nothing in hydrating, I think there is a great deal in hydrating as a suitable preparation of the binder. By it the briquettes are made extremely porous, and while the binder is hydraulic during its passage through the top gases it is turned into a fusion binder in the hearth. I do not know what objections Mr. Schumacher found to the process, but it is in use and those are the advantages claimed for it.

FELIX A. VOGEL:—I understand there are two plants using the Scoria process in Germany at present, neither of which has shown the operating costs here published.

F. L. GRAMMER, Leesburg, Va. (communication to the Secretary*):—The table offered by E. Stütz of the various agglomerating devices is interesting, but there are influencing factors in selecting a process not there exhibited; for example, the great purity of the Gröndal brick.

It is, however, in Dr. Weiskopf's citation that furnacemen will find valuable thought. Most of us have felt that blast pressure regardless of volume influenced reduction of ores and combustion of fuel. It is true that the best fuel records are found in the charcoal furnaces of Europe, where rich uniform ores are used and a metal of very low silicon is made, and such furnaces exhibit low blast pressure. It is, however, to the ore richness, fine crushing (1 to 1.5 in.), acid slag, low silicon in metal, and absence of sulphur in ore that we have usually given the credit for low fuel consumption.

If two-thirds of the gas produced in high-pressure blast-furnace plants goes to boilers, certainly much less would be required where the blast pressure is 7 lb. than where it is 17 lb. It is only by the greatest plant economies in the use of steam that these pressures were overcome without turning the furnace into a gas producer by lightening the burden to make a richer gas fuel.

In a paper presented at the New York meeting of the Institute in October, 1903, I cite[1] 20 to 30 cu. ft. of furnace space per ton of iron per 24 hr.; this was under the conditions of high pressure of blast and great velocity. The fuel consumptions were not such as to excite remark either way.

If with low pressure and slow driving and the consequent fuel economy such outputs as 1 ton per 35 cu. ft. are obtained, the matter is of great interest.

ERNEST STÜTZ, New York, N. Y. (communication to the Secretary†):—In further reply to Professor Hofman's question regarding hydration temperature of cement, I have consulted Professor Mathesius and am advised that according to previous publications the loss of water begins at 500° C. and increases up to 1,100° to 1,200° C. As far as known, it has not been possible to determine the presence of definite hydrates which are decomposed at fixed limits of temperature.

Some practical experience has been derived, however, from the manufacture of so-called Dinas stones from ground quartz with an addition of from 3 to 4 per cent. of caustic lime. Pressed into brick

* Received Aug. 9, 1913.
† Received Nov. 29, 1913.
[1] *Trans.*, xxxiv, 616 (1903).

shape, these are hardened in cylinders similar to those used in the Scoria process. Also German sand-lime bricks are made in a similar manner but with a considerably larger addition of lime. In the subsequent burning process of the Dinas stones it was found that between 1,100° and nearly 1,500° C. these stones are excessively sensitive to shocks or loads. In the kilns it is therefore impossible to place more than two or at the outside three layers on top of one another, because otherwise the bottom bricks are crushed in the course of the burning operations.

The sintering process between lime and quartz begins to bind only after heating in a very careful manner to nearly 1,500° C. and gives the Dinas stones their well-known solidity. Precisely the same observation can be made with ore briquettes composed of lime and iron oxide or cement and iron oxide. The hydrating binder is destroyed at a temperature between 1,000° and 1,100° C. and the sintering binder becomes active only at higher temperatures, so that the destruction of such briquettes is liable to occur in the blast furnace.

The special advantage of the Scoria process lies in the fact that with blast-furnace cinder, sintering as it does between 1,050° and 1,100° C., the hydrosilicates are destroyed only after the sinter binding has begun to be operative. For that reason, as the operation of the process during a number of years has proved, the Scoria briquettes maintain their lumpy consistency up to the melting zone.

Mr. Lee's paper is highly interesting, as it brings out the point that a saving in coke cannot be claimed for operation with nodulized material.

Permission to publish the coke consumption of German furnaces running on briquettes is unfortunately not obtainable, but it is certainly equal to the best published performances of American furnaces running on good lumpy ores for Bessemer iron.

Another interesting statement of Mr. Lee's concerns the CO_2 : CO proportion as being between 1 : 1.8 and 1 : 2.5 for gas-reduced ores (which is in accordance with German experience), while with ores hard to reduce the proportion is only from 1 : 4 to 1 : 6. In the latter case it seems safe to assume that coke consumption will be still above Mr. Lee's figures of 2,300 to 2,400 lb. per ton of iron.

Assuming a cost of nodulizing about equal to that in Germany (from 75 c. to $1 per ton) with no resulting saving of coke, it appears that these costs are not counterbalanced by any advantages in furnace operation. With Scoria briquettes these conditions are, however, greatly improved and coke consumption is sensibly reduced, completely offsetting any greater cost of installation.

Mr. Vogel's discussion adds little to the general knowledge of the subject of briquetting. It is true that Scoria boasts so far of only two installations, but these are in operation with two of the admittedly most up-to-date German metallurgical establishments, Krupp and Thyssen. Both closely investigated all other methods, including Dr. Schumacher's, and after investigation and trial preferred Scoria. They found that the process did not confine itself only to the briquetting of flue dust, that the solidity of the briquettes did not vary from day to day according to differences in operating conditions, and that it could be used for fine ores together with flue dust or without it.

At the Thyssen plant it was found by lengthy experiments that the Scoria process alone among all others was able to make solid briquettes of the fine Norwegian concentrates (ground to an average diameter of 0.1 mm.) so as to be reducible in the top gases; thereby reducing coke consumption. Even the Gröndal process was not able to make briquettes of sufficient hardness with this material.

The Briquetting of Flue Dust in the United States by the Schumacher Process.

BY FELIX A. VOGEL AND A. M. TWEEDY,[1] NEW YORK, N. Y.

(New York Meeting, October, 1913.)

SINCE the publication of Prof. J. W. Richards's paper on The Schumacher Briquetting Process,[2] this process has been in operation on a practical scale in two plants in the United States, and a few notes on the results obtained may be of interest.

An experimental plant, consisting of a toggle press and a hydraulic press, was installed at Johnstown, Pa., together with the necessary bins and other equipment, and an attempt was made to briquette a mixture of Mahoning fines and blast-furnace flue dust, in a ratio of 7 parts of ore to 3 parts of dust. The results were unsatisfactory, in that the briquettes made were not hard enough to stand the severe handling to which they were subjected in the various transferences necessary between the press and the furnace. The trouble was due to several causes: First, the mechanical arrangement of the plant, which did not lend itself to a proper hardening and loading of the briquettes into the cars. Second, the irregular composition of the flue dust, as to both its lime and its fine coke contents. Third, the variability of the moisture content of the ore, and its viscosity when wet. Fourth, the ratio of fine ore to flue dust; that is, it was attempted to bind in too large a quantity of fine ore for the small amount of flue dust used. The first trouble could be overcome, and has been improved upon in the plant as it stands to-day. The second trouble was important, because of the small amount of dust used as a binder, and might have been easily removed by mixing the flue dust of the different furnaces, which, however, was never done. The third difficulty was more serious, but could have been partly solved, had it been possible to meet this along with the fourth difficulty, by decreasing the percentage of ore in the briquettes. It was proposed to do this, but it seems that the problem at the works in question is, primarily, to prepare Mahoning fines for reduction in the blast furnace, and only secondarily to use up the flue dust, and a solution along this line did not seem to be interesting.

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[1] Non-member.

[2] *Trans.*, xliii, 387 (1912).

In Lackawanna, N. Y., a single-press experimental plant, Fig. 1, was installed in October, 1912, and operated for a trial period until Aug. 1, 1913, when it was taken over by the steel company, and is now being operated by them. The plant, Fig. 1, consists of a single toggle press installed at the end of the regular coke bins, two of which bins have been utilized as storage bins for flue dust. The flue dust, as drawn in hopper-bottom cars from the dust catchers of any one of the seven furnaces, is dumped through a 1-in. wire screen into the dust storage bins. Any coarse coke or limestone caught in the screening is shoveled or raked into an adjoining coke bin. From the storage bins the dust is drawn off through cylindrical spouts, 18 in. in diameter, to two table proportioners feeding a standard pug mill 10 ft. long, of the ordinary single-screw type. In this pug mill, or mixer, water and a 10 per cent. solution of iron sulphate are added, the amount of solution being such that the actual salt added is 0.25 per cent., by weight, of the flue dust. The amount of water is varied with the character of the dust so that, with the water in the solution, the total moisture in the dust as it leaves the mixer is from 8 to 10 per cent. From the mixer the dust feeds directly into the press, where rectangular briquettes 7.75 by 6 by 3.25 in., are formed, under a pressure of 5,600 lb. per square inch. It would be out of place to go into a detailed description of the press, more than to say that it is a self-contained toggle press of the revolving-table type, with 12 molding boxes, and designed to turn out from 18 to 22 briquettes per minute. The pressure plunger is counterbalanced by a counter-pressure plunger in an oil cylinder, under a pressure of 135 atmospheres of compressed nitrogen, which has a dual purpose, in that it insures a uniform pressure on each briquette, besides protecting the press in case of foreign material getting into the molding boxes. Further features are safety shearing pins on the table-drive mechanism, which disengage the table from the main driving shaft in case the revolution of the table is stopped in any way. The press, with ordinary care as to lubrication and adjustment, gives no trouble.

The press is operated at a speed to deliver 20 briquettes per minute, or 1,200 per hour. Briquettes weigh from 11.5 to 12 lb. apiece, so production is at the rate of about 6 tons per hour. The briquettes are discharged automatically from the press table on to a belt conveyor by a "kicker." Sufficient storage space was not provided in the briquette shed for all briquettes made running both day and night turns, so the day-turn briquettes are run on the conveyor directly to flat-bottom gondola cars, where they are taken off by hand and stacked. On the night turn, briquettes are taken off the conveyor by hand and stacked in the shed, where they are allowed to stand about 24 hr., then thrown by hand or loaded by the conveyor again into hopper-bottom cars. Both of these arrangements are very crude and expensive, but of the two, the method of

Practical operations at Lackawanna were on fresh flue dust, that is, dust produced currently from the furnaces, seldom more than a week old, and sometimes so fresh from the dust catcher as to be actually red hot. Experiments were made using mixtures of fresh dust and weathered dust from the stock piles, and it was found that equally good briquettes, if not better ones, are made with 50 per cent. of fresh dust and 50 per cent. of weathered dust, the reason being that the weathered dust is always cool, so that the mixture is cooler than the fresh dust alone, and the cementing reaction takes place more slowly and thoroughly. Dust some two months out of the furnace made very good briquettes, and we are led to believe that at least two or three months of weathering are necessary before dust becomes too inert for briquetting alone.

In a single month of which we have complete records, which is typical of average work, the results of operation show a production of 2,727 tons of briquettes made during 23 working days, or an average of 119 tons per day, at a total cost, including all overhead expense, of 60c. per ton. Of this cost, the largest single item, that of handling briquettes after they are made, would be cut in half, if not further reduced, in any permanent, properly designed plant. The cost of catalytic also would have been reduced if pickling liquor had been used, and the total cost might easily have been brought down to 40c. per ton.

All briquettes made during the test run were used currently on the furnaces, and accounted for themselves in tons of pig iron, without bad effects on the furnaces or on their operation. Furnace losses in flue dust, or in any other way, were not greater than they had been when the furnace carried no briquettes. Small percentages of briquettes were used irregularly on a 500-ton furnace throughout the winter and spring, and to get more conclusive results during the summer, briquettes were used on two 300-ton furnaces, where the effect of the small quantity of briquettes would be noted more readily than on the larger furnace.

A summary of operations on one furnace showed that in June, when 13.71 per cent. of briquettes was carried—the largest percentage of briquettes that had been carried for an entire month—the furnace actually made a little more iron with a smaller coke consumption and less flue dust, 2.26 per cent., than it did in previous months with less briquettes.

When the briquettes were badly broken and used in a large amount, say 25 per cent., on a single furnace, a tendency to tighten was noticed in the furnace. Whether this was due to the physical character of the charge, or to the swelling of the ores, or to excessive carbon deposition from the Mesabi ores, or to some other reason, has not been determined. That it is not serious, and can easily be overcome with a little more experience, is obvious from European practice, where the process is in operation in more than 20 different plants, in a number of which even a larger percentage

of briquettes is used on a charge, with no tightening tendencies, and decidedly beneficial results in furnace operation.

In conclusion, and referring again to the previous papers on the subject and their discussions, it may be said that nothing new has developed as regards the action of the catalytic agent in the cementing action of flue dust, when subjected to pressure. This cementing action is partly due to the formation of a silicate of lime and alumina, similar to the action taking place in the setting of Portland cement, and partly due to the oxidation and hydration of the different oxides of iron to $Fe(OH)_3$. Whatever the action of the catalytic may be, it has certainly been shown that such a catalytic is necessary in order that the cementing action may take place.

Very little direct evidence was secured as to what happened to the coke in the flue dust. It went into the furnace with one result, or a combination of several results. It might have been thrown out again as increased flue dust, if the briquettes immediately disintegrated with heat. This, however, did not happen, as all evidence, in the fact that the briquettes accounted for themselves in tons of pig iron produced without increased losses in any way, is directly to the contrary. Then the carbon either must have combined with the CO_2, producing CO and thus enriching the furnace gases: or it was carried down with the rest of the burden and did its part in the reduction of iron. Probably both these actions take place, but, because of the comparatively small amount of briquettes used for a relatively short time, the records of results obtained were not conclusive. There are so many important and variable features in the operation of a blast furnace that test runs to prove small differences should necessarily cover a longer period than has yet been given in this case.

The adaptability of the process to American conditions has been entirely demonstrated, and we feel that, when thoroughly studied, its cheapness and simplicity, as well as the advantages of its product, will give it as general use in blast-furnace practice in this country as it now has in Europe.

The Use of Nodulized Ore in the Blast Furnace.

BY RICHARD HENRY LEE, LEBANON, PA.

(New York Meeting, October, 1913.)

SINCE the economies in the blast furnace resulting from enriching iron ores are so great, much attention has been paid during the past few years to the various methods of concentrating lean ores, and, as the concentrated ore is usually of extreme fineness, various methods of agglomeration and sintering, of more or less value, have been put into operation. The resulting agglomerated concentrates are generally obtained in the form of briquettes or nodules.

The briquettes naturally divide themselves into two classes: (1) those made by the mixture of some binding agent, and then formed into briquettes under great pressure; and (2) those made by sintering the concentrates, the latter either being formed into bricks before exposure to heat or the sintered mass broken into fragments afterward. Of the first class I will not speak, as they are not used on an extensive scale in this country.

The second class of briquettes, or those made by sintering, are simply masses of melted or fused silicate and oxide of iron mingled with more or less silicates of aluminum, lime, and magnesia.

The fact that the material in a briquette is sintered is proof positive that there are no pores in the material. There are, however, quite a number of holes of greater or less diameter through the substance of the briquette. They are not pores in the common acceptance of that term as applied to ores by furnacemen, as the sides of the holes and channels are fused. In reality the material is simply mill cinder. It is practically untouched by the gases in the upper part of the furnace, and the iron has to be reduced by solid carbon in the boshes and hearth. Briquettes sometimes show a little saving in fuel in the furnace over an ore mixture of large lumps, but that saving is made simply because, owing to the numerous holes and channels through the mass, the briquette is practically composed of numerous small or fine lumps, and, as is well known, the finer the

ore is, the lower the fuel consumption, other things being equal. The limiting condition on fineness is the high pressure developed by the fine ore clogging up the mass of stock in the furnace, and the consequent hanging and slipping, and the irregular distribution of blast.

The value of a sintered ore for blast-furnace use depends largely upon the size and physical character of the individual pieces. It has been proved conclusively with a hard magnetic ore that simply crushing the larger lumps to 1 in. or 0.5 in. in size will materially reduce the fuel consumption. It is also a well-known fact that an ore with a porous physical structure which is readily permeable by gases is preferable to one with a vitreous texture. It is to this class of sintered products that both briquettes made by heat, and nodules belong. Neither have any pores in the proper sense of the word, and consequently both require more fuel than porous ores, such as soft hematites, etc.

The physical structure of any sintered product depends more upon its chemical analysis than upon the process of manufacture. It is not true that nodules are vitreous lumps, and that briquettes have an open and porous structure. That is entirely too broad a statement, for in all products agglomerated by heat the binder is a slag, some combination of an oxide of iron with lime, silica, alumina, or all, and sometimes with magnesia in addition.

If the ore is low in iron, say 50 per cent., then any process of sintering will probably produce a clinker similar to mill cinder; that is, the whole mass will fuse completely. At the other extreme, with a very fine magnetite, for example, it is very difficult to produce any sintering at all. With ores analyzing, say, from 58 to 64 per cent. of iron, the proportion of acid to basic elements in the gangue is very important. With Cornwall ore the most infusible mixture is formed when the quotient of the bases (lime and magnesia) divided by the acids (silica and alumina) is between $\frac{1}{3}$ and $\frac{1}{5}$. The ideal result is obtained when just sufficient slag is formed to bind the grains together in a solid but porous mass.

It is important to have the proper temperature while sintering, and this temperature will of course depend upon the chemical analysis of the materials. It is also important economically to burn the maximum of fuel for the maximum tonnage, and it may therefore be advantageous in certain cases to alter the chemical composition of the material by the addition of lime, or, if the product is a concentrate, by varying the ratio of concentration. Blue billy is one of the easiest ores to treat by any process of sintering, and flue dirt, even after the

coke is separated, will almost invariably fuse to a product resembling mill cinder.

The ideal conditions for either briquettes or nodules rarely occur in practice, so that, in estimating the value of these sintered products for a furnace mixture, both briquettes and nodules must be taken as composed of about 98 per cent. of mill cinder and 2 per cent. of porous ore.

Nodules from concentrates made in a revolving kiln, when of the proper chemical composition (in the case of nodules made from concentrated Cornwall ore this has been shown by experience to be about: Fe, 58; SiO_2, 8; Al_2O_3, 2; CaO, 3; and MgO, 3.50 per cent.), run in fines from 0.75 in. in diameter to powder passing through a screen 100 meshes to an inch. The amount of such fines, however, is extremely small. Some monthly averages of screen tests taken at random from our reports show:

Mesh.	No. 1.	No. 2.	No. 3.	No. 4.	No. 5.
Inch.	Per Cent.	Per Cent.	Per Cent.	Per Cent.	Per Cent.
On 0.75.....	21.3	40.0	27.8	38.7	15.8
On 2.5.....	23.0	32.0	32.3	22.7	26.8
On 5.....	21.3	18.0	19.4	19.3	35.0
On 10.....	12.3	5.0	9.7	8.7	16.6
On 20.....	13.9	3.0	6.4	6.7	3.2
On 40.....	6.6	1.0	3.2	2.7	1.3
On 60.....	0.8	0.5	0.8	0.6	0 6
On 110.....	0.4	0.5	0.4	0.3	0.6
Through 110.....	0.4	0.0	0.4	0.3	0.6
Through 20 mesh.	8.2	2.0	4.8	3.9	3.1

We find by the experience of six years that when not more than 2 per cent. of the nodules pass the 20-mesh screen the very best results are obtained in the furnace, as to both fuel and production. A large percentage over 0.75 in. in diameter is of no especial advantage except in lightening the pressure of the blast to a slight degree, and the greater the percentage around 0.25 in. in diameter, the better; above this size the nodules seem to require more fuel. Below it the driving of the furnace is retarded by the increased difficulty of blast penetration and distribution.

The size of the nodules depends very greatly upon the composition of the ore, as above stated. Sometimes when a lot of lean ore is delivered to the concentrator the resulting nodules may run as low as 54 per cent. in iron, with a corresponding increase in the percentage of gangue, but the nodules are always quite coarse; as much as 60 per cent. will remain on the 0.75-in. screen. In this case the blast pressure will at once lessen and the furnace will drive faster.

Again some rich ore will go through the concentrator, bringing the iron in the nodules up to 60 and even 62 per cent.; in this event the nodules will be much finer; sometimes as much as 30 per cent. will pass through the 20-mesh screen. After the furnace has been filled for 6 or 8 hr. on this mixture the pressure will go up, the driving will slacken, and unless precautions are at once taken the sulphur in the iron will run up, and there may be sticking and hanging unless the pressure be kept down to reasonable limits. As the Pennsylvania Steel Co. at its Lebanon plant runs almost all the time upon low-phosphorus, low-sulphur iron, with whatever silicon the customer demands, it is of great importance that conditions in the furnace be kept regular. The Cornwall ore itself as delivered to the concentrator will vary from 30 per cent. iron to 45 per cent., with similar variations, of course, in the gangue. To get an absolutely regular product commercially and under working conditions with such varying ore is manifestly impossible, but by attention and study the nodulizing plant has reduced the variations in the iron content of the nodules to an average of from 56 to 60 per cent., and by using a series of holding pits for the concentrates the daily variation in the nodules is rarely over 2 per cent. in iron, with corresponding differences in the amount of gangue. The ratio of SiO_2, Al_2O_3, CaO, and MgO among themselves varies hardly at all, so the net result of variations in the iron of the nodules means an equivalent change in the free SiO_2 to be fluxed. As practically all the iron made at the Lebanon furnaces must be low in sulphur (below 0.035), and the silicons demanded are generally low, it is of the greatest importance that the composition of the slag be kept regular, especially so as the coke runs rather high in sulphur, requiring a basic slag at all times, even when on high-silicon iron.

To enable the furnace department to know the composition of the nodules before they are charged into the furnace, the Superintendent of the concentrator, Mr. McKechnie, has his samplers take a sample from the nodule kilns every hour. These samples are mixed, and every morning a sample from this mixture is taken to the laboratory for analysis, and the result is sent to the furnace office. By this means, before the nodules are filled into the furnace their composition is known, and any change in the lime required is made. No attention is paid to the variations of the iron. Generally speaking, the same fuel is required whether the nodules run 56 or 60 per cent. The leaner nodules, being larger, take more fuel, while the richer ores, although more easily smelted because finer, yet pack more than the coarse, making the gas distribution less perfect, which in turn

increases the demand for fuel, so that practically we find little or no difference between 56 and 60 per cent. nodules. The Lebanon plant of the Pennsylvania Steel Co. is the only one that runs a furnace exclusively upon nodules, and consequently a few remarks upon the behavior of such a furnace may be interesting.

The furnace at present in blast is 80 ft. high, 11 ft. 6 in. hearth, and 18 ft. bosh, with 18 ft. stock line, filled by a skip with bell bottom emptying over the hopper. The bell lever projecting over one side of the hopper has a tendency to create a channel through the stock, owing to its intercepting the materials falling upon it and causing the larger lumps of coke to roll under it. This, however, we combat more or less successfully by means of baffles and other means. There are 10 6-in. tuyeres, and on an average about 30,000 cu. ft. of air per minute is blown, which is nearly the usual quantity needed for a furnace of that size on porous ores. Indeed, we find that 35,000 cu. ft. is not too much as far as fuel consumption is concerned, but when more than 30,000 cu. ft. of air per minute is put into the furnace the loss in fine nodules blown out of the top is excessive. The slight gain from increased tonnage is offset by an increased amount of nodules required per ton of iron.

Until the concentrating and nodulizing plant at Lebanon was installed the ores used were entirely roasted Cornwall magnetite, running irregularly from 30 to 45 per cent. in iron, from 12 to 25 per cent. in SiO_2, and alumina, lime, and magnesia in varying amounts, but constant relatively to themselves and to the acids, so that, as with nodules now, the free silica present determines the amount of lime to be added to or taken off the charge. Fortunately, Cornwall ore carries a considerable amount of magnesia, which largely increases the liquidity of the cinder. Were it absent, running a furnace on all Cornwall ore would be almost impossible, as in 24 hr. the silica and alumina in the cinder may change from 48 to 38 per cent., and without from 8 to 12 per cent. of magnesia the low-silica slag would be so refractory and stiff that it would not run through the cinder monkey. In fact, the presence of magnesia in the Cornwall ore not only enables a furnace to run alone on this ore, but renders it possible to make low-silicon iron with low sulphur. Often with both roasted Cornwall ore and nodules it is necessary when the coke is high in sulphur to run for months at a time on a cinder carrying only from 40 to 42 per cent. of silica and alumina, and such a cinder would soon clog up a furnace were it not that the magnesia renders such a cinder, in spite of its basicity, as liquid and as free-flowing as the ordinary 48 per cent. cinder with lime alone as a base.

In addition to the daily nodule analysis, we also watch the coke very closely, both as to the quality of the coal used and as to the sulphur and ash. From the nature of the coke operation (we use Semet-Solvay by-product coke, with ovens at the plant), it is not possible to secure an analysis of the coke before it is charged into the furnace, but the ash does not vary much; the sulphur, while high, is also pretty regular, but the coals used do at times vary greatly in the mixture, although we use every endeavor to keep the coal mixture as regular as possible. This regularity in coal mixture I regard as the most important single item making for success in blast-furnace operation, not only in making low-sulphur iron from high-sulphur materials, but with all ores on all kinds of iron. This point has not had the attention paid to it that nearly all the other elements concerned in making iron have had from time to time, but in my opinion, based on nearly 30 years' practical direction of blast furnaces, nine-tenths of all furnace troubles would disappear were it possible to always give a furnace a well-burned coke made from a regular unvarying mixture of coals.

A report of the different tonnages of coal charged each day is sent to the blast furnace office every morning, and as that coke is not pushed until the next day, we know exactly what mixture is in the furnace, and when any coke of a changed mixture will be filled. As different coals, even when the oven temperatures are the same, differ enormously in the quality of coke they make, this knowledge is of great value, and enables the furnace superintendent to take any measures he judges best to meet the change. The hardness of the coke made is of great importance in the successful working of nodules.

The nodules themselves, being a fine ore, fill more or less the openings between the pieces of coke, and if the pieces of coke are soft, and crumble with a large percentage of breeze, it can easily be seen that the blast will have great difficulty in penetrating and distributing itself through the resulting mass. On the contrary, with hard coke and coarser nodules no difficulty whatever is experienced in using all nodules; the pressures are not high, the fuel is moderate, and the furnace works far more regularly than with Mesabi ores. In fact, all difficulties in working nodules come from softness of the coke and the fineness of the nodules. Where these two elements are absent, a furnace will work as well on nodules of any chemical composition as on any other iron-bearing ore. The chemical composition of the nodules can be taken care of by proper fluxing, but for packing in the furnace through soft coke or excessively fine nodules there is no remedy whatever, and where these two conditions hold

a furnace will never run so successfully on all nodules as on gas-reduced ore or ore in lumps.

The amount of fuel needed per ton of iron made from nodules, using well-burned by-product coke made from a mixture of mountain coals, runs from 2,300 to 2,400 lb. in good practice. Little iron is reduced by the gases because of the nature of the nodules, which are but small lumps of mill cinder, requiring solid carbon for reduction; but if the coke happens to be soft the fuel will increase enormously, a vast amount being consumed in the upper part of the stack by the CO_2. With first-class Connellsville beehive coke the fuel would be less, because we have generally noticed that whenever we have charged Connellsville coke the silicon in the iron will jump up, sometimes being raised from around 1.5 to 2.5 per cent. in the casts made on the Connellsville coke.

A very important point in working on all nodules is the location of the melting point. When too low, although the silicon may be high and the furnace hot, yet the gases will be thin, the pressure very high, and the furnace will slacken in driving to hardly more than half the usual gait. On the other hand, a too high melting point will give gases which, while extremely abundant, will burn with difficulty on account of their dustiness. The pressure will be lower than normal, and the driving very fast, but the burden carried will be less than in the best conditions, the hearth cooler, and the sulphur kept down in the iron with great difficulty, but with a good production of iron.

Of course these conditions depend entirely on physical causes. In the first instance, the melting or rather sticky point is in the bosh or narrowing part of the furnace, and the area there being contracted there are fewer passages for the blast to go through the sticky mass. In the second case, where the melting point is high, the area of the mass is greater, affording more passages for the gases, and consequently the pressure is less. The important point is to have this sticky level at just the right height, which can easily be told by the general behavior of the furnace, its fuel economy, and its production and quality.

The variations in fineness in the nodules require pretty careful watching of the penetration of the blast, and to find out whether the blast is uniformly distributed over the tuyere area the old-time test is used: viz., driving a bar through a tuyere into the center of the furnace as quickly as possible, allowing it to remain for 2 min., withdrawing it rapidly, and examining the bar to see whether it is uniformly heated, or hotter in spots. With nodules it seems to be even

more important to have a uniform heat over the tuyere area than with ores reduced by the gases. The gases themselves are, like all furnace gases made from hard non-porous magnetites or mill cinders, very low in CO_2 and high in CO, the ratio between the CO_2 and CO being from 1 : 4 to 1 : 6 instead of from 1 : 1.8 to 1 : 2.5 as with gas-reduced ores; consequently they are much less inflammable, and gas explosions are very rare or unknown while the blast is on the furnace.

With nodules, as with all other ores, a moderate-sized furnace seems to be more economical than the jumbos. All difficulties from soft coke and fine nodules are increased when our 100-ft. furnace is working on all nodules, and harder coke and coarser nodules are required for success than with the 80-ft. furnace.

In conclusion: with a hard regular coke, and nodules of which 95 per cent. will remain on a 20-mesh screen, there are no especial difficulties in working on all nodules, provided some procedures dictated by experience and probably different at every plant are observed. With soft coke and very fine nodules trouble will be had at any furnace and under all conditions of management.

The slag volume required for working with nodules is, within reasonable bounds, not very important.

Discussion.

J. E. Johnson, Jr., New York, N. Y.:—This subject is of great and increasing importance, as the exhaustion of the deposits of ore of ideal character compels the iron industry to turn more and more to those which are less desirable on account of physical structure as well as analysis. Thus there has come about the use of the Mesabi ores in this country and also of the Minette ores in Europe, which produce far greater percentages of flue dust than did the lumpy ores first used. It is a matter of common knowledge that the utilization of this flue dust, containing commonly 50 per cent. or more of metallic iron, has been a problem very difficult and tedious of solution. The number and variety of the solutions now offered is a sufficient proof of this.

I have no intention of going into the merits or demerits of any of these processes as such, but there are certain considerations concerning the effect of their product on the operation and fuel economy of the blast furnace which I believe to be new and which I desire to bring out.

There are two methods, so different as almost to be direct opposites of each other, whereby an ore may exercise an unfavorable effect on the economy of the blast furnace. The first of these is well

known and results from the ore being refractory and highly resistant to the action of the reducing gases of the blast furnace. This results in its being incompletely reduced when it reaches the zone of fusion and in the reduction being finished by the direct action of carbon in the lower portion of the furnace. This is so well understood as to need little elaboration or comment.

There is, however, another action which may be equally as fatal to the economy of the furnace as the foregoing, and as this point is, to the best of my knowledge, not generally appreciated, it is one upon which I desire to lay particular stress. This consists in the ore being, in a sense, too reducible.

A short time ago I received authentic data giving some details of the present operation of one of the largest and best plants in the country. I was struck by two salient points in these data: First, that the fuel consumption averaged about 2,150 lb. of coke, analyzing 88.5 per cent. of fixed carbon; second, that only about 51.5 cu. ft. of air at 60° F. were required to burn a pound of coke. I knew that this plant had formerly made fuel records over long periods of about 1,700 lb. of coke, and while I had no figures of blast required per pound of coke in that practice, I had figures giving this for very similar practice at another plant and at about the same time. This figure was 70 cu. ft. of air per pound of coke.

These two facts, considered in conjunction with some extensive investigations I made earlier in the year concerning charcoal practice, gave the explanation of these two great changes: namely, in coke consumption and in blast requirements.

In the earlier days, before the introduction of the Mesabi ores as a major portion of furnace burdens, the old range ores constituted the principal ore supply. These are exclusively of two kinds—lump and soft—the soft being so sticky that it will stand on an almost vertical slope, and will scarcely run at all. Therefore, when these ores were charged into the furnace they could not and did not run down into intimate contact with the coke, except on the top surface of the latter, with direct contact between the two virtually only in one plane. With the Mesabi ores all this was changed. These ores, being fine and sandy, as soon as they are dried in the upper regions of the furnace run almost like a liquid down through the interstices of the coke as the charges descend and the points of contact between the two are increased to a vast extent.

It is well known that the oxygen of ore in contact with carbon will attack the latter and carry it away with extreme rapidity, irrespective of the composition of the surrounding gas and throughout the entire

range of temperatures occurring in the upper regions of the furnace. This well-known action could go on in the older practice only along the single plane of contact between the ore and the coke, but with the great preponderance of Mesabi ores, which run down freely through the interstices in the coke, it can now take place throughout almost the entire coke charge, thus dissolving away a great percentage of the coke in the upper levels of the furnace. This is proved by the small quantity of blast required to burn the coke.

The theoretical quantity required for this purpose may be easily calculated: 2,150 lb. of coke at 88.5 per cent. fixed carbon contains 1,900 lb. of fixed carbon; deducting from this 90 lb. to cover the carbonization of the iron and a little loss through gas escaping around the bosh, we have 1,810 lb. of carbon to be burned per 2,150 lb. of coke charged into the furnace, or 84.1 per cent. of the weight of the coke.

The oxygen required to burn a pound of carbon to CO is $1\frac{1}{3}$ lb. The air required is $\frac{100}{23}$ times as much, or 5.8 lb. of air per pound of carbon. As the coke gives only 84.1 per cent. of fixed carbon to be burned, this would require 4.9 lb. of air. Air at 60° F. weighs 0.0763 lb. per cubic foot. Therefore, 64.2 cu. ft. would be required to burn a pound of coke at the tuyeres under these conditions. As a matter of fact, only 51.5 cu. ft., or 80 per cent. of this amount, was required. In other words one-fifth of the whole amount of coke must have been dissolved by the direct action of the ore and carried out the top of the furnace without ever getting into the hearth at all.

It may be well to state here that the figures of blast actually used were obtained by an expert and covered the results of an investigation extending over several years.

If any mistake due to leakage occurred, its effect would be to diminish still further the quantity of air actually used and so make the case stronger instead of weaker.

These figures enable us to determine with considerable accuracy the amounts of carbon actually burned in the hearth in the two cases. For present conditions we have 1,810 lb. of carbon available for the hearth, of which 80 per cent., or 1,450 lb., is actually burned by the blast. In the old practice 1,700 lb. of coke at 88.5 per cent. of fixed carbon gave 1,505 lb. of fixed carbon. Deducting from this 90 lb. for carbonization of the iron, etc., we have left 1,415 lb. Of this we may assume that 5 per cent. was lost by solution by the gas in the upper portion of the furnace, leaving a net of 1,350 lb. to be burned by the blast. This is just 100 lb. less than reaches the hearth under present conditions. The difference is to be accounted for by the rela-

tive leanness of the ores used to-day as compared with those used in former practice.

It is commonly considered that the decreased percentage of iron in the ore and the simultaneous increase in silica is wholly responsible for the decline in fuel economy above mentioned, but, in my judgment, this belief is without adequate foundation. It is true that the lumpy ores of Lake Superior averaged at one time almost 60 per cent. in iron and that the average furnace burden to-day averages only 50 or 51 per cent., but this change is due in large part to the fact that the softer and finer ores contain several times as much moisture as the lumpy ores of an earlier day. Five or six per cent. of moisture was normal 20 years ago, whereas many ores to-day contain from 12 to 15 per cent. and not infrequently 5 or 6 per cent. of combined water in addition. The increase in silica has probably not been as great as might appear on casual examination, for the same reason. It is doubtful if furnace burdens ever averaged less than 5 per cent. in silica and the bulk of the ores in use to-day probably do not average over 8 per cent. It must not be forgotten either that there has been an increased tendency to increase the silica in the mixture deliberately in order to get a larger slag volume, this probably being necessitated by the greater volume of sulphur which the increased coke consumption introduces into the furnace. Looking at the matter from another point of view, it is doubtful if the slag volume per ton of iron to-day is greater than 1,100 or 1,200 lb., and in the old days it was probably never less than 800 lb. Such an increase in the slag volume should produce an increase in the coke consumption of only about 100 lb. as against an increase of four or five times that amount which has actually taken place. For these reasons, the view that the increasing leanness of the ore accounts for the increase in fuel consumption does not seem to be tenable, but rather that the change is caused chiefly by the physical condition of the ore.

I was recently told by some friends that the best figures obtainable from the use of the Dwight-Lloyd cellular sinter in furnaces increased the fuel economy of those furnaces to a greater extent than could be accounted for by the greater richness of the material. I confess that I received this statement at the time with that politely concealed skepticism which is so frequently necessary in such matters, but after realizing the enormous loss caused by excessively fine ores in modern practice it occurred to me that there was probably much more truth in the claims of the advocates of a cellular sinter than I had at first been willing to admit. My experience with the dense varieties of nodules which may be called "artificial magnetites" has been such

as to make me extremely doubtful of any claims of increasing fuel economy by their use, especially in conjunction with more reducible ores, but this spongy or completely cellular material must be admitted to have the maximum of advantage from the point of view I have just attempted to bring out. The material is lumpy and angular, and therefore has a minimum of points of contact with the coke, and for this reason the solution loss of the coke must be small, as with lump ores. These properties, due to its peculiar structure, mark it as a distinctly new metallurgical product.

On the other hand, the material is of so open a structure that a gas current can pass through it as freely as the draft passes through it during the process of its production. Its cell walls are extremely thin and therefore its exposure to the heating and reducing action of the gas is a maximum, with a minimum of exposure to the coke, thus simultaneously promoting indirect or gaseous reduction and cutting off direct reduction through contact with the coke, with corresponding benefit to the fuel economy.

It is easily conceivable that a layer of this material charged immediately on top of the coke would act as a mattress to cut off the passage of finer ore charged on top of it and so promote economy to an even greater extent than the quantity charged would lead one to estimate.

The artifically prepared ores from the rotary kiln have not these advantages. It is true that they are in larger pieces than the fine Mesabis, but their bulk per unit of weight is not nearly so great as that of the cellular sinter, while the small round pellets which comprise so great a portion of the whole mass of nodules are in the ideal state for running down through the coke to the maximum possible extent. On the other hand, the pores which may be formed in this material to permit egress of the gas evolved on heating it are sealed and welded shut by the continual rolling which it undergoes during the process of formation. The nodules also have a minimum ratio of surface to mass; therefore their exposure to the gas is the smallest possible. For this reason this material is likely to reach the zone of fusion very largely unreduced, requiring to be reduced there with direct carbon and high heat.

I have operated a furnace on a mixture of three-quarters brown ore and one-quarter of such nodules, and I know that more fuel was required as well as greater time in the furnace and consequent reduction of output, though the furnace was very slowly driven to begin with.

Considering these facts, it seems to me likely that a burden of a

cellular sintered material should give better fuel economy than either fine reducible ore on one side or nodulized ore on the other. The figures that I have given above from actual practice show the loss resulting from the use of fine ore, and are an indication that this saving may be of very great commercial importance. They suggest that making a cellular sinter of the fine portions of Mesabi and other ores and concentrates would effect a great economy in the furnace, in spite of the current opinion on the subject. It points a way toward attaining the low fuel economy of 20 years ago.

HENRY M. HOWE, New York, N. Y.: If that theory were true, wouldn't it be immediately reflected in the compositions of gases? Wouldn't you have an immediate check on that theory in that way?

J. E. JOHNSON, JR.:—Any change in fuel economy is always shown absolutely in the composition of the top gases, but we have no means of distinguishing oxygen which comes from the air from oxygen which comes from the ore, and consequently low CO_2 in the top gases does not indicate whether we have a high solution loss or bad hearth conditions. The top-gas analysis is only an indication of a condition which shows itself much more quickly and plainly in other ways than it does in the gas analysis.

The Cleaning of Blast-Furnace Gas.

BY W. A. FORBES,* NEW YORK, N. Y.

(New York Meeting, October, 1913.)

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INTRODUCTORY.

DURING the early stages of blast-furnace practice, the gas formed in the furnaces by the combustion of the coke used in smelting the ores was allowed to escape into the atmosphere. The light furnished

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by the combustion of this gas as it reached the air was a familiar sight in the days when open-top furnaces were in vogue. As blast-furnace practice progressed, however, involving the use of hot blast and the development of power from the gas, blast-furnace construction was modified to conserve this gas for heating hot-blast stoves, raising steam under boilers, and various miscellaneous purposes.

When the use of blast-furnace gas was extended to the driving of gas engines, the necessity for thoroughly cleaning the gas became apparent, and soon the advantages of also properly cleaning the gas for use in stoves and under boilers became more fully recognized.

Reasons for Gas Cleaning.

Blast-furnace gas as it issues from the stack is laden with various amounts of a fine gritty dust composed of particles of ore, coke, and limestone. This dust has a bad effect in hot-blast stoves and under boilers, where a large proportion of the gas is ultimately consumed, as it reduces the heating efficiency of the stoves by clogging up the checker openings and chambers and by fusing with the brick-work. It reduces the efficiency of the boilers by fouling the combustion chamber and by building-up around the tubes. In both cases, the efficiency of the combustion is impaired by the presence of dust in the gas.

The presence of even small quantities of dust in the blast-furnace gas prevents its economical use in gas engines on account of the gritty dust particles cutting the pistons and cylinders of the engines, causing frequent stops for cleaning and for repairs and renewals. The separation of the dust from the gas, therefore, becomes a matter of great importance from an economical standpoint in order to obtain the best results from the gas.

Thorough cleaning of blast-furnace gas was never attempted until the gas began to be used in gas engines, when the necessity of devising methods for removing the objectionable dust became apparent. Prior to that time, each blast-furnace plant handled the dust in its own way to best suit the local conditions. Thus, in England, practically no attempt was made to remove the dust from the gas, which was allowed to issue directly from the blast furnaces into the gas mains which conveyed the dirty gas to the stoves and boilers. As these mains became clogged with dust, which condition naturally occurred at frequent intervals, the blast furnaces had to be shut down and the dust removed. The gas mains were usually laid underground, adding to the trouble and expense of cleaning, as well as the danger from gas poisoning and explosions.

First Methods of Separation of Dust.

In Germany, where the advantages of using cleaned blast-furnace gas were more generally recognized than in England, attempts were made at first to separate the dust from the gas by various systems of spraying with water in the gas mains, but the resulting slime was difficult to handle, and this method was not widely followed.

It was noticed that more dust was deposited in the gas mains at points where the direction of the gas current changed, or where the mains increased in diameter, and this led to the idea of dry cleaning, by the introduction of dust catchers of large diameter; of cyclone separators, where a centrifugal motion was imparted to the gas by arrangement of its mode of entry into the apparatus; and of zig-zag gas mains, where the course of the gas was continually changed up and down. The deposited dust could then be readily removed by means of valves in the bottom of the apparatus. Dust catchers of various types were developed, the gas entering radially or tangentially, thereby facilitating dust deposition by centrifugal force. A further modification allows the gas to enter the dust catcher tangentially at the top, and to leave the dust catcher through a central pipe projecting deep into the dust catcher.

The basic principle of the separation of dust in these apparatus is the change of direction in the flow of the gas, together with the decreased velocity of the gas in the dust catchers and separators due to their having greater transverse sections than the gas mains.

Amount of Dust Produced by the Blast Furnaces of the United States Steel Corporation during 1912.

Dust conditions in blast-furnace gas are more severe in the United States than in Europe on account of the larger proportion of very fine ores used in the American furnaces, and on account of the harder driving of the furnaces, which causes more dust to be blown over. As an example of blast-furnace practice in the United States, during the year 1912, 98 of the blast furnaces of the United States Steel Corporation drew their ore requirements from the Lake Superior district. They produced 13,090,411 tons of pig iron, out of a total of 13,990,329 tons produced by the Steel Corporation during the year. Of the Lake Superior ores used, 81.5 per cent. was Mesabi ore, which is physically very fine. There were 1,291,512 gross tons of dust, containing approximately 52 per cent. of iron, separated from the blast-furnace gas.

The United States Steel Corporation has installed several plants for briquetting and sintering the dust separated from blast-furnace gases,

with the object of ultimately consuming all the dust in the blast furnaces. Of 1,291,512 tons of flue dust produced in 1912, 788,933 tons were returned to the furnaces without other treatment than spraying or soaking with water; 181,149 tons were converted into briquettes and sinter, and the greater part of the remaining 321,430 tons was stocked until such time as it is possible to use the material in some form or other.

Dust Content in Gas.

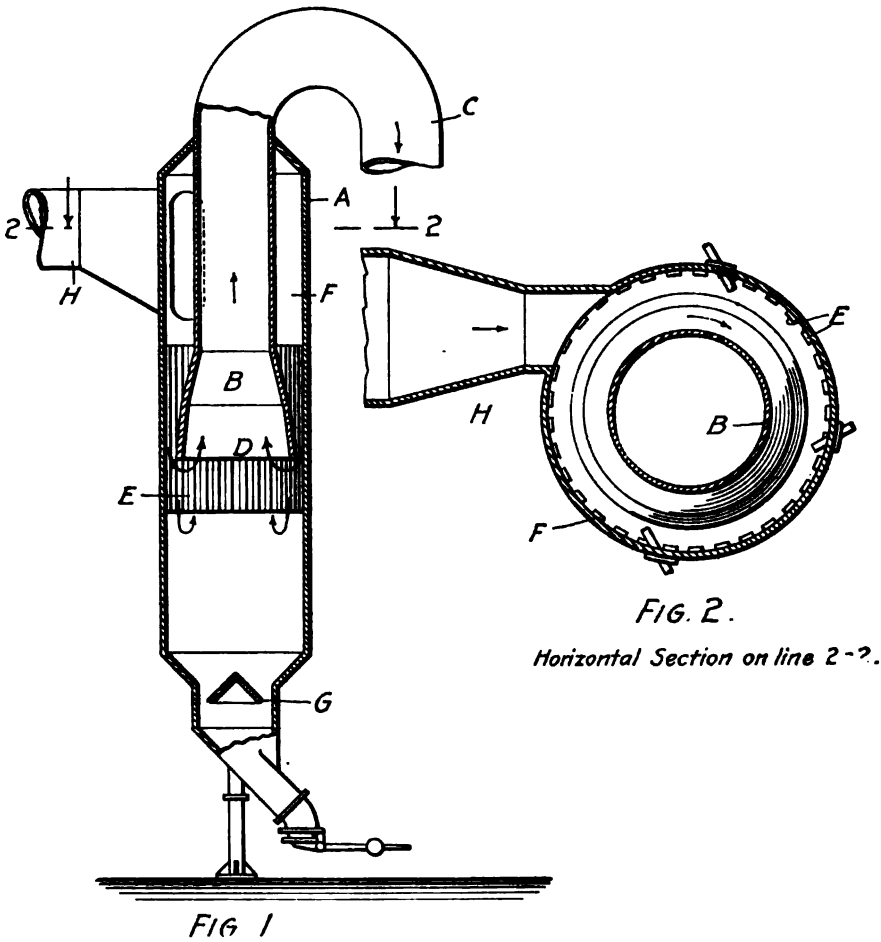
The gas leaving the usual dust catcher contains an average of from 3 to 4 grains of dust per cubic foot, and its further cleaning is accomplished in one or two principal stages, depending on the ultimate use of the gas: namely, primary cleaning and final cleaning. In primary cleaning, the gas is sufficiently cleaned for economical use in heating hot-blast stoves and for raising steam in boilers; it has been found that the best results are obtained when the dust content of the gas, after cleaning, does not exceed 0.2 grain per cubic foot. In final cleaning, the gas is sufficiently cleaned for use in gas engines, and in this case the best practice has resulted when the dust content of the gas, after cleaning, does not exceed 0.008 grain per cubic foot.

Various systems and methods are employed for accomplishing the desired results. In modern practice, the gas leaving the blast furnace is, in practically all cases, conducted through downcomer mains and then through a dust catcher of large capacity, and in some cases through two such dust catchers in series. A considerable proportion of the heavier dust is deposited at this stage. From the dust catcher the gas passes to the additional cleaning apparatus through gas mains, usually equipped with downtakes and valves for the removal of the deposited dust. The mode of treatment from this point on varies considerably, according to the opinions of the operators as to the respective merits of various systems.

Primary Dry Cleaning.

For primary cleaning, a separation of the dust without the use of water—in other words, dry cleaning—has been in favor at many plants on account of the ability to thus conserve the sensible heat of the gas, which is lost when water is used. The fact, however, should not be lost sight of that the benefit derived from the sensible heat of the dry-cleaned gas is largely discounted by the amount of water vapor in the gas. This is especially the case with gas from blast furnaces operating with a high top temperature and using ores and coke containing much moisture existing either free or chemically combined; the water vapor affects the efficiency of the combustion of the gas.

An additional benefit of dry cleaning lies in the greater facility to recover and handle the flue dust in the dry state than in the form of mud or slime in the wet cleaning processes. As before stated, the basic principles in practically all of these dry cleaning systems depend upon a change in the direction of the gas, a reduction in its velocity, and the separation of the dust by gravity and centrifugal force. The



FIGS. 1 AND 2.—BRASSERT-WITTING WHIRLER.

various modifications by which this separation of dust is accomplished are all evolved from the so-called cyclone processes developed in Germany about 20 years ago. Some of these systems recently developed in the United States are the Brassert-Witting, the Roberts, the Kennedy, and the Dyblie. A description of the Brassert-Witting whirler and of the Dyblie whirler will illustrate the general principles of this type of cleaner.

Brassert-Witting Whirler.

As shown in Figs. 1 and 2, the Brassert-Witting whirler consists of a vertical outer cylindrical casing, *A*, and an inner inverted tube, *B*, which at its upper end is integral with the gas main *C*, which takes the cleaned gas away from the apparatus. This inverted tube is flared at its lower end, *D*; a number of iron or steel bars, *E*, are

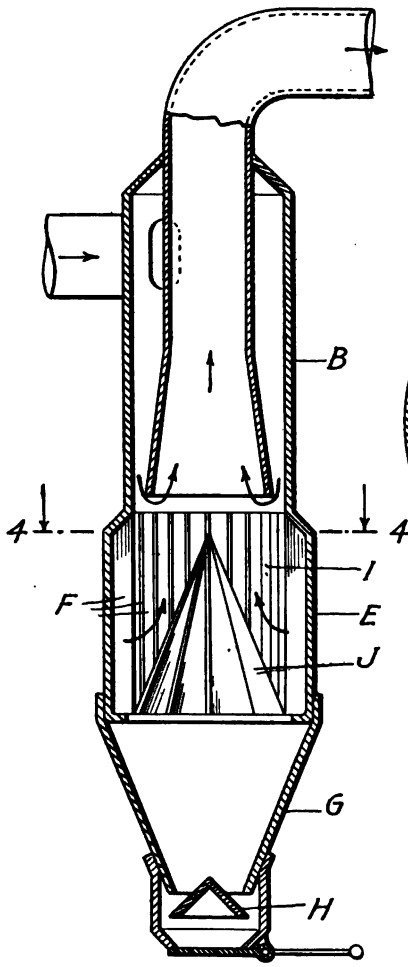


FIG. 3.

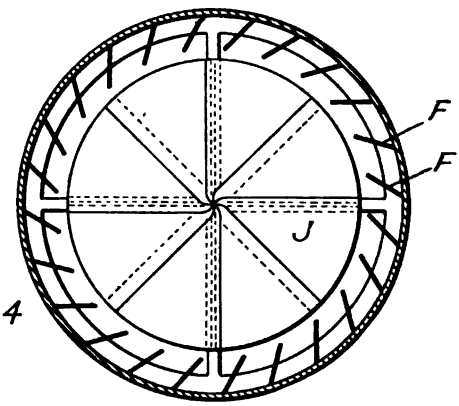


FIG. 4.

Horizontal Section on line 4-4.

FIGS. 3 AND 4.—BRASSERT MODIFICATION OF BRASSERT-WITTING WHIRLER.

fastened vertically around the chamber *F* and extend from a point well above the lower edge of the flared end *D* of the pipe to a point well below the lower edge of this pipe. In the lower part of the chamber *F* is placed a cone, *G*, which allows the separated dust to enter the outlet pipe.

The gas enters the apparatus tangentially through the flue *H* and is given a rotary whirling motion through the annular space between the pipe *B* and the wall of the chamber *A*. On coming in contact with the bars *E* the dust is caught in the channels between these bars and is held in position by the combined action of centrifugal force and friction. As the gas continues to rotate within the annular spaces above mentioned, its velocity is gradually increased by the action of the flared end of the receiving pipe until when it reaches the lower edge of the end its velocity is at a maximum. On passing below this edge, the velocity is constantly decreased, the direction of the gas is changed and it passes upwardly through the flared end of the pipe to the outgoing gas main *C*.

The dust which has been caught in the channels between the baffle bars drops vertically into the bottom of the chamber, past the cone *G* and into the outlet pipe, whence it is removed as desired.

Brassert Modifications of Brassert-Witting Whirler.

In the Brassert modifications of the Brassert-Witting whirler, a sketch of which is shown in Fig. 3, the lower portion *E* of the casing is larger in diameter than the upper portion *B* and is provided with a series of inwardly projecting baffle plates, *F*. The lower portion *G* of the casing *E* is cone-shaped and constitutes the dust-receiving chamber. In the bottom of this chamber is a cone, *H*, whose function is to direct the dust toward the periphery of the dust outlet pipe. Within the chamber *I* another cone, *J*, is located and this cone is provided with a series of baffles, which are arranged as shown in Fig. 4.

In Fig. 5, which is a further modification, a spiral, *L*, is provided for the purpose of directing the flow of the gas. The lower end of the outlet pipe is made barrel-shaped. The outer casing *M* in its lower portion *N* is supplied with the baffles *O*, which instead of being mounted on the casing are bent inwardly therefrom, thus forming the apertures *P* (Fig. 6). The section *N* of the casing is inclosed by an outer casing, *Q*, thus forming the annular chamber *R*, in which partitions *S* are placed to prevent whirling of the gas in this annular chamber.

The gas is introduced and discharged and the dust is separated in a similar manner to that mentioned in the description of the Brassert-Witting whirler.

Dyblie Whirler.

In this whirler, as in most of these types, the separation is accomplished by combined centrifugal force and the action of gravity. One

of the principal features in this particular whirler is the arrangement in the spiral separator of the entrance and exit openings in substantially the same horizontal plane, obviating the necessity of the gas changing its direction of flow at a sharp angle.

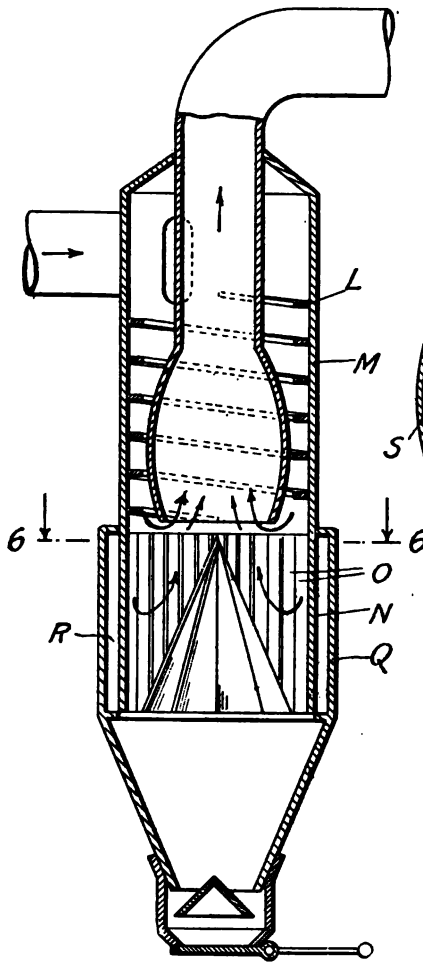


FIG. 5.

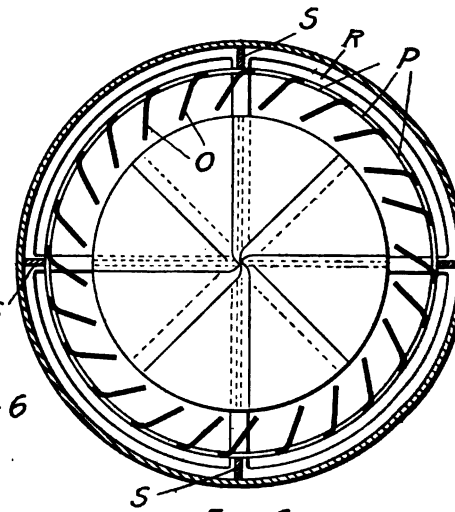
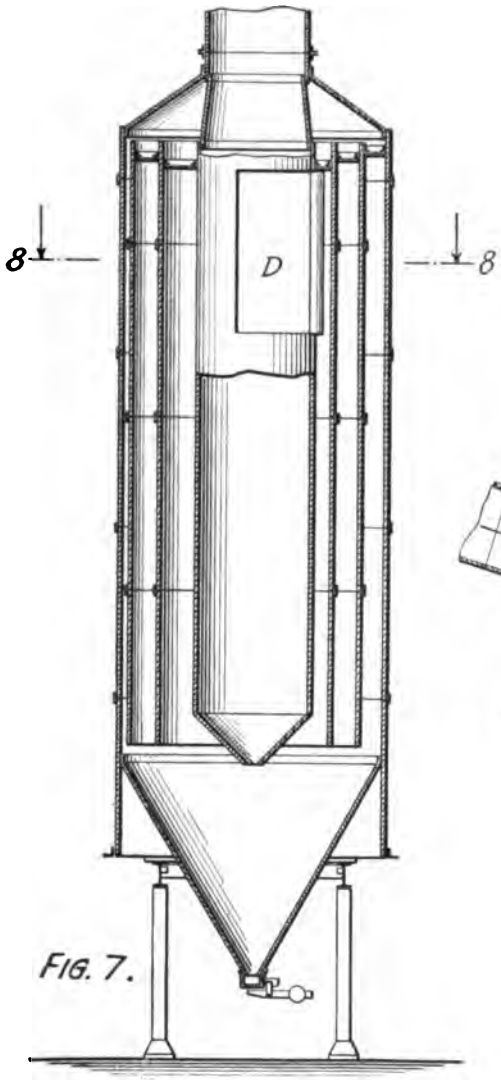


FIG. 6.
Horizontal Section on line 6-6.

FIGS. 5 AND 6.—BRASSERT MODIFICATION OF BRASSERT-WITTING WHIRLER.

Described in general terms, this separator consists of a spiral conduit, the lower open edges of which connect with the dust-collecting chamber. The gas is introduced tangentially and follows a spiral course toward the central axis of the apparatus, the spiral conduit being increased in area before the gas enters the central chamber, prior to its exit from the apparatus.

The dust is separated from the gas by centrifugal force and gravity, and falls through the lower open edges of the spiral into the dust-collecting chamber. The central chamber is provided with a small opening at its lower end, and connects with the innermost spiral of the spiral conduit.



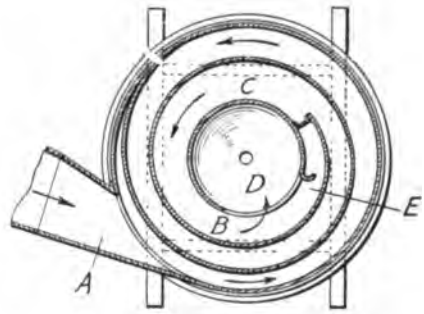


FIG 8.
Horizontal Section on line 8-8.

FIGS. 7 AND 8.—DYBLIE WHIRLER.

In the accompanying sketches, Fig. 7 is a vertical section through the Dyblie whirler and Fig. 8 is a horizontal section. The gas enters tangentially from the gas main through the opening *A* in the shell of

the casing; the gas impinges upon the first turn of the spiral *B* and follows the turns of the spiral. A separation of dust from the gas occurs through centrifugal force, the particles of greatest specific gravity being thrown outwardly and falling by gravity to the bottom of the casing.

At the point *C*, an increased area is provided between the spiral and the central chamber, which causes a decrease in the velocity of the gas, thus allowing a further separation.

The inlet *A* and the outlet *D* are in substantially the same horizontal plane, and this permits the separated material to drop out of the whirling gas and prevents its being caught up in the vortex, which happens when a sudden change in the direction of the flow of the gas occurs.

A deflector, *E*, located at one edge of the opening, is provided. This is in the shape of a hook, which acts to catch any dust which might be carried into the casing, and this completes the separating operation.

Remarks on Efficiency of Dry Cleaning.

It has been demonstrated in practice that dry cleaning by any one of the systems so far referred to, cannot be depended upon by itself to continuously clean the gas from blast furnaces using much fine ore, to the degree desired for use in stoves and under boilers, the amount of dust remaining in the gas ranging from 1 to 3 grains per cubic foot, depending on the working of the blast furnace. Such systems have a value, however, in removing, by simple apparatus and at practically no operating expense, a certain proportion of the dust and so decreasing the duty upon any apparatus installed for further cleaning.

The above remarks on dry cleaning refer in no way to the Halberger-Beth system, recently developed in Germany, which will be treated separately later on in this paper.

Primary Wet Cleaning.

For primary cleaning, in Europe in particular, a separation of the dust by the use of water has been preferred for many years to a dry separation, on account of the very much greater efficiency obtained in cleaning, and on account of the importance of reducing the water vapor contents of the gas to a minimum, thus allowing more efficient combustion. The cooling and washing of the gas are usually performed simultaneously, sufficient water being used to reduce the temperature of the outgoing gases practically to the temperature of

the incoming wash water. Experience has shown that cooling the gas in this manner, to allow condensation and separation of the water vapor, causes less attendant loss of heating efficiency than prevails in heating with vapor-laden gas.

Zschocke System.

Zschocke washers have been used almost entirely in Germany for wet primary cleaning. These consist of cylindrical or square steel towers fitted with a series of wooden grids or hurdles placed at suitable intervals within the apparatus. These hurdles are arranged in such a manner that the water, which is sprayed in at the top of the tower, is broken up into very fine streams, which drip down between the grids and meet the gas coming up, the gas being introduced at the bottom of the tower. The intimate contact so obtained wets down the dust, which is carried with the water to the bottom. These Zschocke towers are usually water-sealed and cone-shaped at the bottom, and the latest type has a siphon arrangement; in either case, the dust is readily removed from the bottom of the apparatus.

Zschocke towers have been found sufficient to cool and clean the gas to the proper degree for use in hot-blast stoves, under boilers, and for similar purposes. A fan washer, into which water is introduced, is frequently used as an auxiliary to the Zschocke towers for primary cleaning, especially when the scrubbing capacity of the towers is small.

A water separator, equipped with internal baffles, is usually located beyond the washer to allow separation of the entrained water.

Zschocke washers are used considerably in the United States, and some additional systems have also been developed here for the wet separation of dust; for instance, the Duquesne spray tower and the Steinbart spray tower. The basic principle of these spray towers consists of the creation of a rain or spray by means of suitably arranged nozzles, and the gas is cleaned and cooled in passing through this spray.

Duquesne Spray Tower.

The Duquesne tower consists of a shell about 80 ft. high by 12 ft. in diameter. As shown in Fig. 9, the tower contains five sets of double screens, the sets being spaced 6 ft. 10 in. apart. Under the first set of screens are distributed seven nozzles, the feed water for which is controlled by a valve outside the tower. Under the fifth set of screens, seven similar nozzles, also controlled by a valve outside the tower, are distributed, just above the range of the lower nozzles.

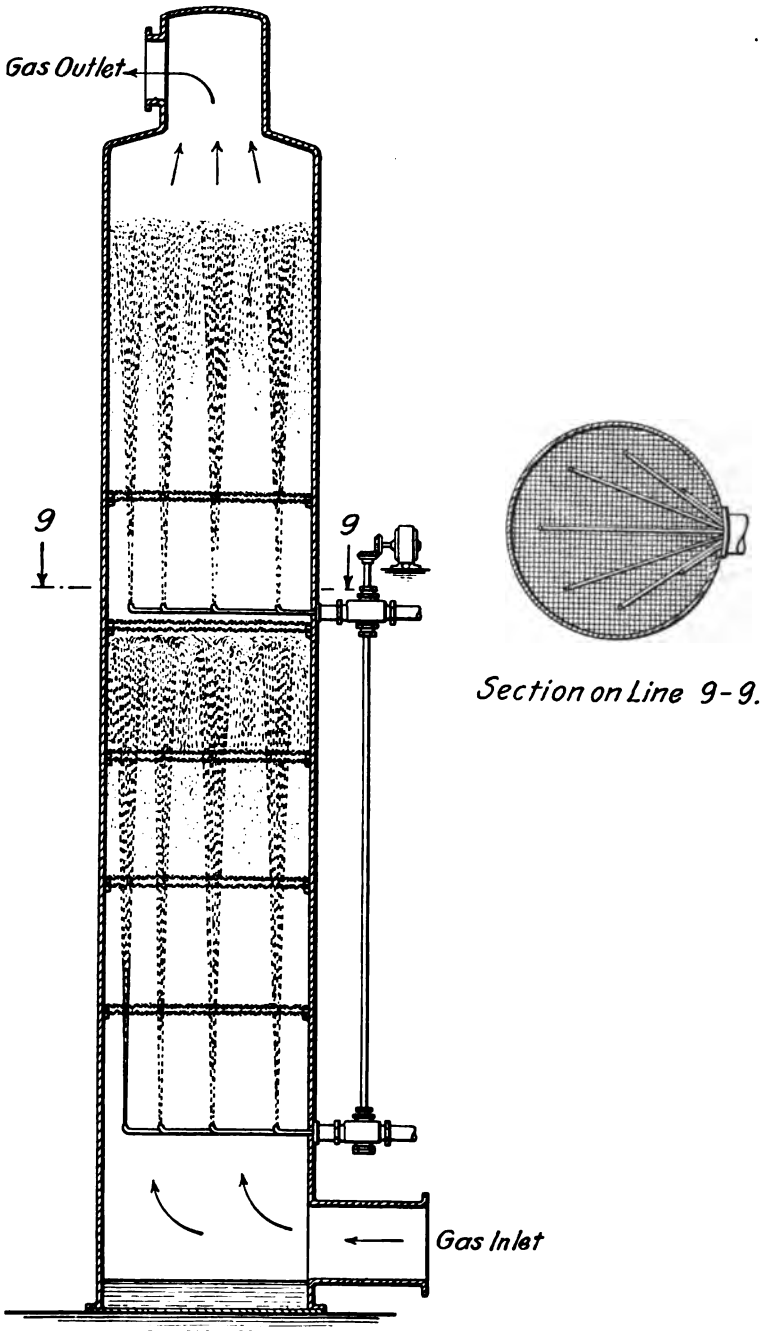


FIG. 9.—DUQUESNE SPRAY TOWER.

The controlling valves have a revolving core which successively blocks off the openings to the different nozzles, thereby temporarily stopping the flow of water and creating an area of low pressure directly above the nozzle. When the core has passed, the flow of water resumes through this nozzle and sprays the gas which has reached this point. The core is revolved electrically, at the rate of about 15 rev. per minute, and a 5-h.p. motor is ample to operate four valves, which are sufficient for two towers.

The screens, which are placed above the nozzles, break up the water into fine drops, permitting intimate contact of the gas and the water.

In the operation of these towers at Duquesne, the gas rises through the scrubber at the rate of 4 ft. per second, and the water at the rate of 60 ft. per second with a head of 35 lb. main pressure. The gas is cooled down very effectively, the temperature of the outgoing gas being only from 5° to 6° F. above the temperature of the incoming water, while the moisture content averages only about 0.5 grain per cubic foot above the saturation point at the temperature of the outgoing gas.

Bian Gas Washer.

The Bian gas washer, as shown in Figs. 10 and 11, consists of a stationary horizontal steel cylinder through which the gas passes from one end to the other. Inside the cylinder, there slowly revolves a shaft which carries a number of vertical disks consisting of wire netting of fine mesh. The diameter of these disks is very slightly less than the inside diameter of the cylinder, and this arrangement necessitates the gas passing through the openings in the screens as it travels through the apparatus. The screens, to the extent of nearly half their diameter, dip into water contained in a trough upon which the open bottom of the cylinder rests, and as the shaft revolves, the part of the screens which has been immersed rises from the water with the meshes covered with thin films of water, thus allowing thorough contact with the gas as it passes through the perforations.

Final Wet Cleaning.

(Some of these systems can also be applied to primary cleaning.)

The amount of cleaning accomplished in Zschocke and similar towers, and in the Bian washer, while satisfactory for stoves and boilers, was found to be not sufficient when the gas was destined for use in gas engines, and the systems of Theisen and Schiele were developed for this purpose.

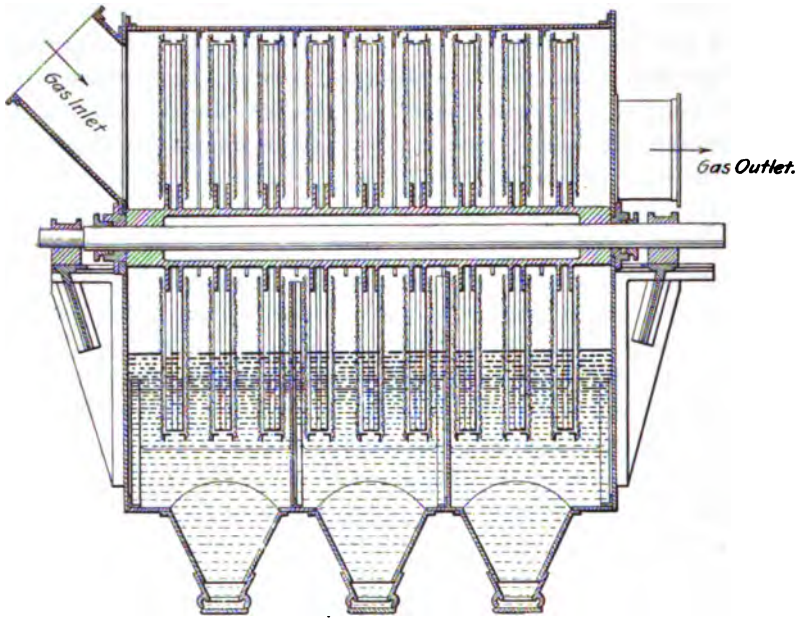


Fig. 10.

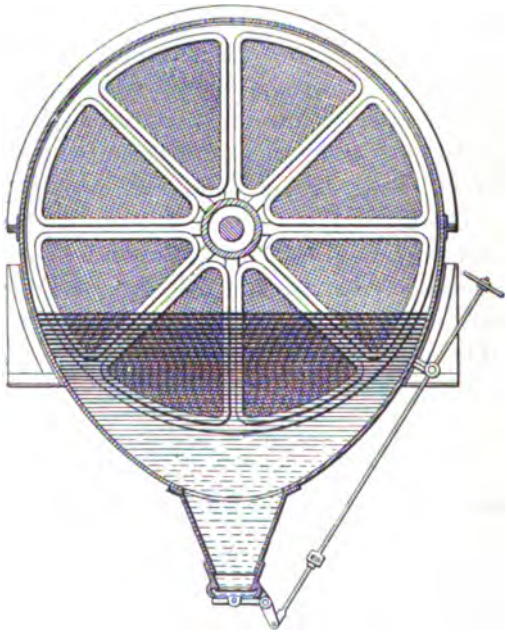


Fig. 11.

FIGS. 10 AND 11.—BIAN GAS WASHER.

Theisen Gas Washer.

The Theisen washer, as shown in Figs. 12 and 13, consists of a casing lined with a special wire netting, within which revolves, at a high speed, a drum carrying numerous fan blades set at oblique angles to the axis of rotation, these blades or vanes being so fitted

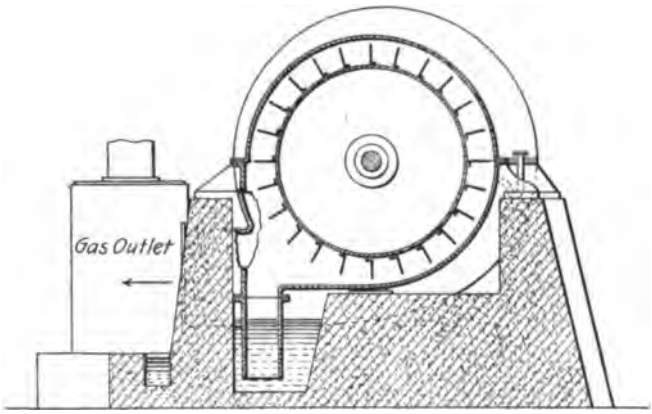


Fig. 12.

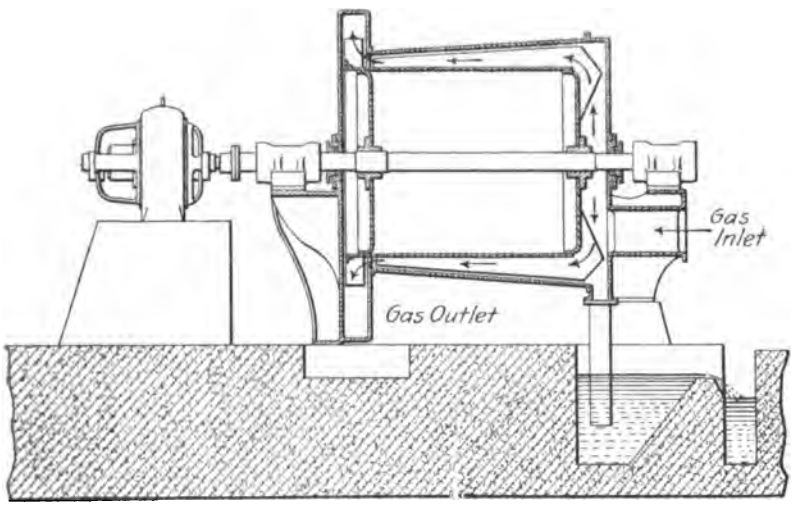


Fig. 13.

FIGS. 12 AND 13.—THEISEN GAS WASHER.

that they form a continuous spiral curve. This allows the gas to be drawn in at one end of the casing and expelled at the other end. Water is admitted at the side of the casing and is converted into a fine spray by the revolutions of the blades, and the spiral arrange-

ment of these blades causes the spray to flow in the opposite direction to the gas, which passes through this spray, being simultaneously cleaned and cooled. The dirty water leaves the apparatus by a water seal at the bottom.

The Theisen and Schiele systems of final wet cleaning have for years given very satisfactory results, but are now being gradually superseded by systems requiring less capital expenditure and less operating expense. Most of these systems can be used for primary cleaning as well as for final cleaning, by installing in two stages. The most important of the wet cleaning systems which perform as efficient cleaning with the consumption of much less power and water than the Theisen and Schiele systems are the disintegrator system of Theisen, the disintegrator system of Schwarz-Bayer, the Fowler & Medley rotary washer, and the Feld rotary washer, while the Halberger-Beth dry cleaning system of filtration through canvas is remarkably efficient in cleaning and is cheap to operate. Following is a detailed description of each of the systems mentioned, together with several other modern systems:

Theisen Disintegrator Gas Washer.

There are two styles of Theisen disintegrator gas washers. One style consists of a casing in which the gas enters by two apertures at the base of the apparatus and is washed by a spray of water in a perforated drum or cage equipped with vanes, the drum revolving within a stationary drum, the gas being drawn through the apparatus by a fan mounted on the same shaft and discharged with the necessary pressure to carry it to the point of consumption. The second style also has the fan mounted on the shaft, but the fan is inclosed within the disintegrator.

The Theisen disintegrator consists of a series of rotary and stationary perforated drums or cages arranged concentrically within one another, as shown in Fig. 14. The stationary drums consist of round bars and the revolving ones of angle bars. The hot raw gas enters the apparatus at the bottom, meets the effluent water and undergoes a preliminary cooling and cleaning in the lower part of the machine. The gas is drawn in counter-current through the series of rotary and stationary drums by means of a fan. The water is converted into a fine spray by the centrifugal action of the rotating drums, and the gas, passing through this spray, is cleaned. The fan is located in the same casing and on the same shaft as the rotary disintegrating drums, the shaft being direct motor driven. Fresh water is introduced into the innermost rotating drums in the form of a finely divided spray.

The cooling and cleaning of the gas and production of the pressure necessary to conduct the clean gas to its point of consumption are all performed in one apparatus and with one motor. It is stated that

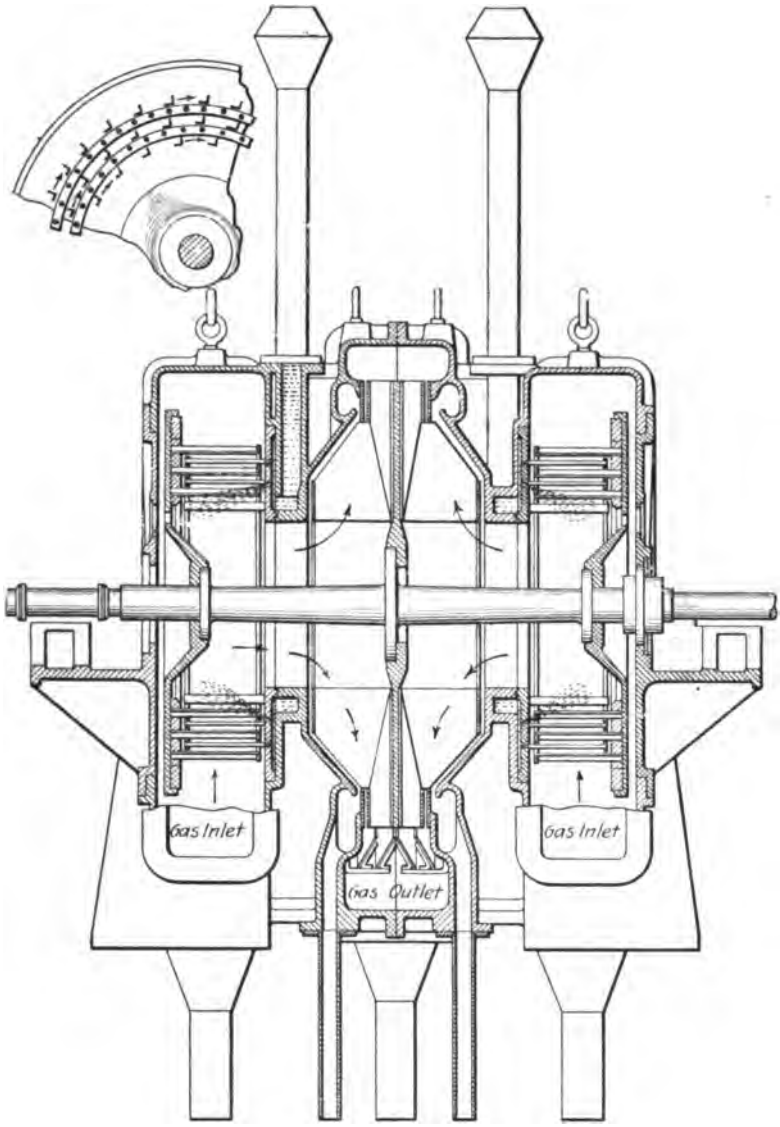


FIG. 14.—THEISEN DISINTEGRATOR GAS WASHER.

this disintegrator is an improvement over the former Theisen apparatus, requiring much less power and water, and performing the necessary cleaning of the gas without preliminary towers.

Schwarz-Bayer Disintegrator Gas Washer.

The Schwarz-Bayer system of gas cleaning makes use of the disintegrator principle, and its general arrangement is simple. The complete set of gas-cleaning apparatus consists of a disintegrator in connection with a saturating chamber in the form of a hood; then a fan placed immediately behind the disintegrator, and finally a water separator. In case both primary and final cleaning are desired, two such sets of apparatus are used, the second of which further cleans the gas which has been primarily cleaned in the first.

The disintegrator, as shown in Figs. 15 and 16, consists substantially of two sets of steel pins, cold-riveted to two steel disks, which disks are set side by side and revolve in opposite directions. The pins of one revolving disk, which interlace with the pins of the other revolving disk, form with the water, through the effect of rotation and dripping, a fine spray or mist, which allows a thorough mixture of the water with the gas traveling among and between the pins before leaving the apparatus.

The gases from the blast furnace pass from the raw gas mains directly into the disintegrators without previously passing through Zschocke towers or similar preliminary washer or cooler. The gas enters through the top of the hood and passes toward the center of the disintegrator, while water is being introduced through the sides to the center. The hood acts to some extent as a pre-cleaner and cooler, as some of the spray from the disintegrators is thrown into the hood and there comes in contact with the hot gas and rapidly evaporates, simultaneously cooling the gas. By the revolution of the disintegrator the water is projected toward the periphery of the apparatus and is broken up into a fine spray; the gas mixes thoroughly with this water and is cooled, and most of the dust contained in the gas is precipitated. The gases pass through the disintegrator in a current counter to that of the water.

The application of the counter-current principle enables the gas to encounter cleaner and colder water in its passage through the disintegrator; hence it is better cleaned, and its temperature is reduced more nearly to the temperature of the entering cooling water. This principle has the effect of materially reducing the amount of water and power consumed. Each disk is direct driven by an individual motor and the speed is determined by the degree of cleanliness desired in the gas. The gas is drawn through the disintegrator by means of a fan located immediately behind the disintegrator apparatus, and passes from the fan to a water separator.

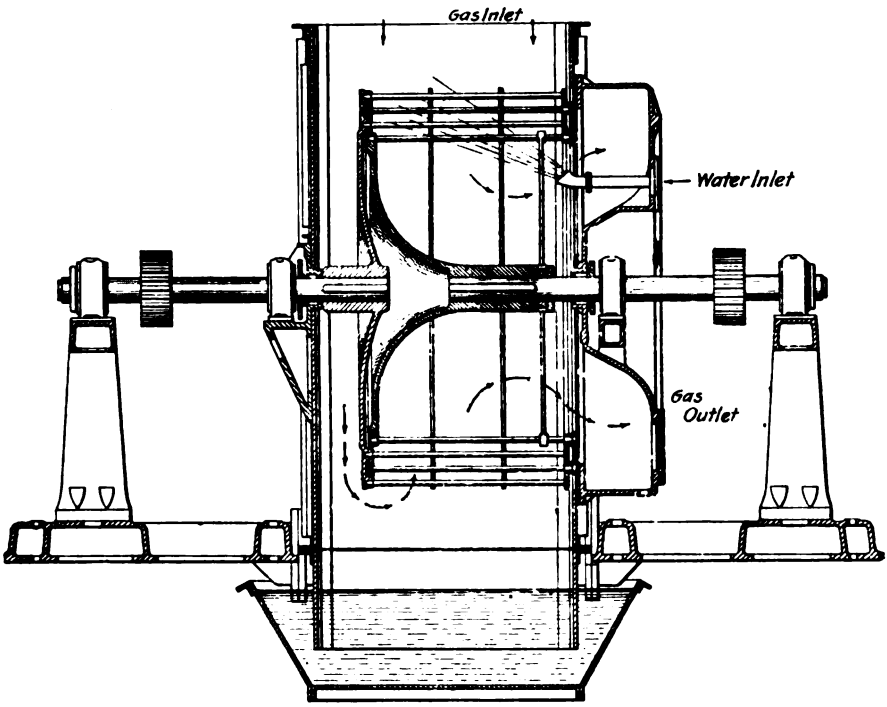


Fig. 15.

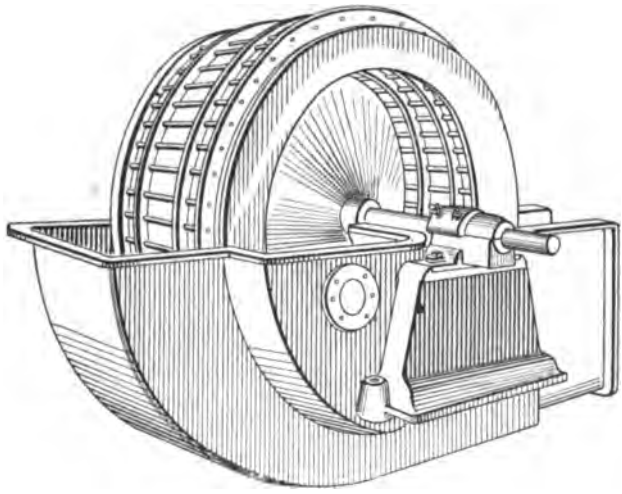


Fig. 16.

FIGS. 15 AND 16.—SCHWARZ-BAYER DISINTEGRATOR GAS WASHER.

The use of pins in this apparatus as a disintegrating medium allows the passage of the gas with very little resistance, and a consequent saving in power. There is also very little possibility of the dust settling on the pins and clogging up the apparatus.

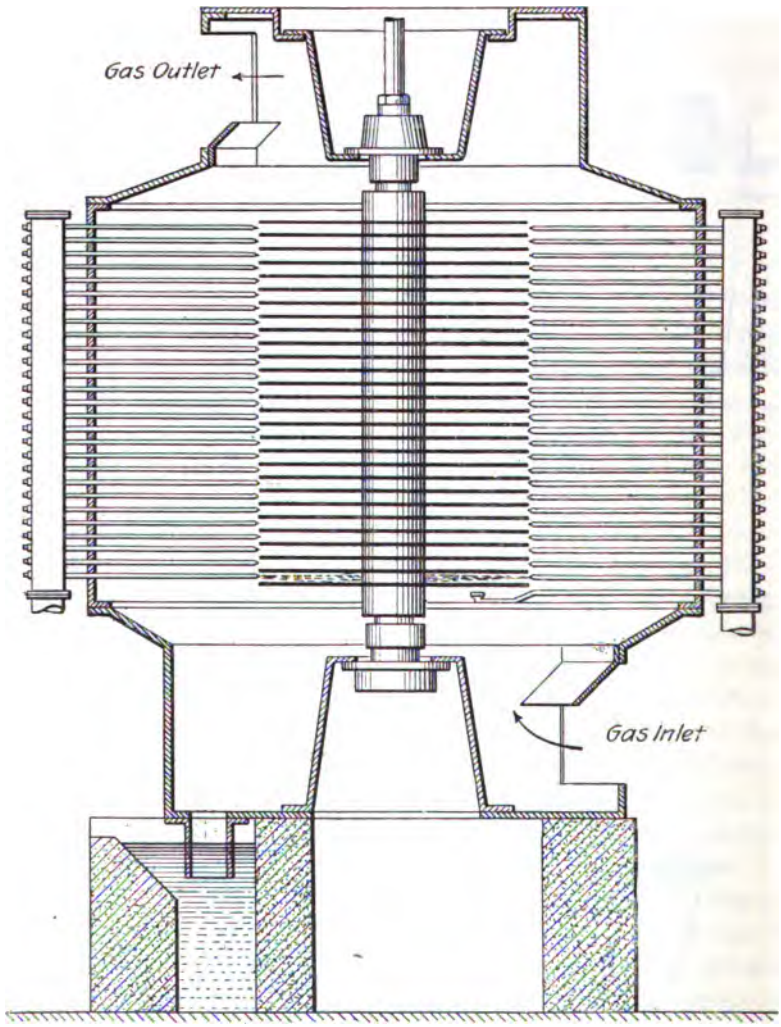


FIG. 17.—FOWLER & MEDLEY VERTICAL GAS WASHER.

Fowler & Medley Vertical Gas Washer.

This apparatus, as shown in Fig. 17, consists of a circular cast-iron casing containing a revolving shaft running vertically through the middle. On this shaft are fixed a number of disks, made either of steel or of cast iron, depending upon whether the water used is alka-

line or acid. Each disk is equipped with a collar separating it from the adjoining disks, and each collar is punched or drilled with six holes, through which six bolts pass vertically, thus holding all the disks in place. The shaft is direct driven with a vertical-spindle motor. Two fixed water sprays are provided for each disk, diametrically opposite each other, one on each side of the washer and projecting between each pair of disks. The jets of water, which are introduced through nozzles having about $\frac{1}{8}$-in. openings, enter with sufficient pressure to strike the collar between the disks, and, as the disks revolve, the water is thrown against the top and bottom of these disks and then against the outside wall of the casing, creating a fine spray or mist in the space between the outer edge of the disk and the wall of the casing, through which space the gas passes. The gas enters the washer at the bottom, passes through this spray or mist, and leaves clean at the top.

This washer can be used for either primary cleaning or final cleaning, or both; in case final cleaning is desired, two washers would be used in series, the first apparatus to clean the gas sufficiently for primary purposes and the second apparatus to finish the cleaning of the gas for gas-engine use.

Feld Gas Washer.

The Feld washer, as shown in Fig. 18, consists of a series of superimposed sections, the bottom of each section being provided with ports for the passage of gas. The gas enters the bottom of the washer and passes from chamber to chamber to the top, whence it is led away. Each section or chamber is provided with a series of cones perforated at the top and mounted upon a cast-iron spider, which is carried on a vertical shaft. The shaft is suspended at the top in a specially designed anti-friction bearing, arranged so as to reduce the power required for operation to a minimum. The water is admitted into the top of the washer and overflows from section to section through the gas ports, the dirty water saturated with dust leaving the bottom of the washer.

When the shaft revolves, the cones do likewise, and the water is raised by centrifugal force along the inner sides of the cones and is atomized at the upper edge. This upper edge of each cone is a little higher than the next outer one, thereby forming a certain number of horizontal sprays of water, depending on the number of cones. The upper portion of the outer cone, which is somewhat higher than the inner one, is perforated. The inner cones supply water to the perforated surface of the outer one. This results in the formation of a

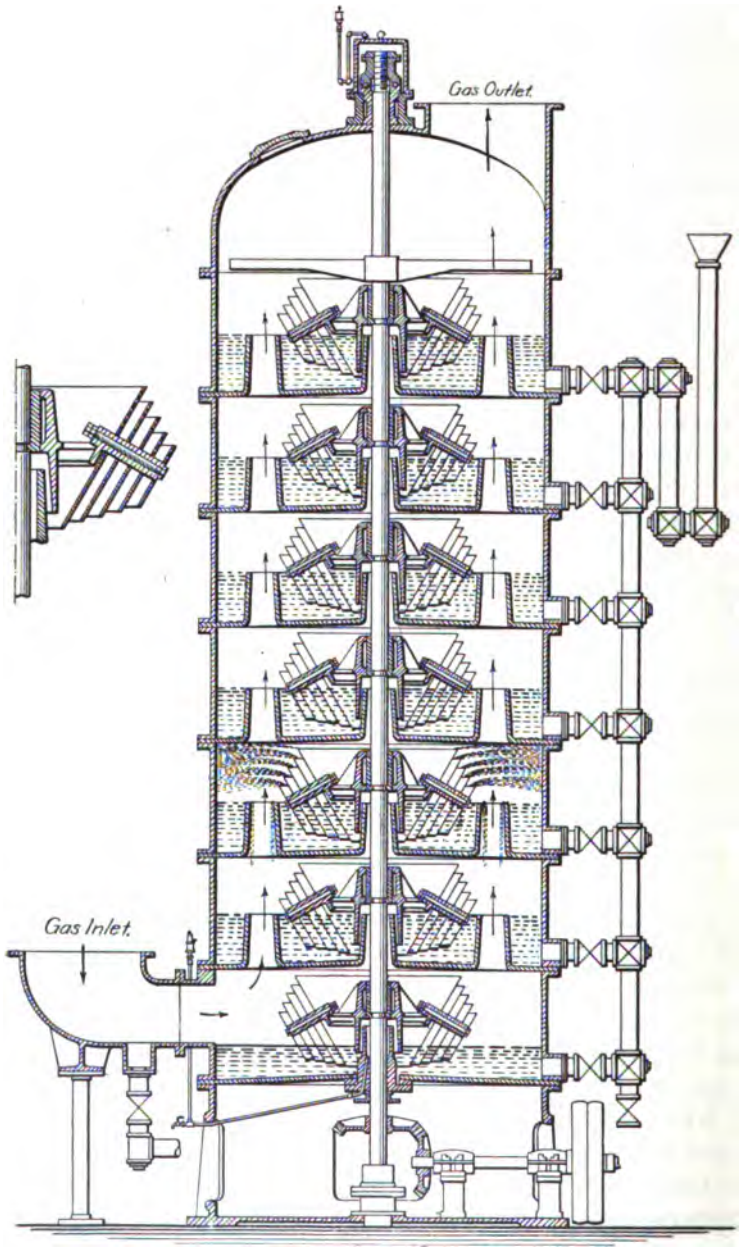


FIG. 18.—FELD GAS WASHER.

series of cascades composed of very small drops of water, through which the gas must pass *en route* through the apparatus.

The washing is accomplished mostly in the lower sections, while the upper sections perform primarily the function of cooling the gas.

For primary washing, the Feld washer is constructed with seven chambers or sections, the lower three being the washing chambers, the fourth one being a separating chamber and the upper three being the cooling chambers. For final washing, in the case of the gas being required for gas-engine purposes, the gas after being primarily cleaned is passed through an additional washer of the same general arrangement.

Reco Centrifugal Gas Washer.

This gas washer is constructed by the Roessing-Ernst Co., of Pittsburg, Pa., and is designed to cool, clean and, if necessary, dry the gas in one apparatus. This washer consists substantially of a vertical outer casing, a tube whose lower end is provided with serrations extending to within a few inches of a water seal, a revolving inverted cup, and a sleeve casing attached to the inverted cup. The outer casing, the cup, the tube, and the sleeve casing are provided with shelves and vanes. The apparatus is belt-driven. The spindle of the rotor on which the driving pulley is fastened is hollow, and the weight of the rotor is taken up by the shaft inside of this sleeve, held by a ball bearing which is backed by a rubber buffer in order to equalize any irregularity during rotation.

As shown in Fig. 19, the hot gas enters the apparatus at the point *A*, passes over the water, a certain amount of which the gas takes up by evaporation, and then passes into the tube *B* through the serrations at its base. During its passage through this tube the gas and water vapor are subjected to a thorough beating and mixing by the action of the vanes *C* of the revolving sleeve casing *D*, fastened to the top of the inverted cup *E*. The gas passes into this inverted cup, which is rotated by the driving sleeve *F* and the pulley *G*, and then flows downward, around and under the lower edge of the cup and then upward between the cup and the outer casing *H*. The outer surface of the revolving cup is provided with concentric shelves *K* and the outer casing *H* is provided with downwardly inclined shelves *I*, which receive the washing water from the water-sealed stuffing box *J* and through a series of water pipes *L*. The water, falling on the rapidly rotating shelves of the cup, is thrown by centrifugal force against the inner walls of the casing and thence flows downwardly along the inclined shelves, dripping on to the next rotating shelf, and so on. In this way the gas, while subjected to a thorough whirling

and beating action, has to pass upward through several films of finely divided water or spray while the water passes downward, carrying with it the separated impurities.

The apparatus operates on the counter-current principle, the cleanest gas passing from the apparatus meeting the cleanest water entering the apparatus.

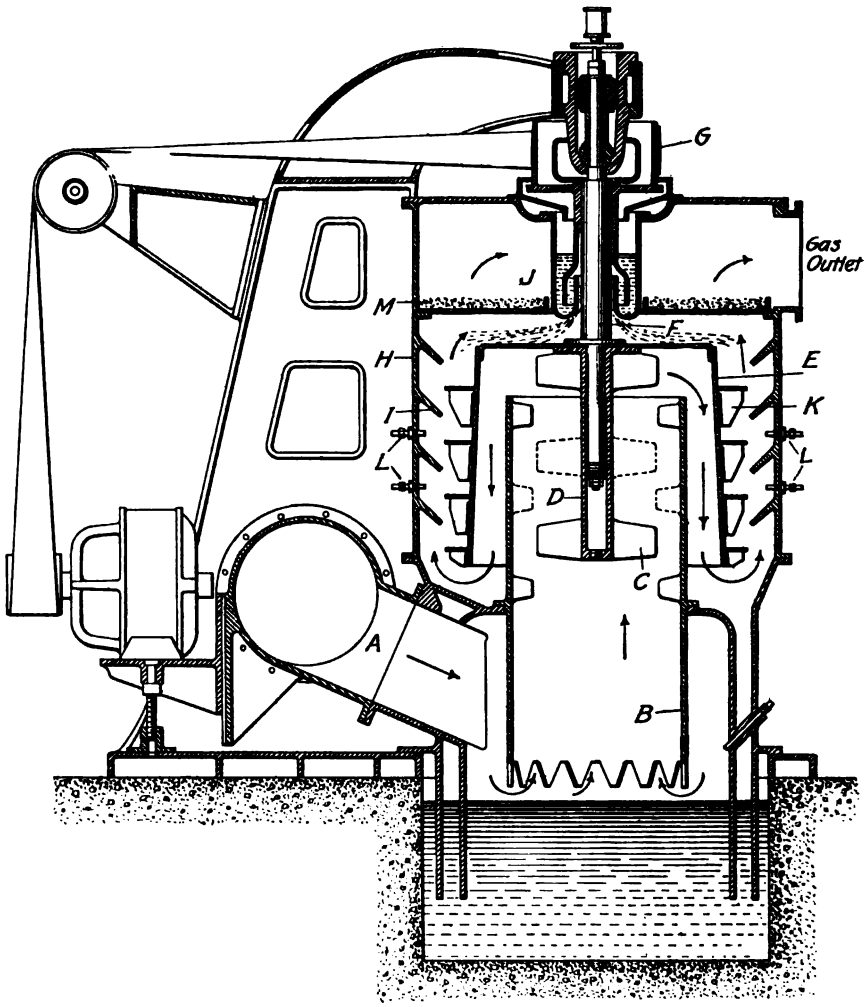


FIG. 19.—RECO CENTRIFUGAL GAS WASHER.

The upper part of the casing is provided with a rack which, it is stated, can be packed with suitable drying material in case it is desired to dry the gas before leaving the apparatus.

Sepulchre Gas Washer.

This system is designed as a final washer to further clean primarily cleaned and cooled gas to the degree necessary for use in gas engines. The principle of this system consists in creating in a vertical tower a very fine spray or mist of water by means of an injector of the Körting type, in which water under pressure is atomized by means of compressed blast-furnace gas, the spray being produced by the expansion of the compressed gas. An intimate mixture of the spray so

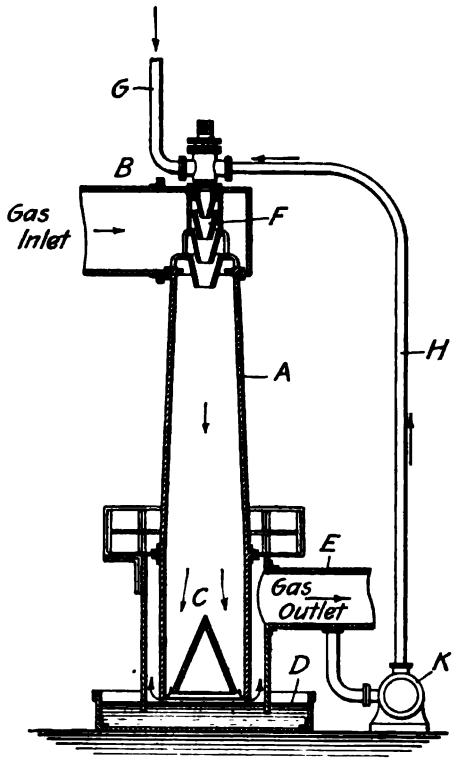


FIG. 20.—SEPULCHRE GAS WASHER.

formed, with the dirty gas entering the apparatus, is obtained by the arrangement of the apparatus.

A separator is provided in connection with this apparatus, which consists substantially of a cone arranged in the lower part of the tower in such a way as to leave between the base of the cone and the walls of the tower a very narrow passage, through which the gases are forced over the surface of a water seal, where the dust and water vapor are deposited.

In the accompanying drawing, Fig. 20, *A* is the vertical tower, the lower end of which terminates a short distance above the surface of the water seal *D*. Within the lower end of the tower is arranged a conical deflector, *C*, and near the top of the tower is the gas inlet, *B*. The lower section of the tower *A* is surrounded by a casing which is open at the bottom and extends beneath the surface of the water in the seal. A gas outlet, *E*, is provided in connection with the outer casing. The Körting injector is located at *F* and the feed water for same is supplied through the pipe *G*. The pressure is supplied by withdrawing a portion of the purified gas from the outlet pipe *E* and forcing this by the compressor *K* through the pipe *H* into the injector simultaneously with a stream of water.

Final Dry Cleaning.

(Some of these systems can also be applied to primary cleaning.)

Halberger-Beth Gas-Cleaning System..

The principle of the Halberger-Beth system, shown in Fig. 21, is based primarily on filtering the gas through canvas bags. The gas coming from the blast furnace passes through the usual dust catchers and gas mains to a cooling tower, where the temperature of the gas is reduced to about 175° F. The cooling tower is arranged so that the necessary amount of cooling can be accomplished either by air or by direct contact with water, depending on the temperature of the gas entering the cooler, which temperature is naturally variable, in accordance with blast-furnace conditions.

From the cooler, the raw gas, by means of the suction of a fan placed beyond the filters, or without a fan when the pressure of the gas issuing from the furnace is sufficient, passes into and through the canvas filtering bags, depositing its impurities on the surface of the bags. These canvas filters are contained in a series of double compartments, each usually holding 12 canvas bags in rows of three by four. Each bag is about 8 in. in diameter by 9 ft. 9 in. long, and is equipped with a ring at each 18 in. of its length to prevent entire collapse of the bag when cleaning. The bags are fastened into a stationary header at the bottom, this bottom end being open, while the top is closed by a steel plate. Each bag is connected with a shaking mechanism located outside and above the filter compartment, and at regular intervals, usually about every 4 min., these bags are automatically shaken, a compartment at a time, for a period of from 15 to 20 sec. By means of a butterfly valve, the uncleaned gas is shut off from the compartment while the shaking is in progress, and

cleaned gas, superheated to the proper temperature of about 175° F., is forced under pressure into the compartment. This causes a partial collapse of the canvas bags, which, in conjunction with the simultaneous shaking, allows the dust to fall from the canvas. The separated dust drops into a hopper beneath the sacks, whence it is transferred by means of a spiral conveyor to a bin, from which it is loaded into cars. At the end of the cleaning period the butterfly valve automatically returns to its original position and filtering commences again.

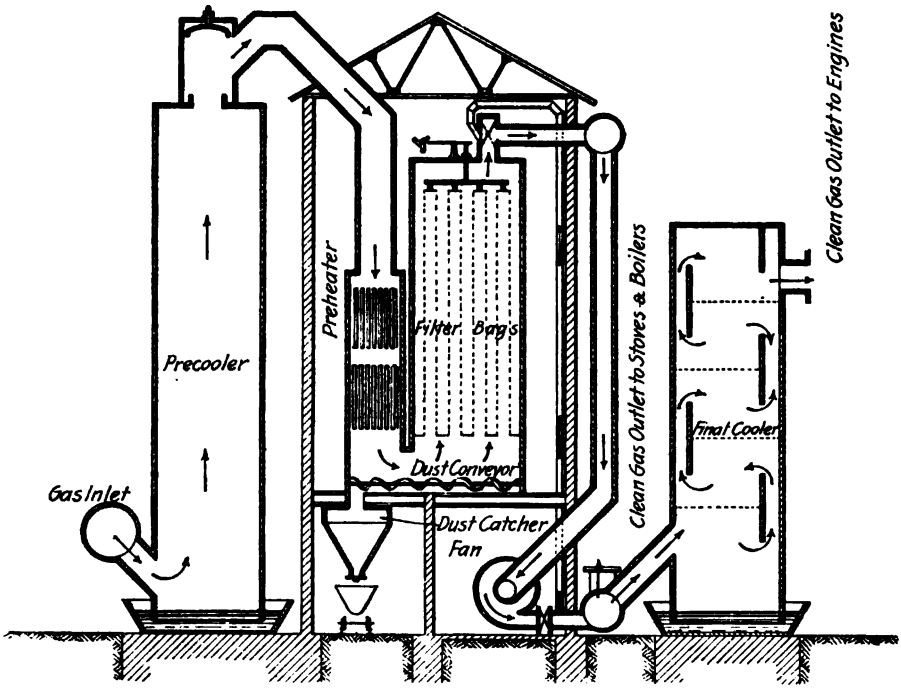


FIG. 21.—HALBERGER-BETH GAS-CLEANING SYSTEM.

It is quite necessary to keep the temperature of the gas at about 175° F., as if much higher than this there is danger of scorching the bags, while if lower the water vapor in the gas is deposited on the canvas and prevents proper filtration. In case the gas becomes cooled below 175° F. in the cooling tower, it is superheated by means of steam or by waste heat from the hot-blast stoves to about this temperature before entering the filtering bags. After leaving the canvas bags, the gas requires no further cleaning for gas engines and is cooled down to the proper temperature in cooling towers of various designs.

The degree of cleanliness of the gas is indicated by the clearness of the effluent water from these towers and no settling basins are required. Consequently, this water can be used over and over again, which is a material item in districts where water is scarce. A further advantage lies in the non-pollution of streams, the laws relating to which are very strict in certain districts.

This system utilizes the basic principle employed in the "bag house" system, which has been used for the last 20 years in connection with recovering zinc dust from the gas issuing from zinc oxide furnaces and collecting dust from lead smelters.

Smith-Bagley Gas-Cleaning System.

A modification of the Halberger-Beth system has been recently devised in England, but has not yet been demonstrated on a practical scale. This consists substantially in replacing the stationary canvas bags used in the Halberger-Beth process by traveling belts composed of canvas or similar textile material.

In this process, it is proposed to cool the gases, after leaving the dust catchers, to the prescribed temperature, by means of a tubular cooler so arranged that fluctuations in temperature can be automatically equalized by an electrical device controlling butterfly valves in the base of certain sections of the gas tubes within the cooler, which will increase or decrease the amount of cooling surface. When duplicate coolers are installed, the electrical governor automatically connects the second cooler with the first in case of any rise in temperature.

After leaving the coolers, the gas enters an apparatus containing a series of belts made of a special textile fabric. These intersect the casing of the apparatus at right angles to the direction of the flow of the gas. The area of these belts is determined by the desired speed or flow of the gas and their texture and composition. By using special fabric for the belts, a certain degree of flexibility is attained by stretching the fabric or releasing the tension in any desired section. This is accomplished by rollers placed before the main operating rolls in the direction of the movement of the belts. The gas passes through the belts and the dust particles are deposited on the surfaces nearest the incoming gases. The belts move continuously and enter and leave the casing of the apparatus through gas-tight rolls suitably covered. The belt fabric and the two roll surfaces make a gas-tight joint.

On leaving the casing, the dust-laden belt enters the magnetic field of a separator, which withdraws any metallic and magnetic dust par-

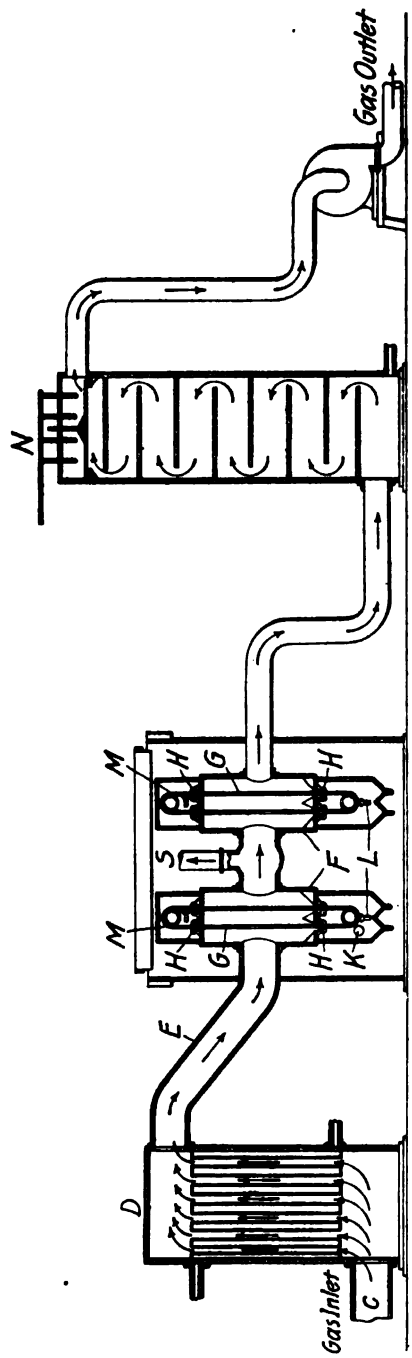


FIG. 22.—SMITH-BAGLEY GAS-CLEANING SYSTEM.

ticles and removes these to a separate bin. The belt then moves forward to vacuum collectors, which remove the dust. Several of these vacuum collectors may be placed in series to operate on the belt so that when the belt re-enters the casing it is thoroughly clean and free from dust. It is then in condition to again be used for removing dust.

The back pressure exerted by the belts can be governed within certain limits by their speed. The bottom surfaces of the casing of

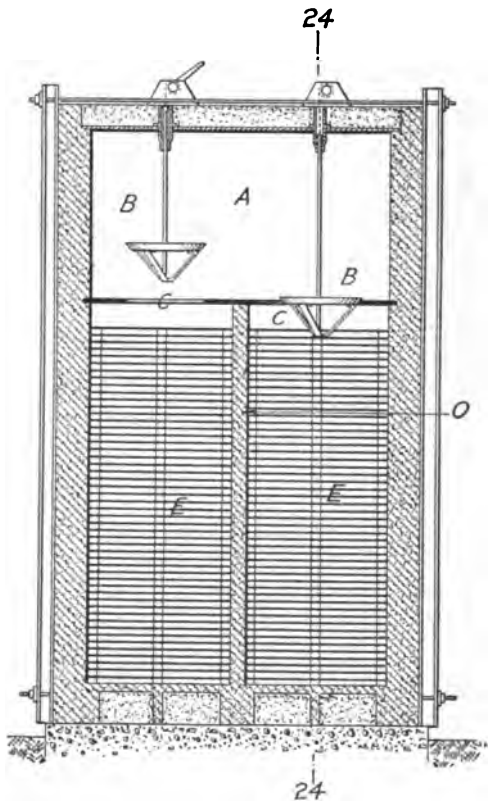


FIG. 23.—HOWARD DUST SEPARATOR. SECTION ON LINE 23–23 OF FIG. 24.

the apparatus are inclined to prevent any dust from accumulating, or pockets can be provided for a similar purpose.

After leaving this apparatus, the gas is cooled down to the desired temperature for use in gas engines by direct coolers.

As shown in the accompanying drawing, Fig. 22, the dirty gases from the dust catcher enter the tubular cooler *D* through the gas main *C* and pass by the gas main *E* to the dry cleaners *F*. The canvas filtering belts are indicated by *G* and the gas-tight rolls by *H*.

K signifies the magnetic separator, *L* the vacuum collectors, *M* the drive rolls, *N* the final cooler, and *S* an outlet to stoves and boilers for partly-cleaned gas.

As before stated, this process has not, to the writer's knowledge, been tried out on a practical scale, but presents probabilities of success.

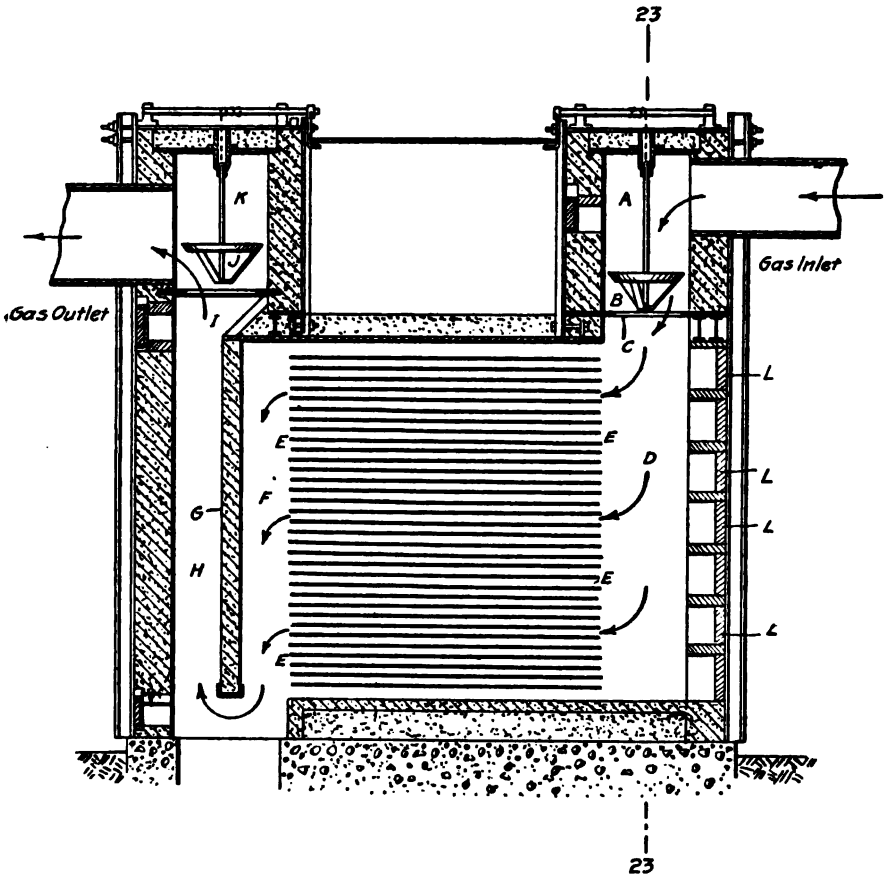


FIG. 24.—HOWARD DUST SEPARATOR. SECTION ON LINE 24–24 OF FIG. 23.

Howard Dust Separator.

An apparatus which up to this time has not been applied to the separation of dust from blast-furnace gases, but which indicates fairly good possibilities in this direction, is the Howard dust separator. This apparatus is used extensively in chemical manufacturing industries for separating fine dust from such gases as those produced from burning pyrites fines in the manufacture of sulphuric acid.

The principle of this process depends upon the deposition of dust on a series of steel plates or shelves confined in a chamber of either brick or steel. These steel plates are usually No. 10 gauge sheets and are placed horizontally one above another about 2.5 in. apart. The gas enters the chamber containing these plates, the chamber being arranged in such a way that the gas is evenly distributed throughout the vertical height of the chamber by means of a baffle wall. The gas to be cleaned must pass over and between these shelves before leaving the chamber, and during its passage the dust is deposited upon the shelves, the deposition being due to the velocity of the gas being checked in passing between the steel plates by the large area of cross-section. As these plates are only 2.5 in. apart, the dust particles need to fall only this distance before finding a surface upon which to rest.

The dust first builds up at the entrance end of the shelves, reducing the space through which the gas flows, and gradually similarly builds up for the length of the shelves. Care must be taken to remove the accumulated dust before too much has gathered, as otherwise inefficient cleaning results. Cleaning holes with covers are provided to allow removal of the accumulated dust, and this is accomplished by means of a flat steel scraper, which is pushed by hand between the plates, thus driving the dust out ahead of the scraper.

In case a steel chamber is installed, as would be the case if this system were applied to blast-furnace gas, a special design has been made which allows ready access to the shelves by means of a sliding section.

In the accompanying Figs. 23 and 24, *A* is a brick entrance header or flue which the gas enters from the furnaces. *B* is a cast-iron damper capable of being closed or opened by a chain and pulley operated outside. *C* is a cast-iron plate with a damper opening over which the damper *B* closes. *D* is a vestibule which gives the gas free entrance to all the plates or shelves. *E* indicates horizontal steel plates placed 2.5 in. apart. *F* is an open space which permits free exit of gases from between all the plates. *G* is a baffle wall placed to equalize the flow of gas between all the plates. *H* is the exit vestibule. *I*, *J*, and *K* indicate the damper opening, damper, and discharge header. *L* indicates the cleaning holes with covers. The course of the gas is indicated by the arrow lines.

Fig. 23 shows a two-compartment separator, in which the dampers provided for each compartment allow one compartment to be cleaned while the other is in operation, *O* representing the division wall.

Separation of Dust by Electrical Precipitation.

Another method which possibly may be applied to the separation of dust from blast-furnace gas is the electrical precipitation of suspended particles. This system has not so far been applied to any great extent to the purification of blast-furnace gas, but is claimed to have been successful in other directions in removing suspended matter from gas and from air. An example of this is the separation of dust in cement plants. In this system, the dust is deposited upon rods or wires electrically charged by high tension direct current, which is intermittently stopped to allow the collected dust to fall from the collecting rods or wires into a hopper below.

The Kapnograph.

This instrument, shown in Fig. 25, continuously indicates the relative degree of cleanliness of the blast-furnace gas going to the gas engines, and is extensively used in European gas-engine stations. Gas from the cleaned-gas main passes through this apparatus and impinges upon a continuous recording chart, upon which the dust in the gas is deposited. The variations in the amount of dust in the gas are indicated by lighter or darker shades on the recording paper, depending on the amount of dust deposited. The flow of gas to the instrument is maintained either by the natural pressure of the gas, or, if this is not sufficient, by an aspirator behind the outlet pipe. The speed of the gas to the nozzle is kept constant by means of a regulator, as shown in sketch, the excess gas over the required amount escaping into the outlet pipe by passing under a partition and through a seal of water.

Methods of Determining the Amount of Dust in Blast-Furnace Gas.

A method employed with good results in Europe for determining the amount of dust in the gas consists in drawing a definite quantity of the blast-furnace gas to be tested through a filter, which is weighed in a dry condition before and after the test. The apparatus for determining the amount of dust consists of a glass tube drawn out at one end and fitted at the other with a ground-glass cover, which is also drawn out to a thin tube. This cover facilitates the placing of the filtering material in the tube, and during the test the cover is fastened to the tube by means of wire. Before the test, the glass tube, filled with suitable filtering material, is placed in a drying furnace and heated at a temperature of 105° C. until its weight is

constant, which usually requires from one to two hours. The drying furnace is arranged so that several tubes can be dried simultaneously.

During the drying process air is drawn through the tubes after having previously been thoroughly dried by passing through bottles

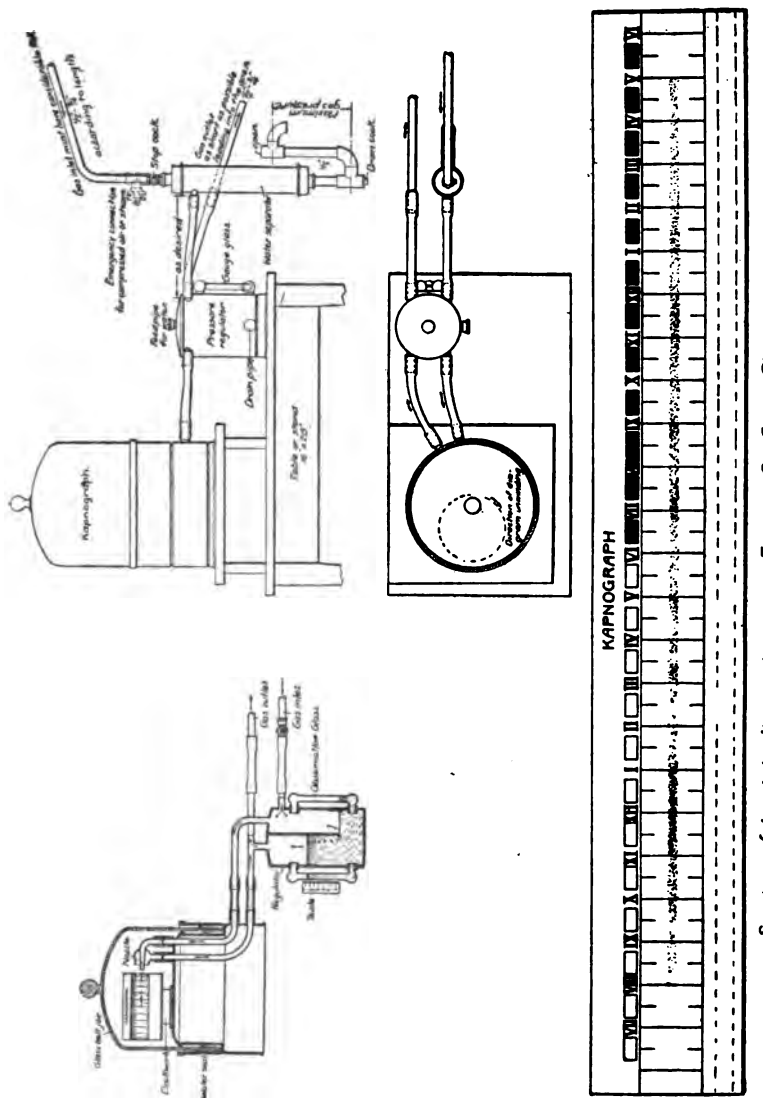


FIG. 25.—THE KAPNOGRAPH.

containing calcium chloride and concentrated sulphuric acid. During the drying process the tubes are weighed until no further increase in weight is observed.

In making the test, the weighed tube containing its filtering ma-

terial is inserted into the gas main, a rubber stopper keeping the test-hole tight. The upper end of the tube is connected with a gas meter, which in turn is connected with a barrel filled with water. The water is allowed to flow out of the barrel and in so doing creates the necessary suction to draw the gas through the filtering tube and through the gas meter. When the necessary amount of gas has been withdrawn the tube is again dried and weighed. The increase in weight indicates the amount of dust in the quantity of gas tested.

Brown Dust, Moisture, and Volume Determinator for Blast-Furnace and other Gases.

This apparatus has been devised in order to accurately determine the amount of dust and moisture contained in blast-furnace gas, as well as the volume of the gas, and is used with considerable success.

Referring to the accompanying drawing, Fig. 26, *A* is a gas main conveying the gas to be tested. *B* is an aperture in the small pipe through which samples of the gas are drawn. *C* is a filtering medium within which the solid constituents of the gas are deposited. *D* is a conduit leading to the exterior of the gas main through which the filtered gas is conducted. *E* represents a flexible connection to a surface condenser, *F*. *G* represents a receptable for some chemical, such as calcium chloride, which can be used for the purpose of taking out the moisture contained in the sample. *H* is a conduit from this moisture-removing receptacle to the rotary air pump, *I*, or through the by-pass *J* to the three-way valve *K* and thence to the gas meter *L*, where the volume of the sample is determined, together with its temperature and pressure; these latter by means of the thermometer *M* and the U-tube *N*, respectively. An electric motor, *O*, is used to operate the pump *I* through the variable-speed drive *P*.

An indication of the velocity of gas or gases in conduit *A* is transmitted through aperture *Q* in the sample pipe and conduit *R* to horizontal pressure gauge *S*; also an indication of the velocity of gas or gases after passing aperture *B* is transmitted from aperture *T* through conduit *U* to horizontal pressure gauge *S*. It is evident that changes of the velocity of the gas or gases in aperture *B* of sample pipe, produced by the suction of pump *I*, or by pressure in gas main *A*, are indicated, and can be accurately controlled and made equal to the velocity of the gas or gases in conduit *A*, the gas main, such indicator being the oil piston shown in glass tube forming a part of the velocity gauge *S*.

The method of operating this apparatus is as follows: The dry weights of the filtering medium *C*, of the receptacle *G*, containing

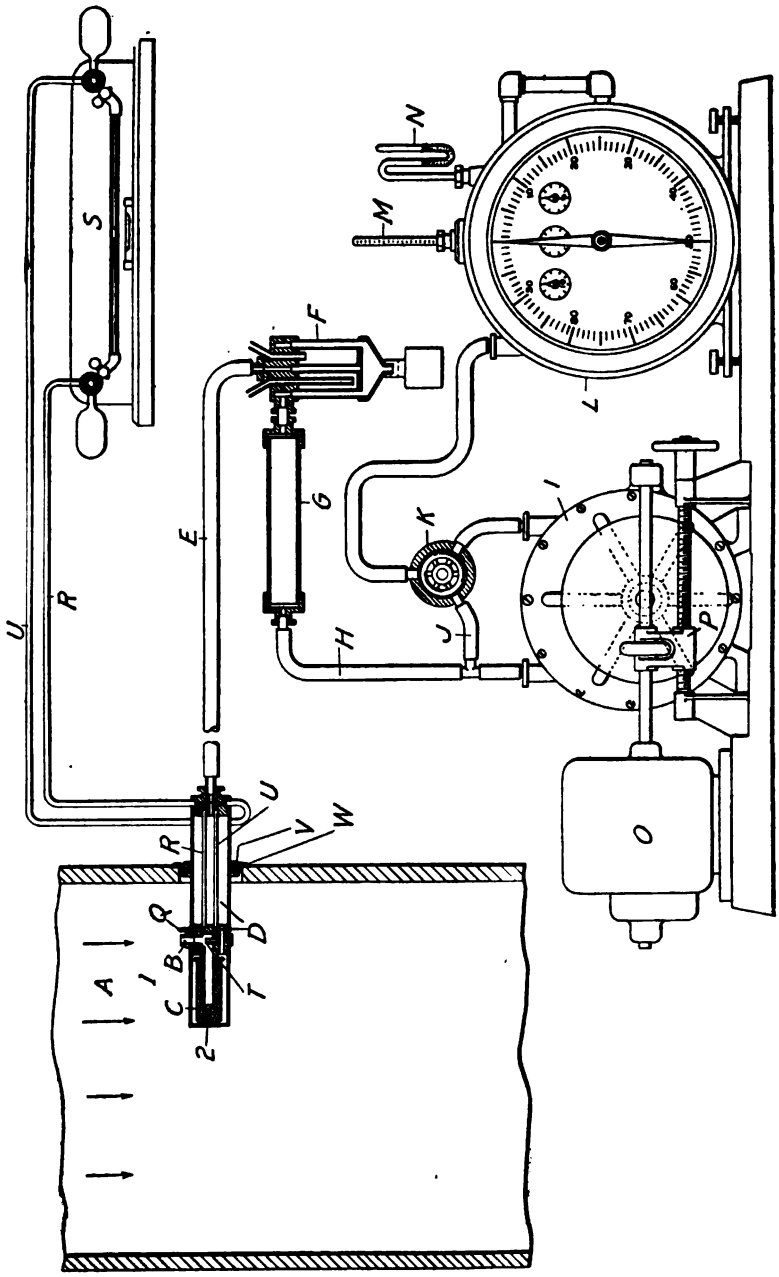


FIG. 26.—THE BROWN DUST, MOISTURE, AND VOLUME DETERMINATOR.

the calcium chloride, and of the measuring flask attached to surface condenser *F*, are very carefully determined. They are then inserted in the apparatus, and the sample pipe is then inserted in the gas main, a tight connection being made between flange *W* and bushing *V*. The meter reading is noted. At the same time that the sample pipe is inserted in the gas main *A*, the time is noted, and the rotary pump *I* started. The speed is then so regulated that the oil piston in the horizontal pressure gauge *S* is maintained in equilibrium. This indicates that the velocity in aperture *B* is exactly equal to the velocity in gas main *A*, this condition having been determined by a measured amount of gas in gas main *A*, and the proper proportioning of aperture and conduits in the sample pipe during the calibration tests. This condition is maintained for an indefinite length of time and the sample pipe is then withdrawn from gas main *A*. The meter reading, multiplied by the ratio of area of aperture *B* to area of gas main *A*, gives the total amount of gas passing through gas main *A* in the elapsed time. The difference between the dry weight of the filtering medium *C*, before and after the test, divided by the number of cubic units shown by the meter, gives the weight of dust per cubic unit. The moisture per cubic unit of gas is found in a similar manner from the sum of the weights of the water in drying receptacle *G*, the water caught in the measuring flask attached to surface condenser *F*, and the weight of water retained in the filtering medium *C*.

The Advantages of Clean Gas.

The use of properly cleaned gas in hot-blast stoves allows their operation for a year or more without having to take the stoves off for cleaning, while the same stoves would require cleaning at least every two or three months when using dirty gas. Besides the saving in labor and repairs, greater efficiency is obtained from the stoves, due to more complete combustion of the gas and consequently a lower temperature of the escaping products of combustion. This results in a smaller amount of gas being required for combustion and thus renders available for other purposes the amount of gas so saved.

The stoves are also more efficient, due to the greater readiness with which a clean brick surface transmits its heat to the air for blast than a brick surface covered with dust.

Condensation and elimination of water vapor from the gas, incident to the wet cleaning process, is another item in the direction of more perfect combustion, which in the presence of water vapor is retarded; also, the amount of heat carried into the stack flues is increased in proportion to the amount of water vapor present.

Smaller checker openings can be employed in stoves when using clean gas, thus allowing a material increase in the amount of heating surface of a hot-blast stove. For instance, in Germany, where practically all hot-blast stoves are heated with clean gas, the checker openings are almost universally smaller than in the United States, where most of the stoves are heated with dirty gas. However, the United States is following Germany in this direction of smaller checker openings, particularly at those plants where clean gas is used.

In boiler practice, similar benefits obtain from the use of clean gas, the settings and tubes requiring cleaning at less frequent intervals, with a resultant greater efficiency.

The strains in stove and boiler installations due to contraction and expansion, caused by frequent stops for cleaning when operating on dirty gas, are largely avoided when operating with clean gas on account of the more continuous operation.

In conclusion, it is my opinion that the proper cleaning of blast-furnace gas for use in hot-blast stoves and under steam boilers is very necessary from an economical standpoint, in all cases where the character of the ores and the operation of the blast furnace cause much dust to be blown over from the furnace. For gas engines, the use of thoroughly cleaned gas is imperative in all cases.

The two primary objects of this paper have been:

1. To state the advantages of cleaning blast-furnace gas where dusty gases are produced.

2. To describe the various systems by which the required cleaning can be accomplished.

I have purposely refrained from expressing an opinion as to the relative merits and costs of the various systems described, preferring that prospective users should themselves determine these questions.

Discussion.

Samuel K. Varnes,* Steelton, Pa.:—We have a gas-cleaning plant consisting of a centrifugal dry dust catcher, and two rain-type wet scrubbing towers, 25 by 60 ft. each, for rough cleaning of blast-furnace gas. This plant was put in early in 1910, with the intention of cleaning gas for use in hot-blast stoves and under boilers, and was so used for several months. Soon after it went into service, the blast-furnace men complained that they could not get as high blast temperatures as they had obtained when using dirty gas. It was decided to run a series of careful tests to determine if this were true and to

* Non-member.

what degree the apparent results might be affected by other conditions. About 60 tests were run, covering a period of five months, and at the end we came to the conclusion that it did not pay us to scrub the gas for stoves and boilers. The tests were all run upon a single stove, no boiler tests being made, but the conclusions of the stove tests being applied to the boilers.

The furnace burden was practically the same for all the tests and averaged:

	Per Cent.
Daquari and other hard ores	57
Nodules	13
Raw Mayari ore	25
Mill cinder and scale	5

The raw Mayari is exceedingly fine, and produces even more flue dirt than does the fine Mesabi ore. The average results of these tests are here shown:

	Wet Scrubbed or Clean Gas.	Dry Cleaned or Dirty Gas.
Grains of dust per cubic foot	0.3	2 to 3
Moisture in gas, pounds of water per pound of dry gas	0.035	0.05
Temperature of gas at burner	100° F.	300° F.
Temperature of stack gas	450° F.	500° F.
Temperature increase of blast in passing through stove	925° F.	1,050° F.

The moisture content of the clean gas is seen to be only 70 per cent. of that of the dirty gas, hence the gas was partly dried by wet cleaning. By "temperature increase of blast in passing through stove" is meant the difference between the temperature of the blast on leaving and on entering the stove. The dirty gas shows 125° higher blast temperature. The maximum increase of temperature in case of the clean gas was 962° F., or less than the average of the dirty gas.

We found further that the efficiency of the stove and burner as a regenerator was higher by about 1 per cent., using dirty gas. The values of these efficiencies are not absolute but relative only, having been determined by the heat-balance method after calculating radiation and convection losses from the shell. Having determined that we got higher efficiencies and higher blast temperatures with dirty gas, and being able to keep a stove in service during a furnace run of three years without stopping to clean it, we could see no reason for continuing to wash gas for stoves, and the practice was accordingly abandoned early in 1911. The scrubbing plant is now used for preliminary washing for the Theisen final washer on gas-engine service.

A. N. DIEHL, Duquesne, Pa.:—We have listened with great pleasure to Mr. Forbes's interesting paper, which has outlined the most prominent methods so far developed in blast-furnace gas cleaning. As he has plainly outlined, there are the two distinct systems, wet and dry cleaning, and also what are termed primary and secondary in the wet method. The primary includes scrubbing or such cleaning as is necessary to fit the gas for stove and boiler use; the secondary, that which further wet cleans the gas sufficiently for engine purposes. In the dry system, the gas may be cleaned by centrifugal action and by filtration. The quantity of gas issuing from a blast furnace is about 150,000 cu. ft. per ton of iron, and varies, with the coke consumption, in combustibles, therefore in thermal capacity. The average temperature of the gas is about 400° F., and it contains varying quantities of dust, depending on the velocity of the gas passing through the stock, the physical condition of the stock, and the working condition of the furnace. Attempts have been made to prevent the dust, so mechanically carried, from being expelled from the furnace stack, by such construction of the gas flues that the material carried up by irregular movements of the furnace will fall back into the furnace instead of going down the downcomer flues to the dust catcher. An arrangement like this will also allow more air to be blown with less metallic loss, even under normal movement of the stock. With the use of 48,000 to 50,000 cu. ft. of air on a furnace with a 16-ft. stock line, the velocity of the gas is sufficient to raise even the heavier particles on the surface of the stock, and keep them in a state of constant vertical oscillation, making their expulsion comparatively easy with any increase of velocity due to slight stock movements, or, if the out-takes are low, the velocity of gas may lead to a continuous stream of dust going over into the downcomer pipe. It is often noticeable that particles of coke, the size of a walnut, are expelled from the bleeders without a gas coloration on hard-blown furnaces. It has been our experience, on a furnace having four gas out-takes just above the stock line, that the increase of 2,000 cu. ft. of blast per minute above 50,000 will nearly double the loss in flue dust with no perceptible change in the working condition. This condition has been experienced during a number of attempts to increase the output and places a restriction on the quantity of air permissible; and furthermore, this actual experience justifies the foregoing explanation. It is thus evident that the gas carries material of all sizes, from the very finest in the normally blown furnace to large lumps expelled by slips. The dust catcher will serve to remove the very coarsest material and also that which is too heavy for the gas velocity to take with it, unless the

dust is trapped. It is very often the case that dust sufficiently heavy to be deposited under normal velocities is picked up and carried forward by a sudden velocity increase due to opening bells or slips. The quantity of dust from a hard-slipping modern furnace to a normal one may vary from 500,000 lb. or more to 5,000 to 10,000 lb. per day, and it is evident that the quantity of dust per cubic foot of gas varies therefore over a great range.

There are three general types of primary wet washers: 1, the impinging washer; 2, the mechanical washer; 3, towers or scrubbers.

1. The impinging washer depends on directing the gas against a surface of water, the velocity imparted to the dust particles being sufficiently great to impel them into the water while the gas changes direction and goes on. These systems remove the heavier material, but expose the hot gas to the danger of becoming so moisture laden as to cause very inefficient combustion in subsequent operations unless the water be removed by further cooling.

2. Mechanical washers are generally dependent on a high-speed, motor-driven rotor revolving in a shell and having water injected in various ways. The water is broken up into a finely divided condition, and thus brought in intimate contact with the gas, being either "beaten" together or thrown together by centrifugal force, the water and sludge then flowing away. While these methods in many cases are successful in cleaning the gas, yet the capacities are low and the cost in installation, maintenance, and power is high.

3. Tower washers or scrubbers may be further subdivided as follows: (*a*) Those having the gas as the dominating element; (*b*) those having the water as the dominating element.

(*a*) In the first type the water is introduced at the top of the apparatus through nozzles and allowed to fall by gravity over baffles, or wooden grids, or in the form of rain. The gas, rising through the tower, comes in contact with the descending water and the wetted surface and thus becomes cooled and cleaned. These scrubbers depend on very low velocity of gas. If the velocity head is sufficiently high to deflect the water from its vertical course, the gas will pass up in a column through the path of least resistance, and thus prevent intimate contact. In these cases the velocity of gas determines the efficiency of the scrubber. Baffles and grids have been successfully used for increasing the efficiency, in different installations, and allow a high gas velocity to be handled, corresponding to the number of obstructions. These baffles in turn mean less gas-cleaning capacity per unit and higher resistance, which lowers the gas pres-

sure accordingly. Another disadvantage of complicated construction within the tower is the tendency of the dirt to build up on any projection unless large quantities of water are used.

(*b*) Those having water as the dominating element depend on the higher pressure and velocity of the water to prevent the gas channeling. The water is introduced through vertical nozzles, turned upward, and the flow intermittently or successively cut off one nozzle. When the flow ceases an area directly above that nozzle becomes quiescent and the gas is driven to this point by the water flowing in the others. The water passing through the nozzle then resumes, driving the gas over the next nozzle, which is cut off in turn. This is repeated until the gas is beyond the range of the nozzles. The water has a tendency, therefore, to drive the gas from one point to another, the gas being sprayed as at the same time it rises in the tower. Thus the gas is being forced in an upward spiral path, being guided and deflected over each nozzle by the velocity and pressure of the water. Screens are used over the nozzles to atomize the water. Fig. 9 in Mr. Forbes's paper shows very clearly the action of this scrubber. The dirty water carrying away the dust passes from the base of the tower by means of a siphon arrangement.

Clean Gas vs. Dirty Gas on Stoves.—In first grasping the problem of cleaning gas, it must be determined for what purposes the gas will be used. If the gas is only to be used for internal-combustion engines, then efficiency in primary cleaning may not be so necessary, as the final cleaners can assume some of the work which should be done in the first stage. If, on the other hand, the gas is to be divided after the first stage, part to stoves and boilers and part to the secondary cleaners, then the best efficiency should be gotten from the primary system. As before mentioned, the average temperature of gas from the top of the blast furnace will be about 400° F., and probably 350° at the stove burners or scrubbers, or wherever it is sent. With Mesabi ores the moisture content of this gas will be about 35 grains of water per cubic foot of gas, calculated at 62°. In one case the gas is burned raw, containing 3 grains of dust per cubic foot, and in the other it is returned to the stoves and boilers at 70°, saturated with 7.98 grains of water and 0.2 grain of dust per cubic foot.

Comparison of Stove Tests.

	Data.	Three-Pass Central Combustion Chamber. Clean Gas.	Two-Pass Central Combustion Chamber. Clean Gas.	Two-Pass Central Combustion Chamber. Dirty Gas.
1	Date of test	6/11/09	8/19/09	8/27/09
2	Furnace and stove number	5 F., 1 S	4 F., 1 S	2 F., 3 S
3	Size of stove, ft.	22 by 100	21 by 96	2 by 96
4	Area of combustion chamber, sq. ft.	25.78	28.47	28.27
5	Area of flues, first pass, sq. ft.	78.25	.....	.....
6	Area of flues, second pass, sq. ft.	36.52	.....	.....
7	Area of flues, checkers, sq. ft.	.....	77.91	77.91
8	Total heating surface, sq. ft.	49,865	39,220	39,220
9	Cubic contents, cu. ft.	20,831	17,974	17,974
10	Equivalent of 9 in. brick	360,000	135,560	135,560
11	Ratio heating surface to cubic contents	2.39	2.18	2.18
	Pressures.			
12	Gas in main, in. water	1.50	1.50	8.00
13	Gas in burner, in. water	0.82	0.54	3.77
14	Pitot tube, in. water	0.122	0.055	0.1085
15	Draft at chimney valve, in. water	0.41	0.50	1.66
16	Blast pressure, pounds	16.1	15.4	16.96
17	Barometer, in. mercury	29.32	29.17	29.36
	Temperatures.			
18	Furnace gas at burners, ° F.	86.6	88.9	300.2
19	Atmosphere, ° F.	72.5	74.0	96.6
20	Cold blast, °F.	152.8	191.7	227.0
21	Hot blast—maximum, ° F.	1,600	1,310	1,425
22	Hot blast—minimum, ° F.	1,360	970	1,000
23	Hot blast—average, ° F.	1,462	1,076	1,148
24	Chimney—maximum, ° F.	720	680	930
25	Chimney—minimum, ° F.	565	485	600
26	Chimney—average, ° F.	670	612	813
27	Engine room—dry bulb, ° F.	89.5	96.0	102.0
28	Engine room—wet bulb, ° F.	72.5	79.0	87.0
	Duration.			
29	Stoves on gas, minutes	179.0	198.75	148.75
30	Stoves on blast, minutes	60.0	64.0	50.5
	Gas.			
31	Heat per cu. ft. at 62° F., B.t.u.	105.8	94.3	99.4
32	Total gas at tempt. and press. at burner, cu. ft.	867,076	693,439	821,669
33	Total gas at 62° F. and 30 in. mercury, cu. ft.	810,333	631,963	579,827
34	Gas per min. at tempt. and press. at burner, cu. ft.	4,844	5,659	5,756
35	Gas per min. at 62° F. and 30 in. mercury	4,527	3,489	3,898
36	Analysis of gas by vol.—carbon dioxide, per cent.	15.90	13.06	12.40
	carbon monoxide, per cent.	25.00	25.78	25.40
	methane, per cent.	1 20	0.00	0.00
	hydrogen, per cent.	4.20	3.69	5.80
	nitogen, per cent.	53.70	57.47	56.40
37	Moisture per cu. ft. of gas, grains	13.53	14.79	20.59
38	Analysis of chimney gas—carbon dioxide, per cent.	24.39	21.50	22.70
	oxygen, per cent.	1.18	2.90	0.00
	carbon monoxide, p. cent	0.12	0.80	0.60
	nitrogen, per cent.	74.31	75.00	76.70
39	Air excess coefficient	1.20	1.08	—0.993

Comparison of Stove Tests.—(Continued.)

	Data.	Three-Pass Central Combustion Chamber. Clean Gas.	Two-Pass Central Combustion Chamber. Clean Gas.	Two-Pass Central Combustion Chamber. Dirty Gas.
	Blast.			
40	Air blown per min. at eng. room temp., cu. ft.	38,518	44,460	42,671
41	Grains moisture per cu. ft.	6.42	8.66	8.48
42	Air blown per min. at 62° F., cu. ft.	35,742	40,580	38,780
43	Total dry air blown, pounds.	163,836	172,959	144,993
44	Total moisture blown, pounds.	2,124	3,170	2,613
	Heat.			
45	Total heat absorbed, B.t.u.	52,756,822	37,665,082	32,834,652
46	Total heat expended, B.t.u.	65,733,124	58,404,422	57,364,804
47	Efficiency, per cent.	62.24	64.40	56.97
	Working Conditions.			
48	Time stove in service since last cleaned, mos-das.	2–29	7–19	1–14
49	Burner opening, inches,	0	Full.	5
50	Cleaning door, inches.	0	3	3
51	Blow off valve open, turns.	4	0	0
52	Piston displacement, per cent.	92.50	91.27	90.88
	Heat Balance.			
53	Heat absorbed by blast, per cent.	62.24	64.48	56.97
54	Heat lost in flue gases, per cent.	17.40	19.28	24.95
55	Heat lost due to incomplete combustion, per cent.	0.53	4.52	2.85
56	Heat lost hydrogen, per cent.	3.48	2.36	3.36
57	Heat lost by moisture in fuel gas, per cent.	0.45	0.58	1.48
58	Radiation, unaccounted for, etc., per cent.	15.90	6.78	10.39
59	Efficiency of burner, per cent.	99.47	95.48	97.15
60	True thermal efficiency, per cent.	62.57	67.53	58.64

NOTE.—Item No. 60, 54/60.

The data tabulated below form the basis for the comparative table following, and are averages of our condition during 1909. The reports of boiler and stove tests are records made within a year. No assumptions have been made unless noted.

Average tonnage basic iron per day	443 tons.
Coke per ton iron	2,256 lb.
Limestone per ton iron	1,118 lb.
Wind blown per lb. coke	60.82
Gas per ton iron	12,000 lb.
Gas at 62° per ton iron	156,000 cu. ft.
Gas at 62° used at stoves, per minute	4,170
Gas at 62° used at boilers	33,010
Heat per cu. ft. furnace gas at 62°	93.9 B.t.u.
Average efficiency stoves on rough gas	56 per cent.
Average efficiency stoves on clean gas	64
Average efficiency boilers on rough gas	56
Average efficiency boilers on clean gas	62
Force required to clean stoves per furnace	2
Wages for same	$3.91
Force required to clean boilers per turn	1 per turn.
Wages for same	$2.04
Force required at scrubbers	1 per turn.
Wages for same	$2.50

Heat Value of Coke at Tuyeres.

$4{,}450 \times 0.8411 = 3{,}743$ B.t.u. Heat available per pound coke burned to CO at tuyeres. (Average carbon in coke during 1909.)

$3{,}743 \times 0.84 = 3{,}144$ B.t.u. Equivalent in coke of heat increment in hot blast. (Percentage coke burned at tuyeres.)

Average Weight Blast per Ton Iron.

7 grains moisture. Average engine room temperature, 90°.

$7.000 \div 7000 = 0.001000$ Weight moisture per cu. ft. air.

$0.00100 \times 26.36 = 0.02636$ Volume moisture per cu. ft. (uncorrected).

$0.02636 \times \frac{459 + 90}{459 + 212} = 0.02157$ Actual volume moisture.

$0.0761 \times \frac{459 + 62}{459 + 90} = 0.07222$ Weight dry air per cu. ft. at 90° F.

$0.0761 \times 0.0784 = 0.07066$ Weight dry air actually present.

0.00100 Weight moisture actually present.

0.07166 Weight air blown per cu. ft. at 62° F.

$2{,}256 \times 60.82 = 137{,}164$ cu. ft. air blown per ton iron.

$137{,}164 \times 0.07166 = 9{,}829$ Weight moisture air blown.

$9{,}829 \times 0.98605 = 9{,}692$ lb. dry air blown per ton iron.

$9{,}829 \times 0.01395 = 137$ lb. moisture blown per ton iron.

Heat in Blast per Ton Iron per 1° F.

9,692 × 0.2375 × 1 = 2,303 B.t.u. Heat in dry air.
137 × 0.48 × 1 = 66 B.t.u. Heat in moisture.

2,368 B.t.u. Total heat moisture and air.

Cubic Feet Gas per Ton Iron—Distribution—Heat.

Average Analysis Furnace Gas—Unwashed.

CO_2 = 13.06 × 0.1227	= 0.016025	19.70
CO = 25.78 × 0.0781	= 0.020133	24.74
H_2 = 3.69 × 0.00559	= 0.000206	0.25
N_2 = 57.47 × 0.0783	= 0.044909	55.31
	0.081363	

0.1970 × 3/11 = 0.05372 Weight C. in CO_2 per pound furnace gas.
0.2474 × 3/7 = 0.10603 Weight C. in CO per pound furnace gas.

0.15975 Weight C. per pound furnace gas.

2,256 Coke per ton iron.
15 Loss in flue dust.

2,241 × 0.8411 = 1,884.9 pounds C. charged in coke.
1,117 × 0.0975 = 198.9 pounds C. charged in limestone.

1,993.8 pounds C. charged.
86.4 pounds C. in iron.

1,907.4 pounds C. in gas.

1,907.4 ÷ 0.15975 = 11,940 pounds gas per ton iron.
11,940 ÷ 0.081363 = 146,750 cu. ft. gas per ton iron at 32° F.
146,750 × $\frac{521}{491}$ = 155,702 cu. ft. gas per ton iron at 62° F.
156,000 × 0.95 = 148,000 cu. ft. gas available per ton iron allowing for 0.5 per cent. leakage.
148,000 × 0.275 = 40,700 cu. ft. gas available at stoves per ton iron.
148,000 × 0.725 = 107,300 cu. ft. gas available at boilers per ton iron.
40,700 × $\frac{443}{1440}$ = 12,520 cu. ft. gas available at stoves per minute.
107,300 × $\frac{443}{1440}$ = 33,010 cu. ft. gas available at boilers.

0.2474 × 4,325 = 1,069.9
0.0025 × 62,032 = 155.1

1,225.0 B.t.u. heat per pound furnace gas.

1,225 × 0.081363 × $\frac{491}{521}$ = 93.9 B.t.u. per cu. ft. gas at 62° F.

Comparative Working of Kennedy Two-Pass Hot-Blast Stoves. Clean and Dirty Gas.

Estimated Savings by Use of Clean Gas.

Dirty Gas.

Heat in Blast.

Gas available in stoves per ton iron		40,700
Heat efficiency of stoves on dirty gas		55.86, or 56%
Heat available in gas per ton iron	40,700 × 93.9 =	3,821,730 B.t.u.
Heat available in blast per ton iron	3,821,730 × 0.56 =	2,140,169 B.t.u.
Heat in blast per 1° F.		2,368 B.t.u.
Average temperature cold blast (from all tests)		207° F.
Rise in temperature of blast	2,140,169 ÷ 2,368 =	903° F.
Average temperature hot blast	903 and 207 =	1,110° F.

Note.—Efficiency of stove is figured on latent heat only of gas, and is representative of stove efficiency after half the life of a run. Efficiencies obtained in clean and raw gas reflect all thermal relations of the sensible heats of burned and unburned gas in both cases.

Loss in Heat to Blast while Stove is off for Cleaning.

On dirty gas, a stove is taken off for cleaning every four months. The stove is off five days. The first day the stove is left on blast throughout the day in connection with the other three stoves, and is run down to about 600°. Another day is taken in cooling the stove down to 100° F. Two days are usually required for cleaning when the stove is not in bad shape. The fifth day, the stove is put on gas and brought back to about 1,250°.

Note.—These figures were based on separate stove tests taken independently and not necessarily those preceding these calculations.

Clean Gas.

Heat in Blast.

Gas available in stoves per ton iron		39,500
Heat efficiency of stoves on clean gas		63.98, or 64%
Heat available in gas per ton iron	38,500 × 93.9 =	3,709,050 B.t.u.
Heat available in blast per ton iron	3,709,050 × 0.64 =	2,373,792 B.t.u.
Heat in blast per 1° F.		2,368 B.t.u.
Average temperature cold blast (from all tests)		207° F.
Rise in temperature of blast	2,373,792 ÷ 2,368 =	1,003° F.
Average temperature hot blast	1,003 and 207 =	1,210° F.
Gain in temperature of blast	1,210 − 1,110 =	100° F.
Gain in heat in blast	2,373,792 − 2,140,169 =	233,623
Net Saving per ton iron by higher heat	$\frac{233,623 \times 2.77}{3,144 \times 2,240}$ =	$0.0919

At the Duquesne Furnaces we have never taken a stove off for cleaning during the campaign. At such times as we reline, say during four years' service, the entire set of stoves do not accumulate more than 10,000 lb. of dirt.

Kennedy Two-Pass Hot-Blast Stoves. Clean and Dirty Gas.

Estimated Savings by Use of Clean Gas.

Dirty Gas.		Clean Gas.	
Heat taken out of brickwork by blast in cooling down to 600°	2,162,000 × 0.26 × (900 − 600) = 168,636,000 B.t.u.		
a Heat necessary to bring stove back to 1,250° F.	2,162,000 × 0.26 × 1,110 = 623,953,000 B.t.u.	No cleaning necessary.	
Net loss of heat in brickwork	623,953,000 − 168,636.000 = 455,317,000 B.t.u.		
a Net available heat in gas represented	455,317,000 ÷ 0.64 = 711,429,000 B.t.u.		
Tonnage during period of four months	443 × 30 × 4 = 53,160 tons.		
Heat lost per ton iron in gas	711,429,000 ÷ 53,160 = 13,383 B.t.u.		
Loss per ton iron by lost heat, one stove	$\frac{13,383 \times 0.64 \times 2.77}{3,144 \times 2.240} = 0.0033$		
Loss per ton iron by lost heat	0.0033 × 4 = $0.0132	Gain for clean gas	$0.0132

a Efficiency and heat of a clean gas stove figured.

Comparative Working of Kennedy Two-Pass Hot-Blast Stoves. Clean and Dirty Gas.

Estimated Savings in Use of Clean Gas.

Dirty Gas.		Clean Gas.	
Loss in Heat in Blast while Stove is off Cleaning.—(Continued.)			
The stove being off for cleaning five days, during which time none of the heat in the stove is available, the loss in heat to the blast is considerable. Our records show a drop of from 100° to 150° during this period.		No cleaning necessary.	
Gas available for stoves per min	12,520 cu. ft.		
While stove is off for cleaning 1/9 of this gas is diverted to the boilers	12,520 × 1/9 = 1,390 cu. ft. per M		
And the remainder is burned in the stoves	12,520 × 8/9 = 11,130 cu. ft. per M		
Heat usually available in gas	12,520 × 93.9 = 1,175,628 B.t.u.		
Heat available in gas while one is off for cleaning	11,130 × 93.9 = 1,045,107 B.t.u.		
Loss in available heat in gas per minute	1,175,628 − 1,045,107 = 130.521 B.t.u.		
Loss in heat in blast per minute	130,521 × 0.56 = 73,092 B.t.u.		
Loss in temperature blast	$\frac{73,09 \times 1,440}{2,368 \times 443} = 100°$ F.		
Loss in heat in blast for period stove off	78,090 × 1,440 × 5 = 526,248,000 B.t.u.		
Loss in heat for four stoves	625,248,000 × 4 = 2,104,992,000 B.t.u.		
Loss per ton iron	$\frac{39,580 \times 2.77}{3,144 \times 2,240} = \$0.0156$	Gain by use of clean gas	$0.0156
Heat available for boilers	1,390 × 93.9 × 1,440 × 4 = 75,180,096 B.t.u.		
Boiler horse power available	75,180,096 ÷ 59,784 = 1,258 B.H.P.		

Comparative Working of Kennedy Two-Pass Hot-Blast Stoves. Clean and Dirty Gas.

Estimated Savings by Use of Clean Gas.

Dirty Gas.		Clean Gas.	
Value B. H. P.	$\frac{1,258 \times 4 \times \$1.38}{2,240} = \$3.10$	Less boiler house credits.	0.0002
Value B. H. P. for four stoves	$3.100 \times 4 = 12.40$	No cleaning necessary.	
Value B. H. P per ton iron	$12.40 \div 53,160 = 0.0002$		

On account of deposition of dirt in the well of the stove, we find it necessary to clean the well of each stove out once a day. During this time the cleaning door is wide open and there is a great inrush of air. From test made with large excess of air, it appears that the efficiency drops from 56 to 50 per cent. during this period.

Four stoves off for cleaning one hour each = 4 hours per day	$\frac{4 \times 60 \times 12,520}{3}$ = (gas burned) 1,001,600 cu. ft.		
Heat available	$1,001,600 \times 93.9 = 94,050,240$ B.t.u.		
Heat available in blast	$94,050,240 \times 0.56 = 52,668,134$ B.t.u.		
Heat available in blast when off cleaning	$94,050,240 \times 0.50 = 47,025,120$ B.t.u.		
Net loss on heat	$52,668,134 - 47,025,120 = 5,643,014$ B.t.u.		
Net loss per ton	$\frac{5,643,014 \times 2.77}{3,144 \times 2,240 \times 443} = \$0.0050$	*Gain for clean gas*	$0.0050
Cost labor on cleaning stoves	1 foreman ar $2.28 = 2.28		
	3 laborers at 1.85 = 5.55		
Total	$7.83		

Comparative Working of Kennedy Two-Pass Hot-Blast Stove. Clean and Dirty Gas.

Estimated Saving by Use of Clean Gas.

Dirty Gas.		Clean Gas. *Total Savings in Operating Furnace with Clean Gas at Stoves.*	
Cost of labor saved on clean gas	$7.83 \div 2 = \$3.195$	On increased heat	$0.0919
Saving per ton	$3.195 \div 443 = 0.0088$	On cleaning stoves	0.0288
		On cleaning well	0.0050
		On cleaning labor	0.0088
		Total	0.1345
		Less boiler credits	0.0002
		Saving per ton	0.1343
		Less cost of cleaning gas (at average cost '12)	0.0345
		Net saving on stoves	$0.0998

B.t.u. Saved by Use of Clean Gas, per Ton.

	B.t.u.
Higher heat	233,623
Cleaning stove	35,318
Cleaning well	12,738
Total	281,679

281,679 ÷ 3144 = 85 lb. coke.
85 × 0.0606 = 5.2 silica.
5.2 × 1.4 = 7.3 lime.
7.3 ÷ 0.469 = 15.6 lb. limestone.

Cost of Limestone.

$$\frac{15.6 \times 1.03}{2{,}240} = \$0.0072$$

Increased Production.

2,256 × 60.82 × 443 = 60,783,995 cu. ft.
2,171 × 60.82 = 132,101 cu. ft.
60,783,995 ÷ 132,101 = 460 tons.
460 ÷ 443 = 1.0384 — 3.84 increased yield.
1.356 × 0.0384 = $0.0521 savings by increased production on cost above.

Gas Lost at Boilers.

85 × 0.8411 = 71 C. lost to gas.
71 ÷ 1,907 = 3.72 decrease in amount gas made on clean gas.
148,000 × 0.9628 = 142,500 cu. ft. gas available per ton Fe. Fce. on clean gas.
142,500 — 39,500 = 103,000 cu. ft. gas available per ton Fe at boilers.
107,300 — 103,000 = 4,300 cu. ft. gas lost to boilers per ton iron.

Summary.

On better work on stoves	$0.0998
Saving in limestone	0.0072
Saving on increased production	0.0521
TOTAL SAVING PER TON IRON	$0.1591

It is shown in the foregoing paragraphs that a considerable saving is effected through washing gas for stoves, *i. e.*, $0.1591, due principally to increased efficiency, capacity of heating available, and labor. The calculated figure of 130° difference in hot blast checks with the average temperature of our hot blast during the first eight months of this year. Nos. 1 and 2 Furnaces are on dirty gas and Nos. 3 and 4 on clean gas. All these stoves are practically the same, and the actual difference in temperature over this period is 129° in favor of

the clean-gas stoves. The boiler savings, which time prevents going into, will by similar deductions amount to a net saving of $0.0348, due to less cleaning and greater efficiency. The difference in boiler efficiency is 6 per cent.

Gas cleaning brings up a number of very interesting developments and each one requires a careful study. We have only touched on the most important ones from the standpoint of the actual operation. Our tests have been carried out with extreme accuracy and represent average conditions. Installation of gas cleaning, like most other improvements, depends on local conditions and is just another problem to be solved. It is hoped that the data submitted may be of some assistance to the engineer who is wrestling with the subject.

FREDERICK H. WAGNER,* Baltimore, Md. (communication to the Secretary†):—The paper presented by Mr. Forbes deals with the subject of Blast-Furnace Gas Cleaning in a very comprehensive manner, and depicts the various apparatus used for this purpose; but I regret that no data on the relative cost of cleaning this gas at the various plants have been given, especially as in this country the same practice is almost universal, viz.: the use of (*a*) dry dust cleaners, (*b*) static washers, and (*c*) Theisen washers.

As the various plants are of practically the same design, using apparatus of the same character but with some structural modifications, it might be assumed that the costs of cleaning the gas would necessarily approximate each other in all instances; but this is not the case, and differences amounting to from one and a half to four times the minimum cost of 14 c. per 100,000 cu. ft. can be pointed out.

These differences are due to the variation in water used in the same character of apparatus, as well as to the difference in power consumed, and in some instances the cost of labor is in excess of what it should be, due to the location and design of the apparatus.

You are all familiar with these conditions, and it would be useless to waste your time by adding to the above statement, my present object being to call to your attention two systems which are being employed in Europe at present, and to point out the differences between the two, as well as some discrepancies which have been published regarding one of them. I refer to the Feld plant at Pompey, France, and the Schwarz-Bayer plant at Donawitz, Germany.

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* Non-member.

† Received Oct. 16, 1913.

Both the Feld and the Schwarz-Bayer washers, or disintegrators, have been described in Mr. Forbes's paper, but I desire first to call attention to the structural differences in these two machines, and which differences lead to an excess in power consumption in one machine over the other. You will note that in the Schwarz washer the water is disintegrated by the action of the horizontal rods attached to the disks revolving in opposite directions, and the paddle wheel on a boat is an exaggerated example of the action of these disintegrating rods; due to the resistance of the water to the blades of the paddle wheel, the boat is propelled, and the power consumed is in proportion to this resistance. If the disintegrating rods were made infinitesimally small there would be practically no resistance, and no power consumption, but then the water would not be disintegrated, or pulverized.

This also holds good for the new Theisen washer, which also employs these atomizing rods. The confined action of these machines, and the incompressibility of the injected water, also add to the power consumed, this action causing a loss in gas pressure during the passage of the gas through the washer, this loss in pressure being dependent upon the quantity of water injected. If gas is to be delivered at a certain fixed pressure, and a considerable portion of the original pressure is dissipated in passing through the apparatus, this loss must be made good by means of a fan, an additional power requirement over that which would be normal without the above pressure loss.

In the Feld washer the action is entirely different, and its design has been based upon the result to be achieved while pursuing the path of least resistance. The rapid revolution of the vertical shaft (100 rev. per minute for the largest size) causes a film of water to mount the inner surface of the cones and to leave them through the perforations at the top in a finely pulverized or atomized state, the only resistance therefore being that offered by lifting the weight of the water up the cone, plus the friction in the single, specially designed roller bearing at the top, from which the entire weight is suspended; the maximum back pressure thrown by this washer is only 2 in. of water.

As a power-consuming machine the Feld washer is therefore of a superior design, because the moving parts of the Feld washer offer less resistance to the prime mover, and the effort to pulverize the water is minimized; as an example it may be mentioned that a Feld washer of a capacity to cool and primarily wash from 32,000 to

35,000 cu. ft. of gas per minute only requires 20 h.p. for its operation as against 115 h.p. for a standard Thiesen. I am unable to give the power for the Schwarz washer alone, as my data are for the total power used, including the fan, as explained below.

As a thermal machine some apparatus are superior to others, and the one which can bring the gas into the most intimate contact with fine sprays of water will reduce the total temperature, by condensing the water vapor carried in the gas, in the most efficient manner, and without any waste.

As explained by Mr. Forbes, the Feld primary washer is a combined washer and condenser, the lower portion being the active dust extractor and the upper portion the condenser, the hot water from the latter being the best dust remover; there the Feld washer, as a primary cleaner, can take the gas at any top temperature, the number of cooling chambers being arranged accordingly. It is a well-known fact that properly to remove dust from gas the entire dust particle must be thoroughly wetted, and as it ascends and meets the heavier water particles, it becomes weighted and falls to the bottom; this is the action in the Feld washer, where the gas enters at the bottom and leaves at the top, the water taking the counter direction.

The water vapor in the gas requires a certain amount of cold water for its condensation, and in spite of some statements made regarding the low water consumption of certain apparatus, it is a physical impossibility that this water vapor will be condensed with less cooling water than the thermal conditions require, and this theoretical figure will even be slightly exceeded in practice; the following case is given as an example of the theoretical quantity of water required for condensing or cooling purposes.

It is desired to reduce the temperature of the gas from 500° F. to 86° F., or from 260° C. to 30° C., the dew point of the gas being at 52° C. with 120 g. of water vapor content per cubic meter (46.5 grains per cubic foot). The heat value at 52° C. is 95.5 calories per cubic meter, and the heat capacity of the water-saturated gas at 52° C. amounts to 0.389 calorie per cubic meter.

At 260° C. the heat value will therefore be $95.5 + 0.389\ (260 - 52) = 176.5$ calories, or, a gas having a dew point of 52° C., due to its water content of 120 g. per cubic meter, and which gas is superheated to 260° C., possesses a total heat value of 176.5 calories referred to water content.

When the gas is brought into contact with the water in a Feld washer it passes from a superheated into a saturated condition; the heat required for superheating is transformed into latent heat by the

evaporation of water, and the total heat of 176.5 calories per cubic meter in a saturated condition corresponds to a temperature of about 63° C.; or upon entering the washer, the superheated gas is brought into such intimate contact with the water that the temperature is reduced from 260° C. to 63° C., during which period the gas passes into a water-saturated condition.

The amount of cooling water required to reduce the temperature of the gas from 260° C. to 30° C. is determined from the total heat in the gas; as seen above, the total heat in the gas at 260° C. amounts to 176.5 calories per cubic meter, and at 30° C. the total heat in the cooled gas is 31.12 calories per cubic meter, and this difference of 176.5 — 31.12 = 145.38 calories must therefore be absorbed.

Assuming that the cooling water has a temperature of 20° C. (68° F.), and that the water is heated in the cooling chambers of the washer to 55° C. (131° F.), 1 liter of water would absorb 55 — 20 = 35 calories, and for 145.38 calories there would be required $\frac{145.38}{35}$ = 4.15 liters per cubic meter of gas, or in the case of a blast furnace producing 2,700,000 cu. ft. of gas per hour (76,500 cu. m.), the amount of water used would be 76,500 × 4.15 = 317,475 liters per hour, or 31 gal. per 1,000 cu. ft. No additional water is required for washing the gas for stove or boiler use, as the hot water from the upper chambers is used for this purpose.

A statement has been published to the effect that the Schwarz-Bayer system at Donawitz used only 0.56 to 0.75 liter of water per cubic meter of gas, a physical impossibility if the gas contained any water vapor, and it is hard to imagine any condition in which this would not be true. The statement made no mention of water vapor content, but as the Feld plant at Pompey is of practically the same capacity as the Schwarz-Bayer at Donawitz, it is interesting to note the following comparison:

Item.	Donawitz.	Pompey.
Gas per hour	45,000 cu. m.	41,700 cu. m.
Gas inlet temperature	130° to 200° C.	150° C.
Gas outlet temperature	39° to 42° C.	25° C.
Temperature difference	91° to 158° C.	125° C.
Water in raw gas	.....	120 gr. per cu. m.
Water in cleaned gas	.....	2.8 gr. per cu. m.
Water used per cubic meter	0.56 to 0.75 liter	3.62 liters.
Power consumed	124 to 128 h.p.	55 to 60 h.p.

This shows that only about one-sixth of the water used at Pompey was claimed at Donawitz. In passing upon this statement, the Editor of *Stahl und Eisen*, April 17, 1913, remarks that the low power consumption at Donawitz was probably due to the low dust content in the raw gas (1.8 to 2 g. per cubic meter), and therefore states that to his knowledge in other works where the dust content was higher, the power consumption was also higher. The dust content at the Pompey works, where the Feld washer is used, is from 3 to 5 g. per cubic meter, and the lower power consumption at Pompey arises from the greater mechanical efficiency of the Feld washer as compared with disintegrators of the Schwarz or later Theisen type. Besides this, the statement from Donawitz does not separate the power required for washer and fan, but assuming that the same fan power was consumed in both plants, viz., 35 h.p., the Feld washer would have used 20 to 25 h.p. as against 89 to 93 h.p. for the Schwarz-Bayer.

Referring again to the small amount of cooling water used at Donawitz, it must be assumed that a considerable amount of this injection water was vaporized, thus increasing instead of decreasing the water burden in the gas, and that to properly condense this vapor more water must necessarily be injected; this necessarily larger amount of cooling water requires a correspondingly higher power consumption, and only after the gas has been cooled below the dew point will it be possible to clean the gas with this small quantity of water. This is proved at Pompey, where three-quarters of the cooling water is drawn off from the Feld washer at the bottom of the cooling chambers (this water can be sent to a reservoir, cooled, and used over again) and only one-fourth of the 3.62 liters, or 0.9 liter, is used to clean the gas. The entire object of the cooler is to extract the water vapor from the gas, and not to increase its water content as must have been the case at Donawitz.

Fig. 27 shows the general arrangement of a Feld primary washing plant, and follows very closely the plant now being operated at Pompey; the plant shown in the diagram consists of

A, Inlet main from dry dust catcher.
B, Hydraulic dip for main.
C, Feld washer.
D, Exhauster or fan.
E, Outlet main.
F, Hydraulic dip for main.
H, Settling tanks.
J, Mud draw-off tanks.

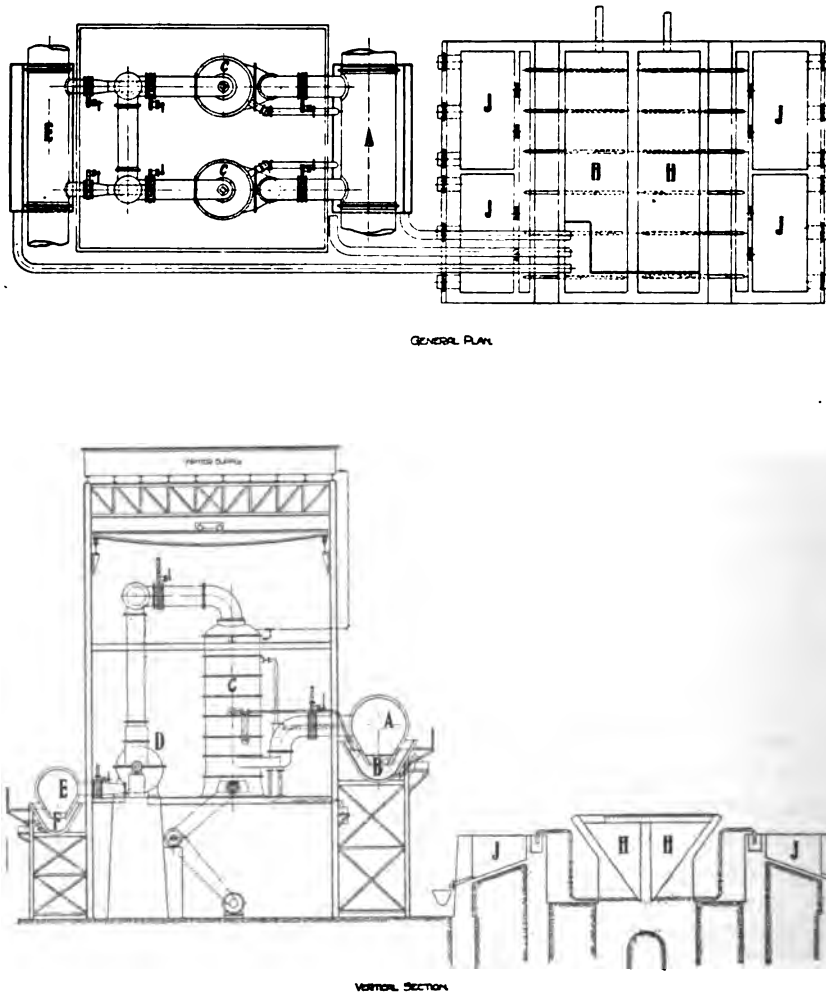

FIG. 27.—BLAST-FURNACE GAS-WASHING PLANT, FELD SYSTEM.

The method of operation is as follows: The gas from the dry dust catcher is conducted through main *A* to the bottom of the washers; this main is open at the bottom, the lower edges dipping into the hydraulic seal at *B*. Any dust deposited by friction in this main is dropped into the water and thus carried to the settling basins; the hydraulic head causing the heavier liquid to syphon out of *B* into the mud trough. This hydraulic feature also provides a safety valve against any possible explosion in the furnace, as an explosion can only blow the water seal and thus find relief. The gas passes upward through the Feld washer *C*, being cleaned in the lower sections and cooled in the upper sections. The exhauster, or fan, *D*, moves the primarily cleaned gas on through the main *E*, from whence it is conducted to the stoves and boilers. If a final treatment of the gas for engine purposes is desired, an additional washer is placed behind main *E*, and a proportionate part of the gas sent through it, the primary washer taking the place of the usual towers, Zschocke, Steinbardt, Duquesne, etc., and the final washer replacing the usual Theisen. The mud-bearing water is conducted by means of open troughs to the settling basin *H*, where the mud is allowed to settle to the bottom, the clear water being drawn off at the top and used again. The hydraulic pressure of the water column in the basin forces the concentrated mud from the bottom of the tank up through the goosenecks which deposit the mud in the draw-off tank *J*, and finally by gates into cars or other means of conveyance for final deposit.

Fig. 27 shows the washers elevated and in a semi-inclosed building, a water-supply tank forming the roof, this construction at Pompey being due to local conditions. It is not necessary to house the apparatus, only a covering for the motors being required. The plant shown in Fig. 28 is designed to treat the gases from two blast furnaces, or 5,400,000 cu. ft. of gas per hour, and is intended to give this entire amount of final cleaning for engine purposes, hence the two final washers.

The arrangement of the plant permits of easy supervision and provides a continuous operating system, as no periodical shutdown periods for cleaning are required; one plant has been in continuous operation for 36 months without any shutdown for repairs or cleaning, operating 24 hr. per day.

The estimated cost of cleaning 5,400,000 cu. ft. of blast-furnace gas per hour for gas-engine purposes, with water at $10 per million gallons and power at 1 c. per kilowatt-hour, less labor, would be about 6 c. per 100,000 cu. ft.; the labor is a minimum, as the only attention

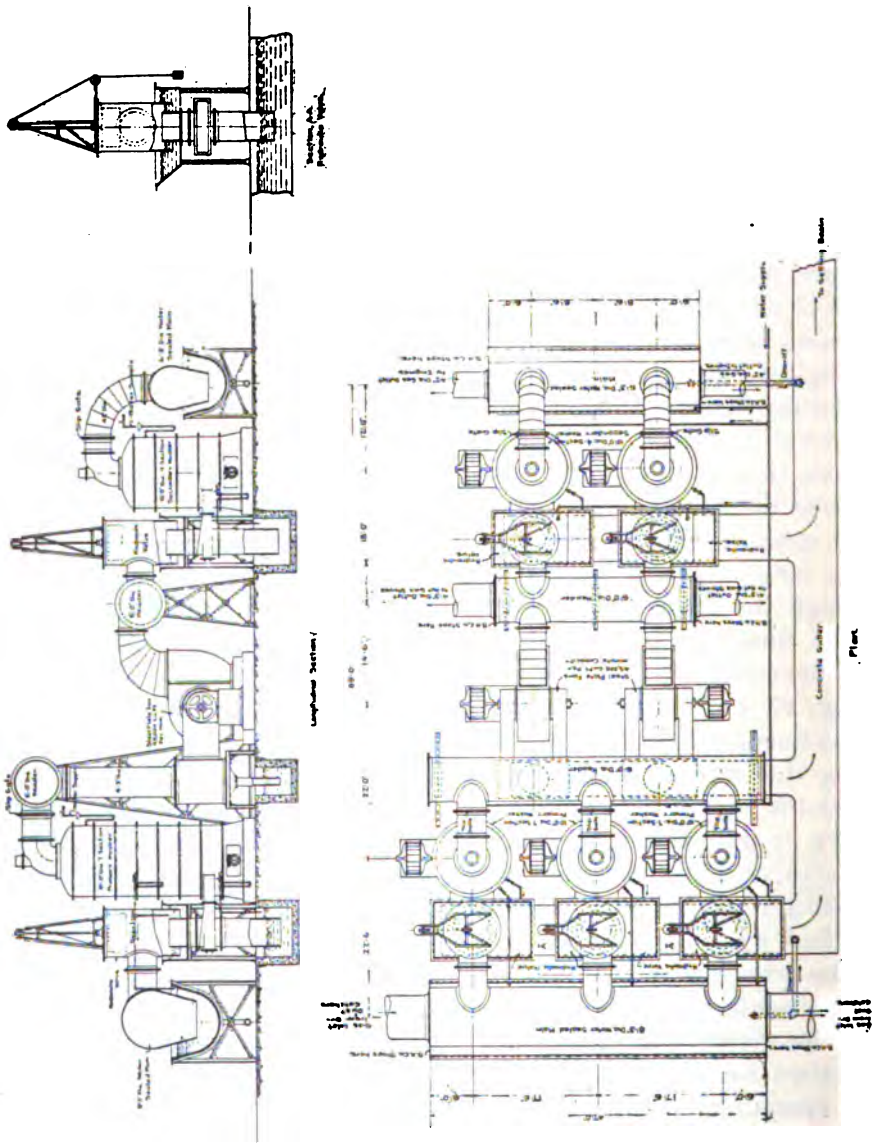

FIG. 28.—GAS-WASHING PLANT FOR TWO FURNACES.

the apparatus requires is proper lubrication, all dust being carried away by water.

This entire plant, Fig. 28, consisting of three primary and two secondary or final washers, motors, fans, hydraulic valves, and pipe connections as indicated, capable of completely cleaning 5,400,000 cu. ft. of gas per hour, would cost about $100,000, or about $92,000 without hydraulic mains, using closed pipe, erected upon purchasers' foundations, and the ground space occupied is only 100 by 47 ft. as compared with a Schwarz-Bayer plant of the same capacity at 147 by 55 ft.

I would also state that Feld washers on dust extraction having a total capacity of 355,400,000 cu. ft. of gas per day are now in operation in Europe, with the result that the final cleaning of the gas leaves only 0.008 grain of dust per cubic foot, so that this system can no longer be considered as an experiment; in this connection I would refer to the statement made by B. W. Head[1] before the West of Scotland Iron and Steel Institute. Mr. Head had visited the European Continent for the purpose of examining blast-furnace conditions; upon his return he called attention to the failure of the Scottish steel manufacturer to take advantage of the improved methods adopted in Germany, Belgium, and France, and was particular to lay stress upon the subject of gas washing for engine purposes, pointing out the difference in the methods practiced in the British Isles and those used on the Continent, mentioning especially the Feld vertical, centrifugal washer for this purpose.

The Feld washer embodies the following six principles in one machine, and which principles are universally acknowledged as necessary to completely separate dust from a gas:

1. *Cooling.*—The hot gas upon entering the washer is intimately mixed with sufficient water to lower the temperature to the desired point, thus altering its volume, density, and vapor tension, producing a dew point which precipitates the moisture held in suspension.

2. *Impinging.*—The gas, upon entering the washer, impinges upon the water spray, which in turn throws it upon the sides of the washer, from whence it impinges upon the bottom plate of the next chamber.

3. *Centrifugal Action.*—The gas is subjected to centrifugal action in that when it comes in contact with the water thrown out by the revolving cones it is whirled about in each washing chamber, because the water leaves the cones on a tangent and carries the gas with it; this centrifugal action can best be estimated when it is stated that

[1] *Journal of West of Scotland Iron and Steel Institute*, vol. xix, Nos. 6 and 7, pp. 266 and 319.

the periphery speed of the cones delivering the water is 1,580 ft. per minute.

4. *Direction.*—The direction, or flow of the gas, is constantly changed, in that the whirling water carries the gas with it in a circular direction, after which it passes upward, and then at right angles to the upward flow across the partition plate, and then up through the ports to have this cycle repeated in the next chamber.

5. *Velocity.*—The velocity is changed as many times as there are sections to the washer, the gas being contracted at the ports and expanded in the chambers; in a 10-ft. diameter washer the speed of the gas through the ports is at the rate of 42 ft. per second, and in the washing chamber at the rate of 9 ft. per second.

6. *Filtering and Washing.*—The gas is passed through a most minute spray of water, the water thus acting as a filter. No better washing effect can be produced than is that due to the water delivered by the cones; the water is so finely divided that it entirely surrounds and wets the dust particles, weighting them and causing them to drop. I regret that there are no Feld plants on dust extraction to be shown you in this country, our only washers here being used in various chemical processes.

Experiments were conducted at our shops by the representative of a large Western copper smelter with the object of determining the value of a Feld washer on the throwing down of sulphur fumes: these experiments proved so successful that the concern immediately ordered a washer to be placed in its experimental plant in California.

Before closing my remarks on wet cleaning, I would also refer to the Reco centrifugal washer, mentioned by Mr. Forbes. This washer is built with an idea of accomplishing the Feld results, but its very structural nature tends to remove any such successful issue. The washing water is thrown out into space in a thin stream from the horizontal shelves attached to the inverted cup; if these shelves are not perfectly horizontal more water will be thrown off at one point than at another, with the result that the gas will follow the line of least resistance and pass through the space but slightly filled with water, and thus escape the desired cleaning.

I also note that Mr. Forbes refers to the Halberger-Beth gas-cleaning system, which system is dependent upon bag filtration for its final cleaning of the gas. I regret to see that an attempt is being made to introduce this necessary evil adjunct of the copper, lead, and zinc smelter into blast-furnace practice; this evil in the smelter is deep rooted, the first bag house having been erected in this country in Colorado in 1887 and was used on lead fumes. It seems that the

whole tendency since that time has been to try to improve this evil instead of discarding it for something better, as is being done in Europe. This evil is most pronounced in lead smelters, where the operators are subject to continuous contact with lead dust, with consequent ill effect on health. A wet system should surely lend itself to better sanitary conditions.

A bag house passing 165,000 cu. ft. of gas per minute, operating 4,000 bags, cost, including building, woolen bags, distributing flue, fans, motors, and shaking arrangement complete, $34.40 per bag, and occupies a ground space of 19,593 sq. ft., while a Feld plant of the same capacity for smelter work, based on the above number of bags, would cost about $22 per bag, and occupy about 6,000 sq. ft. of ground area; the efficiency of the above bag house was found to be from 84 to 90 per cent., while under the same conditions a Feld plant could be depended upon to give 95 to 97 per cent. efficiency, and at the same time present a far safer investment, as if the gases, for some unforeseen reason, are not cooled properly before entering the bag chamber, fires will result, with the possible destruction of the cleaning plant. In the bag house price quoted above, precoolers and, if applied to blast-furnace practice, final coolers, were not included.

JOSEPH HARTSHORNE, Pottstown, Pa.:—It is stated in the abstract of the discussion of Mr. Forbes's paper by Mr. Wagner, read by the Secretary, that the Schwarz-Bayer cleaning plant at Donawitz and the Feld cleaning plant at Pompey are described and compared and that "the amount of water used at Donawitz is questioned."

Since neither the figures nor the reason for the question were given in the abstract, comment cannot be made upon them. I wish, however, to state that from my knowledge of Donawitz and of its management, I can bear witness to the care and accuracy of their work. I think any figures given by them may be accepted as accurate.

I saw several of these Schwarz-Bayer cleaners at work in various places in Europe in 1912. In the absence of my notes, I cannot give the exact figures, but the general results were that, in a 60,000 cu. m. machine, the requirements per 1,000 cu. m. were: total power, from 2.5 to 2.8 h.p., and water, 1.2 to 1.5 liters. Such a machine would reduce the dust from 12 g. per cubic meter to 0.1 g. A secondary, or finishing, machine would further reduce it to 0.01 g.

STUART B. MARSHALL, Dunbar, Pa.:—I wish to speak about Mr. Diehl's remarks, as my experience with the blast furnace has been

practically along those lines, without the elaborate statistical data which he has reported so completely. My observation at one furnace where the gas was not cleaned, right beside another where the gas was cleaned, enabled me to watch its effect upon the heating of the stoves of the same design; and it was an absolutely positive fact, so far as my experience is concerned, that with the cleaner gas the heats in the stoves were better and the cost of cleaning less; the gas under the boilers easier to handle, the efficiency and economy of the boilers better, and results were along the lines of Mr. Diehl's discussion.

That is, my practical experience has been, as he puts it, except that I have not had the means, or taken the time, to get up such complete data as he has. I think it is extremely valuable corroboration because it brings out the experience where you have one operated one way and the other operated the other way side by side. In my mind there is no doubt about the increased value of clean gas; especially, as Mr. Forbes states, when you have the clean gas you can get more heat efficiency in the stove by having more checkers, permitting the use of smaller spaces, and that is an important feature.

I think as we develop along that line we will lower some of our costs, as Mr. Forbes has stated in his most excellent paper.

JOSEPH W. RICHARDS, South Bethlehem, Pa.:—I would call attention to the fact that the sensible heat which is lost by cooling is an important amount of heat, and if the gases are put through the washer and cooled at 100° F. we lose a very large amount of heat, which should be saved if possible. The amount of water vapor which is ordinarily in the gas is not very detrimental to the use of the gas. It carries out some sensible heat, but I do not think the water vapor affects the combustion of the gas any more than any other inert ingredient of the gas would. I think it would be highly desirable if a method of cleaning gas could be invented other than by washing it and reducing its temperature to ordinary temperature. If the Cottrell process, for instance, could be adopted in an apparatus that could be kept hot, so that the sensible heat of the gas could be conserved, it might then clean the gas completely and save the sensible heat which is carried out of the furnace, which is such an important item to save. I do not regard washing the gas as absolutely necessary in order to remove the last trace of fine particles; they might be removed by other means and the sensible heat which the gas contains thus saved.

J. E. JOHNSON, JR., New York, N. Y.:—I have been considering

this matter along the same lines on which Professor Richards has just spoken.

If the burnt gas is discharged from the stoves or boilers at a temperature as low as or lower than that at which it comes from the furnace there is obviously no loss from the presence of water vapor; that is to say, if the gas from the furnace is at 500°, as it often is, and is discharged from the stoves at 500°, the water vapor plays no part except to cut down the maximum combustion temperature, and if the gas could be freed of dust and burned under these conditions we should undoubtedly get 10 per cent. or 15 per cent. more heat from it than we do by cooling it down to atmospheric temperature and so condensing out the water vapor.

I have just been asking if anything has been done with the Cottrell process. It seems to me that the electrostatic method of cleaning the gas while it is hot and preserving that initial heat is the one which has the most to recommend it if it can be carried out on a commercial scale and with a reasonable capital expenditure. This of course does not refer to gas for gas engines, which must be cooled in any event.

SAMUEL K. VARNES:—I purposely refrained from giving any reason for our poorer results with the clean gas, hoping some one else would bring up the point. Professor Richards has just given the reason that we decided accounted for our results. We found the loss of sensible heat to be the determining factor. The combustion temperatures were about 135° F. lower with clean gas and this lower temperature was directly shown in almost equal amount in the blast temperatures secured.

J. W. RICHARDS:—I can understand why a plumber, who is not a scientific man, will paint a radiator a light color, such as you see here by the window. That appliance is intended to radiate heat, and yet they paint it the worst color they can get for that purpose; it should be black. On the other hand, I cannot understand why you blast-furnace men paint your stoves the best color for radiating heat. You want to keep the blast as hot as possible coming from the stoves to the furnace, also in the cleaning apparatus you want to conserve the heat of the gas as much as possible, and yet you paint them all black. If I had charge of a blast furnace I should at least try experiments to see whether it is not possible to find some kind of a paint or whitewash to keep those things white and thus reduce the radiation losses.

DINGLER'SCHE MASCHINEN-FABRIK, AKTIEN-GESELLSCHAFT, Zweibruecken-Pfalz, Germany (communication to the Secretary *):—We take pleasure in supplementing Mr. Forbes's most interesting paper with the following estimates of the cost of installation and the cost of operation for several plants employing the Halberger-Beth system for the dry cleaning of blast-furnace gas. The cost of installation includes cost of erection, and in Appendix I, item 1, "Shipment" includes import duty into France.

We guarantee an operating cost of 15 to 20 pfennigs per 1,000 cu. m. of gas cleaned ($0.10 to $0.15 per 100,000 cu. ft.). The latter figure would only be reached when the local conditions are extremely unfavorable and the cost of installation is excessive.

In regard to the estimates, there are several points to which one would call particular attention. The rate of amortization is taken as 10 per cent. and the interest charge 5 per cent. Power has been figured at 2.5 to 4.5 pfennigs (½ c. to 1 c.) per horse power hour, which values are rather high for European conditions. The latter rate is used in estimating the cost of 28.8 pfennigs, Appendix III, which cost has never been reached in practice. In this case the actual power consumption was about 70 h. p., instead of 105 as estimated.

For the operation of the largest plant one attendant is sufficient, with possibly an assistant to tend to the oiling and also the discharging of filter dust into the cars.

The life of the filter bags has been taken at six months, but there are instances of their lasting 18 months. This would effect a reduction in the operating cost.

The dust, as separated by the Halberger-Beth system, can be profitably and advantageously used as a binder in the manufacture of briquettes. To this the following means of utilizing this troublesome by-product of the wet-cleaning methods can be added: namely, as a cement hardener, as an insulating material, and as an ingredient in the manufacture of glass. These various methods of utilizing the dust are of special advantage.

Appendix I.—Calculation of cost for Halberger-Beth dry gas-cleaning installation of an hourly capacity of 66,000 cu. m. (2,330,724 cu. ft.) of blast-furnace gas at 0° C. and 760 mm. barometer.

A. *Cost of Plant.*

	Francs.	
1. Shipment, including reserve parts	219,000	$42,267.00
2. Coarse tin jobbing	50,000	9,650.00
3. Iron construction of building	22,750	4,390.75
4. Foundation, masonry, and roofing	3,500	675.50
5. Motors and accessories	15,000	2,895.00
6. Tracks for handling dust, etc	2,500	482,50
Total	312,750	$60,360.75

* Received Oct. 21, 1913.

B. *Operating Cost.*

	Francs.	
1. Amortization (10 per cent.) and interest (5 per cent.)	46,900	$9,051.70
2. Power consumption, 200 h.p. for 360 days at 3 centimes per h.p. per hour	65,000	12,545.00
3. Wages for two attendants at 1,800 fr	3,600	694.80
4. Repairs, lighting, and oiling	3,000	579.00
5. Bag renewals	4,230	816.39
6. Dust transportation (average 10 g. per cu. m., 0.66 ton per hour, at 1.25 fr.)	7,140	1,378.02
7. Water consumption, 0.1 liter per. cu. m. gas $= 6.6 \times 24 \times 360 \times 0.1 =$	570	110.01
8. Steam consumption, 10 kg. per 1,000 cu. m. gas	11,450	2,209.85
Total	141,890	$27,384.77

Quantity of gas cleaned per year, 570,400,000 cu. m. (20,100,000,000 cu. ft. approximately). Therefore, cost of dry cleaning 1,000 cu. m. gas = 24.8 centimes, or, cost of dry cleaning 100,000 cu. ft. gas = $0.136. Since the dust can be sold at 7 fr. per ton it will bring a return of $0.66 \times 24 \times 360 \times 7 = 40{,}000$ fr. per year. Therefore the net operating cost is 101,800 fr., or, net cost of dry cleaning 1,000 cu. m. gas, 17.8 centimes, or, net cost of dry cleaning 100,000 cu. ft. gas = $0.097.

Appendix II.—Estimated cost of dry cleaning 1,000 cu. m. and 100,000 cu. ft. of gas by the Halberger-Beth system in a plant of 48,000 cu. m. (1,694,400 cu. ft.) hourly capacity at 0° C., and 760 mm. barometer pressure.

A. *Cost of Plant.*

	Marks,	
1. Shipment from Dingler'sche Maschinen Aktien Ges., including reserve parts and erection charge	173,500	$41,293.00
2. Iron construction for building, 55 tons at 270 marks	14,850	3,534.30
3. Foundations, masonry, roofing, and windows	2,800	666.40
4. Motors and equipment: 2 of 150, 2 of 15, and 2 of 8 h.p.	15,000	3,570.00
5. Changes in tracks, and sundries	1,600	380.80
Total	207,750	$49,444.50

B. *Operating Cost.*

	Marks.	
1. Amortization (10 per cent.) and interest (5 per cent.)	31,200	$7,425.60
2. Power, $\frac{148 \times 24 \times 360 \times 3}{100}$	38,400	9,139.20
3. Wages for two attendants at 1,500 marks	3,000	714.00
4. Maintenance, lighting, and oiling	2,600	618.80
5. Bag renewal	4,620	1,099.56
6. Dust transportation	2,480	590.24
7. Water consumption, 0.1 liter per cu. m. gas, $= 4.8 \times 24 \times 360 \times 0.01$	415	98.77
Total	82,715	$19,686.17

Quantity of gas cleaned per year equals 414,720,000 cu. m., or 14,630,000,000 cu. ft. approximately. Therefore, cost of dry cleaning 1,000 cu. m. gas = 19.9 pfennigs, or, cost of dry cleaning 100,000 cu. ft. gas = $0.134.

The flue dust, being sold at 6 marks per ton, will bring a return of $6 \times 2{,}480 = 14{,}880$ marks, or give a reduction of $\frac{14{,}880{,}000}{414{,}720} = 3.6$ pfennigs in cost per 1,000 cu. m. of gas. Therefore, the net cost of dry cleaning 1,000 cu. m. gas = 19.9 — 3.6 = 16.3 pfennigs, or, the net cost of dry cleaning 100,000 cu. ft. of gas = $0.11.

Appendix III.—Estimated cost of plant and operation of a gas-cleaning plant using the Halberger-Beth system. Hourly capacity, 30,000 cu. m. (1,059,420 cu. ft.), and can be increased to 90,000 cu. m. (3,178,260 cu. ft.).

A. *Cost of Plant.*

	Marks.	
1. Shipment, including reserve parts and after cooler	134,500	$32,011.00
2. Iron construction for building, 40 tons at 270 marks	10,800	2,570.40
3. Foundation, masonry, roofing, and windows	3,000	714.00
4. Motors and electrical equipment	14,000	3,332.00
5. Changes in tracks, and sundries	2,000	476.00
Total	164,300	$39,103.40

B. *Operating Cost.*

	Marks.	
1. Amortization (10 per cent.) and interest (5 per cent)	24,645	5,865.51
2. Power, $\frac{105 \times 24 \times 360 \times 4.5}{100}$	40,800	9,710.40
3. Wages for two attendants, one per shift, at 1,500 marks	3,000	714.00
4. Maintenance, lighting, and oiling	1,800	428.40
5. Bag renewals	2,880	685.44
6. Dust transportation (average 6 g. per cu. m. of gas = 0.18 kg. $\times$ 24 $\times$ 360 $\times$ 1)	1,550	368.90
7. Water consumption, maximum 0.1 liter per cu. m. gas = 3 $\times$ 24 $\times$ 360 $\times$ 0.076	198	47.12
Total	74,873	$17,819.77

Gas cleaned per year, 30,000 $\times$ 24 $\times$ 360 = 259,000,000 cu. m. (9,140,000,000 cu. ft.). Therefore, cost of dry cleaning 1,000 cu. m. gas = 28.8 pfennigs, or, cost of dry cleaning 100,000 cu. ft. gas = $0.194. Considering that the dust is sold at 6 marks per ton, it would bring a return of 0.18 $\times$ 24 $\times$ 360 $\times$ 6 = 9,300 marks, which brings the total operating cost down to 65,573 marks. Therefore, net cost of dry cleaning 1,000 cu. m. gas = 25 pfennigs, or, net cost of dry cleaning 100,000 cu. ft. gas = $0.169.

LOUIS SCHWARZ & Co., Dortmund, Germany (communication to the Secretary *):—As discussion of Mr. Forbes's paper on the Cleaning of Blast-Furnace Gas, we present herewith some data relative to the cost of installation and operation of the Schwarz-Bayer disintegrator.

The capital expenditure for a gas-cleaning plant capable of treating 5,500,000 cu. ft. of gas per hour is, in Germany, approximately $36,000.

Experience in many plants has proved that the power requirements range from 6 to 9 h.p. per 100,000 cu. ft. of gas.

The water consumption in treating a gas having a dust content of 2.50 to 3.50 grains per cubic foot is approximately 122 gal. per 100,000 cu. ft. of gas, when the cooling water has a temperature of 15° C.

The dust contents of gas cleaned for stoves and boilers in such an apparatus will range from 0.04 to 0.1 grain of dust per cubic foot, and in further cleaning such gas for gas engines the dust contents will range from 0.004 to 0.008 grain per cubic foot.

The cost of operation is in the neighborhood of 6 to 7 c. per 100,000 cu. ft. of gas, and this includes all operating charges.

We feel that our system has many advantages over other existing systems, especially in the way of low power consumption and low water consumption. The Schwarz-Bayer system occupies comparatively little ground space compared to most other systems.

The capital expenditure for pumps and piping is very small on account of the small amount of water consumed and on account of only requiring a head of 6 to 8 ft. of water.

EDUARD THEISEN Co., Munich, Germany (communication to the Secretary †):—As discussion of Mr. Forbes's paper on the Cleaning of Blast-Furnace Gas, we present some particulars of the operation of a Theisen disintegrator gas washer for cleaning the gas for stoves and boilers at a blast-furnace plant in the Luxemburg district:

Number of revolutions per minute, 626.8.

Capacity of plant, 1,750,000 cu. ft. of gas per hour.

Water consumed, 230 gal. per 100,000 cu. ft. of gas.

Gas suction before entering disintegrator, 2.7 in. of water.

Gas pressure after leaving the disintegrator, 5.6 in. of water.

Dust content of gases before entering disintegrator, 0.5 grain per cu. ft.

* Received Oct. 16, 1913.
† Received Oct. 16, 1913.

Dust content of gases after leaving disintegrator, 0.01 grain per cu. ft.

Power required, 11.5 to 12 h.p. per 100,000 cu. ft. of gas.

At the same plant the operating particulars obtained in cleaning gas for gas engines are as follows:

Number of revolutions per minute, 658.

Water consumed, 415 gal. per 100,000 cu. ft. of gas.

Pressure of gas before entering disintegrator, 3.5 in. of water.

Pressure of gas after leaving disintegrator, 6 in. of water.

Dust content of gas before entering disintegrator, 0.4 grain per cu. ft.

Dust content of gas after leaving disintegrator, 0.005 grain per cu. ft.

Power required, 16 h.p. per 100,000 cu. ft. of gas.

The present Theisen disintegrator is a great improvement over the former Theisen apparatus, particularly in the amount of power required, and this apparatus has been adopted on an extensive scale in European blast-furnace plants.

FOWLER & MEDLEY Co., Liverpool, England (communication to the Secretary *):—As discussion of Mr. Forbes's paper on the Cleaning of Blast-Furnace Gas, we present herewith particulars taken from the results shown by our vertical gas-washing machines installed at the works of the Partington Steel & Iron Co., Ltd., Irlam, Manchester, England, and we have chosen these machines as examples because they are both the latest in design and the largest we have yet built.

This installation consists of five of our No. 8 cleaners, taking gas from three blast furnaces. At present two furnaces are in blast and three of our cleaners in use and the results of the tests made by the Partington Steel & Iron Co. are:

Gas passing per hour per machine, 500,000 cu. ft.

Dust in gas before cleaners, 176 grains per 100 cu. ft.

Dust in gas after cleaners, 0.88 grain per 100 cu. ft.

Power used to drive each machine, 11 kw. 14 B. h.p.

Pressure used in passing the gas through the cleaners, $\frac{3}{8}$ in. of water gauge.

Water used in each cleaner, about 6,000 gal. per minute.

The water used in this plant is canal water, but we use sea water elsewhere, and also water that has been settled and cooled and returned to the cleaners.

* Received Oct. 16, 1913.

The ground space used is 10 by 10 ft. for each cleaner.

The foundation is a ring of concrete 6 ft. 6 in. in diameter and 2 ft. 6 in. high.

The cleaners themselves remain perfectly clean.

There is no visible deposit of dust in the stoves or the boiler flues.

Capital Outlay.

The price of each No. 8 machine is $3,250.

The outlay per machine erected and ready for use, with 20-h.p. vertical spindle, 600 rev. per minute motor, motor panel, wiring and foundations, but not including gas mains and water supply, would be about $3,900. No buildings are needed.

Operating Cost No. 8 Cleaner per Year of 8,736 Hr.

Power at 0.5 c. per kilowatt-hour	$475
Depreciation at 10 per cent. per annum	390
Stores	10
Attendants, 2 men for 12 machines at $7.50 per week	65
Total operating cost exclusive of water supply	$940

Advantages of the Fowler & Medley Gas Coolers and Cleaners.

1. Low first cost.
2. Low operating expenses. Note how little power is needed.
3. Efficient cooling and cleaning.
4. The machines keep themselves perfectly clean, so that, however dirty the gas entering them may be, they do not have to be shut down periodically to be cleaned.
5. The pressure required to send the gas through the cleaners is low, so that no fan is necessary for feeding stoves and boilers with the clean gas.
6. The ground space needed is small.
7. The foundations needed are cheap.
8. No buildings are required.
9. We are now prepared to build machines of such a size that one only would be needed for each blast furnace. Such a machine would not require more power or water than the No. 8 size of which we have given particulars above, so far as its capacity for cleaning is concerned.
10. For fine cleaning of the gas for use in gas engines, after it has been washed and cooled in machines of the type exhibited, we make a cleaner of exactly the same general type, but with the spraying disks so fed with water that the requirements per machine do not exceed 150 gal. per hour.

With such secondary cleaning, troubles in gas engines due to dust entirely disappear.

W. A. FORBES:—I have not much to add to the very able discussions by the various gentlemen. I would like to make it plain, however, that the blast-furnace conditions under which Mr. Varnes now operates are altogether different from the conditions under which Mr. Diehl operates, especially in relation to the amount of water vapor in the issuing gas. The furnaces with which Mr. Varnes is connected use nodulized Cuban ore containing practically no moisture, and produce very little dust, while the furnaces which Mr. Diehl operates use fine Mesabi ore almost exclusively, containing 15 to 20 per cent. of moisture, and this ore, as is well known, produces large quantities of dust. In addition to the high natural moisture in the Mesabi ore, a certain amount of water is added to the stock before charging, to try to reduce the amount of dust blown over. Consequently, the gas under Mr. Diehl's conditions contains a great deal more water vapor than under Mr. Varnes's conditions, and it has been our experience that it is a distinct advantage to reduce the amount of water vapor in the gas by cooling. This reduction in amount of water vapor results in better combustion of the gas in the stoves and under the boilers, and much more than compensates for the loss of sensible heat in the gas incident to cooling. I might also add that Mr. Diehl's ore conditions prevail at a large majority of the American blast furnaces.

In regard to the written discussion by manufacturers of various foreign systems, it is unfortunate that only the Feld, of those participating in these discussions, has an American representative, as otherwise the criticisms of some of the systems by Mr. Wagner, the Feld representative in this country, could have been answered. I think it is only fair to state that the Feld system has not been adopted on the European Continent to nearly the extent of any of the other Continental systems referred to in the discussions, or in England to the extent of the English system described by me. Answering Mr. Wagner's remarks about the Halberger-Beth system, which is dependent upon bag filtration for cleaning the gas, I would draw the gentleman's attention to the many installations of this system which have been erected in Europe since it was first brought out less than five years ago. The numerous installations are largely due to the general belief that there is no other system which cleans blast-furnace gas so thoroughly as the Halberger-Beth.

A Chart for Use in Connection with Wet and Dry Bulb Thermometers in Making Psychrometric Determinations.

BY CLARENCE P. LINVILLE, EVERETT, PA.

(New York Meeting, October, 1913.)

IN an article published in the *Iron Trade Review*,[1] I gave a convenient arrangement for the installation of wet and dry bulb thermometers for use in making moisture determinations in the air being blown into a blast furnace, and also gave a chart for use in obtaining moisture values from the thermometer readings. The data from which this chart was prepared were obtained from the tables in use by the U. S. Weather Bureau.[2] The work of Carrier[3] has shown that these tables are somewhat in error. He has recalculated the values for moisture as referred to wet and dry bulb thermometer readings, and he has prepared a psychrometric chart from the values thus obtained. This work has placed the subject of psychrometry upon a correct scientific basis. I think it is proper that we should recognize the importance of his work and give him credit for one of the most interesting and valuable researches that have been published in recent years.

Blast-furnace men are not ready to use, as he suggests,[4] a value expressed in grains of water accompanying a pound of dry air. However convenient this may be for heating and ventilating work and other engineering problems, there are several reasons for preferring the use of a value expressed in grains of water in a cubic foot of mixed air and water vapor, in connection with combustion phenomena and the effects of moisture on blast-furnace operation. Previous papers presented to the Institute have used such values and we have

[1] Determining Moisture in Air Blast, *Iron Trade Review*, vol. xlvii, No. 26, p. 1205 (Dec. 29, 1910).

[2] *Weather Bureau Bulletin No. 235*, U. S. Department of Agriculture.

[3] Willis H. Carrier: Rational Psychrometric Formulæ. *Transactions of the American Society of Mechanical Engineers*, vol. xxxiii, pp. 1005 to 1039 (1911).

[4] See discussion of paper by Bruce Walter, An Improved Method of Drying Air for Blast Furnaces, *Proceedings of the Engineers' Society of Western Pennsylvania*, vol. xxviii, No. 4, p. 292 (May, 1912).

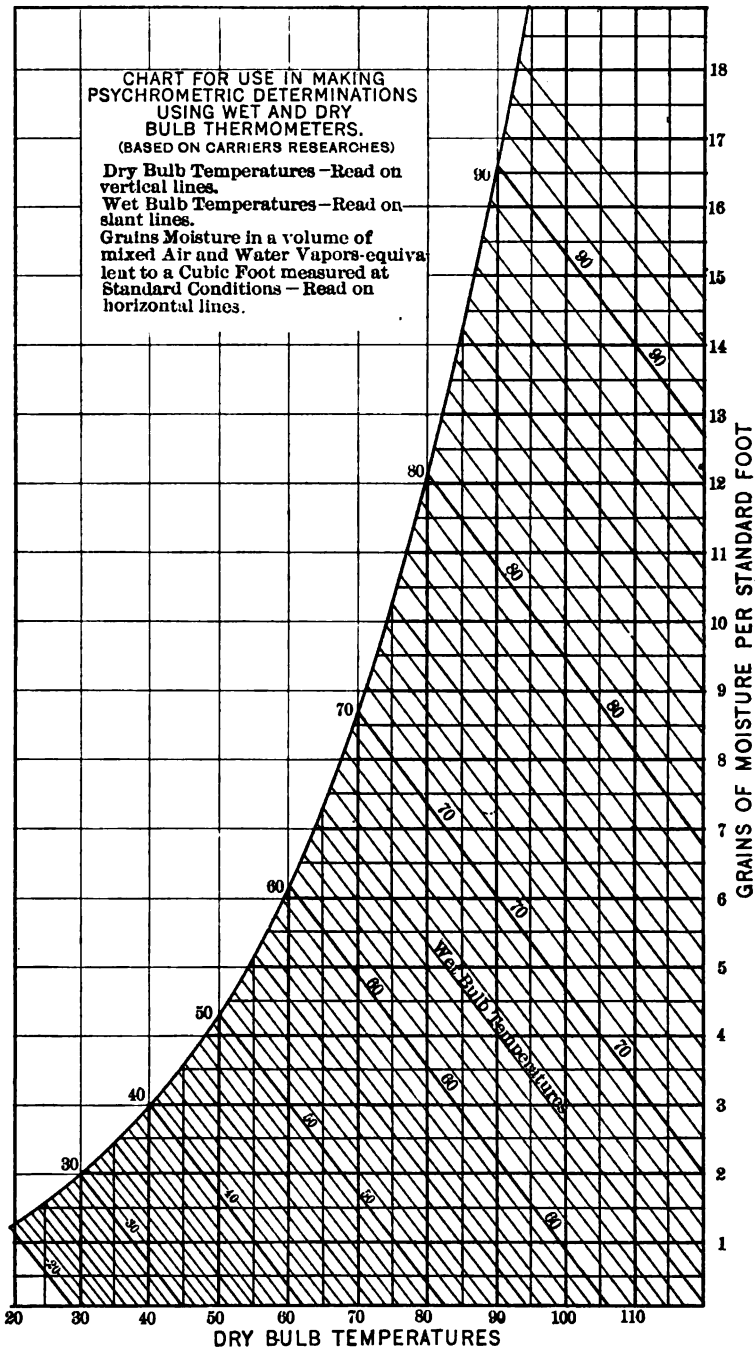

FIG. 1.—CHART FOR USE IN MAKING PSYCHROMETRIC DETERMINATIONS.

become somewhat familiar with the effects of moisture, as expressed in terms of grains per cubic foot of air, on furnace operation.

It can be shown that the theoretical temperature of combustion of carbon varies in almost exact proportion to the amount of moisture present in a cubic foot of the mixed air and water vapors, while the relation becomes much more complex when the amount of water is expressed in grains accompanying a pound of dry air.

For these reasons, and also because of a desire for a convenient chart for use in my own investigations, I have thought it worth while to prepare a chart, basing it upon the work of Carrier, but making the necessary modifications so that the readings will give, when used with temperatures taken with dry and wet bulb thermometers, values expressed in grains of moisture actually present in a volume of mixed air and water vapors, which will be equivalent to a cubic foot measured at standard conditions of temperature and pressure (32° F. and 29.92 in. barometer).

It may be noted that the method used in obtaining the values for use in preparing this chart was as follows:

Since Mr. Carrier states that a correction of 1.5 per cent. of the difference between the dry and wet bulb readings should be made by subtracting this amount from the wet bulb temperature,[5] to correct for radiation, all computations made took account of this correction and consequently the chart can be used without the necessity for making such correction of thermometer readings. It should be stated, however, that it is necessary to maintain an air velocity over the thermometer bulbs of approximately 2,000 ft. per minute. Thus, for example, if the reading of the wet bulb should be 80° with a dry bulb temperature of 110°, the corrected readings would be 79.55° and 110° respectively. Hence in preparing the chart the value used for 80° and 110° was obtained by using the reading given by Carrier's chart for 79.55° and 110°.

Since the value so obtained gives grains of moisture accompanying a pound of dry air, it is necessary to reduce it to grains in a standard foot of the mixed gases. This was done by determining the volume of the water vapor in cubic feet, standard conditions, adding it to the volume of a pound of dry air, standard conditions, and dividing the total grains by the number of cubic feet of mixed gas. This value then gives grains of water vapor in a volume of mixed air and water vapors, such as would give a cubic foot of the mixed gases at standard conditions.

Enough values were computed so that a sufficient number of points

[5] Private communication from W. H. Carrier.

could be located for the construction of a chart, and these values were plotted, and the chart constructed as shown.

As an example of the use of the chart, let us assume that the dry bulb reading is 110° and the wet bulb reading is 80°. Reading on the horizontal line we come to the vertical line reading 110.° Following this line upward until it intersects the slant line marked 80° and then reading over horizontally, we find that the readings indicate 8.07 grains of moisture per standard foot.

In case it should be desired to convert these values into grains of moisture in a cubic foot of mixed vapors at any other temperature than 32° the following table is given:

Temperature. Degrees F.	Factor.	Temperature. Degrees F.	Factor.
0	1.0669	65	0.9370
5	1.0582	70	0.9281
10	1.0469	75	0.9195
15	1.0358	80	0.9109
20	1.0250	85	0.9025
25	1.0144	90	0.8943
30	1.0041	95	0.8862
32	1.0000	100	0.8783
35	0.9916	105	0.8706
40	0.9840	110	0.8629
45	0.9742	115	0.8554
50	0.9646	120	0.8480
55	0.9552	125	0.8407
60	0.9460		

The Slagging Gas Producer.

BY WILLIAM HUTTON BLAUVELT, SYRACUSE, N. Y.

(New York Meeting, October, 1913.)

THE type of gas producer in which the ashes are fluxed and run off as slag was among the very earliest made. Ebelmen built the first one in 1840 at Audincourt, France, only a year after the installation of the first gas producer of which we have record. Charcoal was used as the fuel, with blast-furnace slag and clay as flux. My interest in this type of producer began when as a boy I saw the fluxing producer at Chester, N. J., which was invented by W. J. Taylor, and described by him in 1881.[1] This producer was invented independently by Mr. Taylor to meet the difficulties he experienced in making producer gas for roasting sulphurous iron ores. It will be of interest to review briefly his description of his producer. It was built like a small blast furnace, having a hearth 24 in. in diameter and 24 in. high. The bosh angle was 25° from the vertical; the diameter at the bosh 4 ft. and at the top 3 ft.; the total height 12 ft. The producer contained one water-cooled tuyere 12 in. above the bottom, with a 1.5-in. nozzle. Depth of the coal above the tuyere, 6 ft. Mr. Taylor mixed the coal with from 30 to 40 per cent. of basic blast-furnace slag to flux the ash. Occasionally some limestone was also used, but never limestone alone, as the use of the furnace cinder gave a larger volume of slag and made it easier to maintain a proper fluidity. The producer was blown with a small Weimer blowing engine, delivering 300 ft. of air per minute, at from 1 to 1.5 lb. pressure. The slag was tapped every 2 hr. and was black and glassy in appearance. Broken or egg size anthracite was used. The fine sizes of anthracite could not be made to work. Mr. Taylor expressed the belief that bituminous coal could be used if not too fine, but no experiments were made with this coal. Runs were made of four weeks' duration, with no stops or changes. Mr. Taylor reported the following advantages:

1. Excellent gas, very uniform in quality. The high fuel bed permitted no air to pass unconsumed, and the gas was almost entirely free from carbonic acid.

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[1] *Trans.*, ix, 309 (1880–81).

2. No cleaning of the producer to remove ashes, so no waste of coal and no cessation or irregularities in the flow of gas.

3. Quantity of gas easily controlled, and "anyone familiar with blast-furnace practice can run it, particularly if cinder for fluxing is available."

The power required for blowing the producer for gasifying 200 lb. of coal per hour was 1.5 h.p., or, assuming 3 lb. of coal per horse power, 2.25 per cent. of the coal gasified. This consumption of steam compares favorably with ordinary producers, but the rate of gasification, about 16 lb. per square foot per hour, was not remarkable for the size of fuel used. Mr. Taylor did not continue the operation of the fluxing producer, mainly on account of the skilled attention required in its operation. A man "familiar with blast-furnace practice" is often not available for the operation of producers. The high pressure of blast used and the necessity for employing a blowing engine were also among the disadvantages. He did not state why only the larger and more costly sizes of anthracite could be used, but at present comparative prices this would be a serious objection.

There appears to be no further record of experiments with the slagging producer until within the last few years. A battery of "S. F. H." slagging producers was installed at the glass works at Gironcourt in 1907, and this plant is reported to be still in operation, furnishing gas to the glass works. In the report of the operation of this plant there is no reference to the type of flux used, but it is reported that all kinds of fuel are successfully gasified, no matter what the content of ash, the only necessity being that the fuel contain sufficient fixed carbon to develop heat to gasify the carbon and fuse the ash. It is claimed that the heat required to fuse the ash is much less than the equivalent of the carbon lost in the ash of ordinary producers. One coal containing from 20 to 25 per cent. of ash gave a gas containing from 2 to 3 per cent. of CO_2, 28 to 30 per cent. of CO, and 9 to 10 per cent. of H_2 and CH_4.

The most recent work done with the slagging producer is that described by the Bureau of Mines.[2] This work was done at the Pittsburg Laboratory of the Bureau, with a view to determining the value of this type of producers for utilizing low-grade fuels. It does not appear from the report of these tests that the slagging producer gives results essentially superior to other types of producers for gasifying low-grade fuels. The experiments at the Coal Testing Plant of the Bureau at St. Louis in 1903 and 1904, showed that fuels containing very high percentages of ash could be successfully gasified in producers of the ordinary type.

The report of the Bureau of Mines does not give the dimensions of the

[2] The Slagging Type of Gas Producer, *Technical Paper No. 20, U. S. Bureau of Mines* (1912).

producer used, but six air tuyeres were employed, and in addition four separate steam tuyeres located above. On account of the extremely high temperatures, it was found necessary to provide pipe coils in the brick lining for water cooling, and magnesite brick was used between the coils and the fire. The cooling coil extended over a space 20 in. above the air tuyeres. The blast pressure used was from 5 to 16 in. of water, and it was found advantageous to pre-heat the air to about 440° F. Steam was employed at times up to 0.75 lb. per pound of fuel gasified, and it was found that this produced no chilling effect on the slag, as the steam was introduced above the point of highest temperature. It was found difficult to operate this type of producer intermittently without trouble from the chilling of the slag, so it does not appear that it would be satisfactory in cases where gas is required only in the daytime, for example. The analysis of the gas showed high CO and low CO_2 content, the latter being as low as 1.5 per cent., but no analyses are reported showing the effect of steam on the composition of the gas.

The experiments developed a number of difficulties in the fluxing of the ash, and it was found that the theoretical percentage of limestone was not nearly sufficient to produce a fluid slag. Frequently large quantities of fine ash blew over with the gas, "because of the heavy air blast," and probably some of the limestone was blown over with it. One occasion is reported where practically all of the ash and limestone escaped in this way. Comparing the air pressures reported with those used in producers of the ordinary type, it would be interesting to observe, in further experimenting, if a careful proportioning of the depth of the fuel bed to the rate of combustion would reduce the trouble from this source.

When the conditions were right and the operation of the producer was maintained continuously there was no trouble in tapping off the slag in a satisfactorily fluid condition, and in maintaining a uniformly high quality of gas. The Bureau has been carrying on experimental work since the publication of the above report, and we hope to have before long the results of this additional work.

The latest publication regarding the slagging producer is a French patent issued to E. Servais, Jan. 21, 1913. In this producer there are two sets of tuyeres, one set arranged just above the fire zone and supplied with steam or gas to reduce the temperature in the fuel bed by its decomposition. A second set of tuyeres is arranged just below, through which the air blast is admitted. These tuyeres are set in an eccentric ring, in order that a whirling movement may be given to the air in order to agitate the molten slag in the crucible. The inventor claims that this rapidly melts down any solid lumps that may form. A combustion chamber is arranged beneath the crucible in which a mixture of gas and air may be burned to help in maintaining the temperature above the fusing point of the slag.

In operating this producer the fuel is sprayed with lime water, or mixed with a suitable amount of basic blast-furnace slag.

The work done thus far with the slagging producer does not indicate that the problem has been thoroughly worked out, and there is much experimenting to be done before this producer can be put on a commercial basis. The increasing cost of the higher grades of fuel, and the large amounts of low-grade fuel that are available in almost all parts of the country, made the perfecting of a producer that will successfully gasify these low grades a most interesting and important problem.

There is one field where the slagging producer would have material advantages. It would be ideal for gasifying coals having a fusible ash. No matter how badly the ash might clinker in an ordinary producer, it would have no terrors for the slagging producer; in fact, the more fusible the ash, the better. Such coals are ordinarily gasified with the use of an excess of steam, which is not only costly to generate, but injurious to the gas, as much of it passes through undecomposed. There are many of these clinkering coals which are objectionable in the ordinary gas producer, and if the slagging gas producer can be worked out so that the cost of operation is about on a par with other types, it will find a field of usefulness waiting for it.

In connection with the treatment of clinkering coals I give below a brief bibliography of publications on clinkering, for which I am indebted to F. N. Morton of Philadelphia.

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Management of Water Gas Generators. By Dr. John F. Wing.—*American Gas Light Journal*, Apr. 18, 1910, p. 730; Apr. 25, 1910, p. 772.

Best Anthracite Coal for Use in Water Gas Generators.—*Progressive Age*, July 1, 1910, p. 547.

What Causes Clinker and What is its Chemical Composition?—Question Box, Ohio Gas Light Association, 1905, p. 252.

Relation Between the Composition of Coal Ash and Its Fusibility. By Eugene Prost.—*Moniteur Industriel*, Dec. 11, 1897. (A translation of this article appeared in the *Colliery Guardian*, Mar. 18, 1898.)

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The Freezing Point of Iron. By H. C. H. Carpenter.—*Journal of the Iron and Steel Institute*, 1908, III, p. 290.

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The Operation of the Boiler Plant and the Cost of Making Steam. By J. T. Sparrow.—*Proceedings of Association of Edison Illuminating Companies*, 1908, p. 192.

* The Temperatures at Which Certain Ferrous and Calcic Silicates are Formed in Fusion, and the Effect upon these Temperatures of the Presence of Certain Metallic Oxides. By H. O. Hofman.—*Transactionz of the American Institute of Mining Engineers*, 1899, vol. 29, p. 682.

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* NOTE: In this article the following references are given:

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Howe.—*Transactions of the American Institute of Mining Engineers*, 18, p. 724.

Gredt.—*Stahl und Eisen*, 1889, 9, p. 759; also, Åkerman's Critique, *id.*, 1890, p. 424.

DISCUSSION.

JOSEPH W. RICHARDS, South Bethlehem, Pa.:—There are two striking advantages of the slagging gas producer: one is that you can use a coal with a very fusible ash, and are not bothered by the clinkering of the coal; but the particular advantage is that on a very small floor space or ground space you get a very high capacity. Where room or space is at a premium, then, the slagging producer can give you a large output on a small area. There is one necessary loss, however, that I wish to call attention to: namely, that in a slagging gas producer you necessarily lose all the heat of the slag which comes out of the furnace, so that the heat efficiency of the furnace as a gas producer is diminished by the sensible heat of the slag coming out, whereas in the ordinary gas producer the ashes come out nearly cold and you save nearly all their sensible heat.

Over-Oxidation of Steel.

BY W. R. SHIMER AND F. O. KICHLINE,* BETHLEHEM, PA.

(New York Meeting, October, 1913.)

THE investigation herein described was carried out for the purpose of studying, both by chemical and metallographical means, the extent of over-oxidation of steel that can be accomplished by excessive overblowing in a Bessemer converter. In addition there are appended some results of basic open-hearth and crucible steels.

Remarks on Chemical Methods.

The test block used was hemispherical in shape, 4.5 in. in diameter and 2 in. thick. Drillings for analysis were obtained by drilling through the entire solid part of the block, carefully avoiding all cracks and blow-holes and mixing these drillings thoroughly. Drillings were carefully examined for scale and the finest were gone over with a microscope to be sure all scale from whatever source had been removed.

Oxygen was determined by the well-known method of Ledebur, modified according to Cushman,[1] with, however, the removal of the preheating tube, which was found unnecessary. This method has been found to give concordant results. Blanks run from 0.0003 to 0.0006 g. Duplicate determinations varied not more than 0.001 to 0.003 per cent.

It seems to be generally supposed that the hydrogen method gives only that oxygen which is combined with iron. Whether this be true or not, the content of oxygen existing as iron oxide no doubt predominates sufficiently, so that the result obtained is sufficient to indicate whether the steel is over-oxidized or not.

We are well aware that the oxides of manganese and chromium, when existing alone, are not reduced by hydrogen at a red heat. To learn of their action in steel the following work was carried out: A sample of ferro-manganese and of ferro-chrome were each exposed

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* Non-member.

[1] *Journal Industrial and Engineering Chemistry*, vol. iii., p. 372.

in the electric furnace to an atmosphere of oxygen until completely oxidized. These powdered oxides were then run for oxygen by the usual hydrogen method, but only a very small percentage of the total oxygen was obtained in either case.

A sample of rail steel of the following composition was oxidized in the electric furnace as before: C, 0.740 per cent.; Mn, 0.82 per cent.; P, 0.024 per cent.; S, 0.042 per cent.; Si, 0.098 per cent. One-half gram of these completely oxidized drillings were run for oxygen by the hydrogen method. The percentage of oxygen found was slightly higher than the theoretical amount. These reduced drillings were then run for oxides by the iron-iodide method. A residue of less than 0.10 per cent. iron was obtained, carrying with it only traces of manganese.

With the same object in view a sample of drillings from a nickel-chrome steel of approximately the following composition was oxidized as before: C, 0.35 per cent.; Mn, 0.38 per cent.; P, 0.026 per cent.; S, 0.028 per cent.; Si, 0.058 per cent.; Ni, 3.85 per cent.; Cr, 1.95 per cent. The oxidized drillings were then transferred to the apparatus for determining oxygen and run as before. To insure thorough reduction, the drillings were run for oxygen the second time. A slight increase in weight was obtained. A third run gave no further reaction for oxygen. The percentage of oxygen found was again greater than the theoretical figure. The drillings were then dissolved in iron iodide. A slight residue only was obtained, containing merely traces of Fe, Mn, and Cr.

The foregoing experiments tend to show that when manganese and chromium are oxidized, while alloyed with iron, their oxides are readily reduced by hydrogen. This can at least be true when they exist in such amounts as they are usually found in steel.

On this point we might quote from a valuable paper by G. Mars,[1] in which he says: "The oxides of both metals [meaning iron and manganese] are distinguished from each other in this way; when manganese oxide appears alone it will not be reduced by hydrogen, but if both oxides are present then the hydrogen will reduce them." A few of the results for oxygen by the hydrogen method were checked by the iron-iodide method and agreed closely.

The experiments were carried on in a Bessemer converter blowing steel for the Duplex process at the plant of the Bethlehem Steel Co. Hot metal used, with the exception of Heat *I*, had the following composition: C, 3.50 per cent.; Mn, 0.70 per cent.; P, 0.450 per cent.; S, 0.050 per cent.; Si, 1.20 per cent.

[1] Die Bestimmung der Schlackeneinschlusse im Stahl, *Stahl und Eisen.*

Experiments.

Heat A :—45,000 lb. of iron were blown in the usual way for soft steel, the converter was turned over and Test 1 taken, which analyzed as follows· C, 0.06 per cent.; Mn, 0.02 per cent.; O, 0.027 and 0.026 per cent.

The vessel was then turned up and the heat blown until the flame showed extensive over-oxidation and the metal was getting cold. It was then turned down and Test 2 taken immediately.

Test 2 analyzed: C, 0.03 per cent.; Mn, 0.01 per cent.; O, 0.074 per cent.

The oxygen content is not as high as might have been expected in a steel, after making such a vigorous attempt at over-oxidation, and when we consider that the test was poured while the steel still contained the full amount of entangled oxides and chilled before they could separate out.

To the metal in the vessel after taking test sample No. 2 was added 3,000 lb. of hot metal. To insure thorough mixing the vessel was manipulated and just a puff of air blown through. It was then turned down and allowed to rest about one minute, and Test 3 was taken while pouring into the ladle.

Test 3 analyzed: C, 0.15 per cent.; Mn, 0.03 per cent.; O, 0.023 per cent.

This test showed how quickly the over-oxidized, or over-blown, heat was deoxidized by the addition of the hot metal.

No microscopic samples were taken of Heat *A*.

Heat B :—Since Test 1 (Heat *A*) was blown very low in carbon, Heat *B* was blown to leave more carbon in the bath, and Test 4 taken. Heat *B* weighed 40,000 lb.

Test 4 analyzed: C, 0.25 per cent.; Mn, 0.04 per cent.; O, 0.027 per cent.

The vessel was turned up and blown to apparent over-oxidation, when Test 5 was taken in the usual way.

Test 5 analyzed: C, 0.04 per cent.; Mn, 0.02 per cent.; O, 0.032 per cent.

This heat was recarburized with 9,000 lb. of hot metal, the vessel was manipulated to insure thorough mixing, and Test 6 taken while pouring into the ladle, after allowing a few minutes for the reducing action of the metalloids to be exerted. The analysis of Test 6 showed: C, 0.780 per cent.; Mn, 0.13 per cent.; O, 0.024 per cent.; Si, 0.140 per cent.

See micrographs Nos. 4, 4a, 5, 5a, 6, 6a, on Plate I. Micrographs 4, 5, and 6 (taken before etching specimens) represent Tests

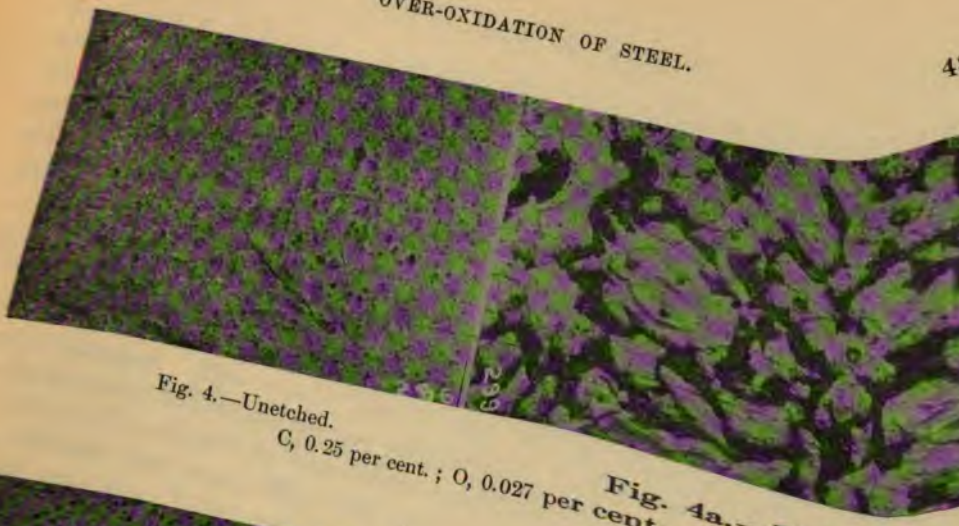


Fig. 4.—Unetched.
C, 0.25 per cent. ; O, 0.027 per cent.

Fig. 4a.—Etched.

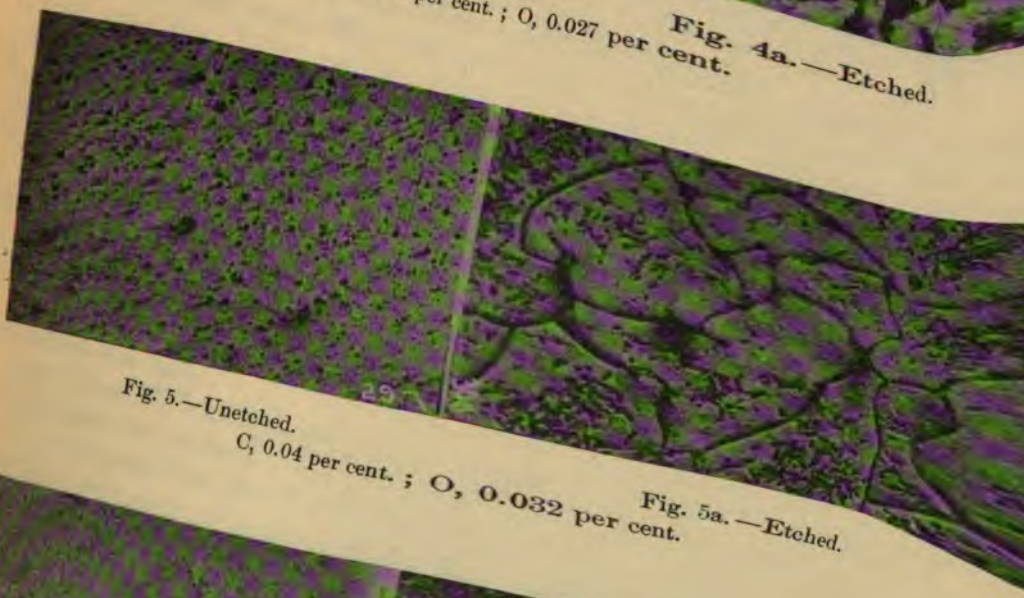


Fig. 5.—Unetched.
C, 0.04 per cent. ; O, 0.032 per cent.

Fig. 5a.—Etched.

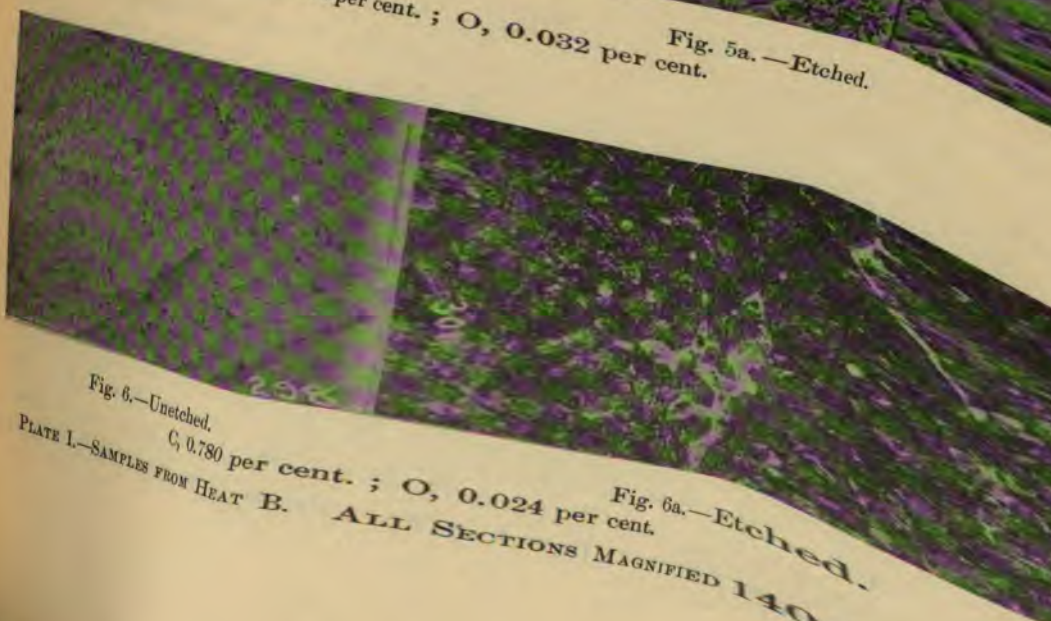


Fig. 6.—Unetched.
C, 0.780 per cent. ; O, 0.024 per cent.

Fig. 6a.—Etched.

PLATE I.—SAMPLES FROM HEAT B. ALL SECTIONS MAGNIFIED 140 DIAMETERS.

Fig. 7.—Unetched.

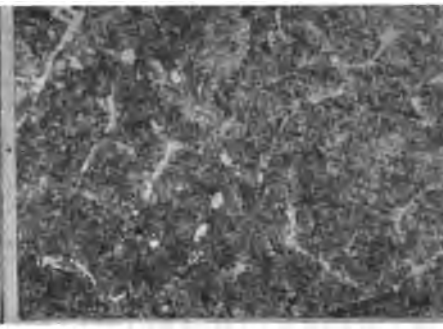


Fig. 7a.—Etched.

C, 0.744 per cent. ; O, 0.036 per cent.

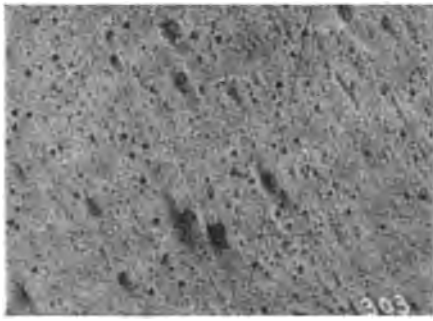


Fig. 8.—Unetched.

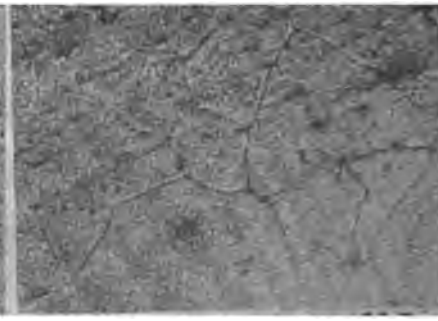


Fig. 8a.—Etched.

C, 0.03 per cent. ; O, 0.064 per cent.

Fig. 9.—Unetched.

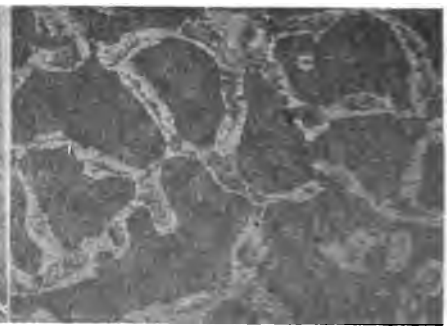


Fig. 9a.—Etched.

C, 0.430 per cent. ; O, 0.029 per cent.

PLATE II.—SAMPLES FROM HEAT C. ALL SECTIONS MAGNIFIED 140 DIAMETERS.

4, 5 and 6 and show substantially the same proportion of impurities as the chemical results for oxygen. Nos. 4 and 6 contain fewer pits then No. 5. Also, note in 5a (sample after etching) that the oxides occur in the boundaries of the ferrite crystals. This structure is almost pure ferrite, since there is practically no carbon in this sample.

In the above experiment we were unable to over-blow to any great extent on account of the hot metal being too low in silicon. We therefore awaited an opportunity when the iron was "hot" (*i. e.*, high in silicon), so we could give a long over-blow (say 5 min.) without danger. This would give a very hot blown metal and allow satisfactory conditions for over-oxidation.

Heat C:—45,000 lb. were blown and Test 7 taken, which analyzed: C, 0.744 per cent.; Mn, 0.05 per cent.; O, 0.036 per cent. The heat was then blown soft (to about 0.10 per cent. carbon) and then, since the metal was very hot, a prolonged after-blow of about five minutes was given. The vessel was turned down and Test 8 was taken, which analyzed: C, 0.03 per cent.; Mn, 0.01 per cent.; O, 0.064 per cent.

To the vessel was added 5,600 lb. of hot metal for recarburization and after holding for two minutes, the heat was poured and Test 9 was taken. The analysis of Test 9 showed: C, 0.430 per cent.; Mn, 0.02 per cent.; O, 0.029 per cent.

It will be noticed how quickly the addition of the hot metal deoxidized the bath.

See Plate II., which shows micrographs 7, 8 and 9 (unetched) and 7a, 8a and 9a (etched). No. 7 contains fewer and also smaller pits (or oxide spots) than No. 8, and in No. 9 the spots are of a different nature; not oxide pits, since they contain bright centers. (These are probably iron phosphide, out of solution, as there was about 0.480 per cent. phosphorus in the blown metal, and this was thrown out of solution when the recarburizer was added.) It will be noticed in the etched micrograph 9a that the ferrite bands contain a peculiar gray center, not found in ordinary steels, which is phosphide of iron, or steadite.

The three tests, Nos. 7, 8 and 9, were forged with the following results:

No. 7 fell to pieces under the first blow of the hammer.

No. 8 forged readily to a 0.75 in. square bar.

No. 9 fell to pieces in the same manner as No. 7.

Nos. 7 and 9 (both high in carbon) contained phosphide of iron, out of solution, which caused them to break during forging.

Micrograph 8f (Plate III.) shows the structure of No. 8 sample after forging. The large dark gray portion is a blow-hole forged together.

Heat D:—A heat of 42,340 lb. was blown soft immediately after Heat C, and Test 10 was taken. The analysis of Test 10 gave: C, 0.08 per cent.; Mn, 0.02 per cent.; O, 0.027 per cent.

Then the vessel was turned up and the heat given a five-minute after-blow and Test 11 taken. Test 11 analyzed: C, 0.02 per cent.; Mn, 0.02 per cent.; O, 0.048 and 0.049 per cent.

Then 7,620 lb. of hot metal were added to the vessel, which was manipulated to mix the bath, and Test 12 was taken immediately, the analysis being: C, 0.648 per cent.; O, 0.030 per cent.

The heat was then held three minutes and poured out; Test 13 was taken while pouring into the ladle. The analysis of this test showed: C, 0.610 per cent.; O, 0.023 per cent.

See micrographs Nos. 10 to 13 and 10a to 13a and $13a_1$, Plates III. and IV.

No. 10 shows less oxides than No. 11.

No. 11.—Some parts of the sample were better and some worse than the part photographed.

No. 12.—This piece shows less oxides than No. 11.

No. 13 shows lower oxides than No. 12. The large spots shown in the print are slag inclusions.

The etched pieces are shown by micrographs 10a to 13a and $13a_1$. They show the representative structures of the different tests. Nos. 10a and 11a, not having much carbon, show almost pure ferrite. In 11a the oxide spots occur along the boundary lines of the crystals.

Nos. 12a, 13a and $13a_1$ show the tests containing the higher carbon (after recarburization), which has thrown the iron phosphide out of solution. The ferrite in these prints contains dark centers; these are the iron phosphide. This explains the nature of the peculiar spots in the unetched tests, Nos. 12 and 13, which are not of the same appearance as oxides. Micrographs 10f and 11f (Plate IV.) show the appearance of the respective tests after forging. The gray spots are forged-out blow-holes. Note the absence of pearlite in these tests.

Test blocks Nos. 12 and 13 would not forge; they both fell to pieces on account of their iron phosphide content.

Heat E:—To 48,340 lb. of hot metal, 2,000 lb. of magnetic ore was added, before blowing, with the object of reducing the time of the blow and of studying the extent to which the steel might be over-oxidized. Test 14 was taken immediately after the blow. Test

NOTE.—Micrographs marked f represent the structures of forged tests, after etching.

Fig. 8f.—Structure after forging and etching.
O, 0.064 per cent.

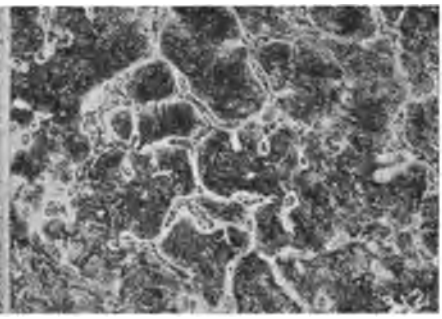


Fig. 13a_1.—Etched.

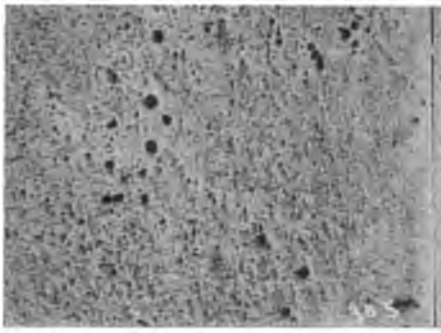


Fig. 10.—Unetched.

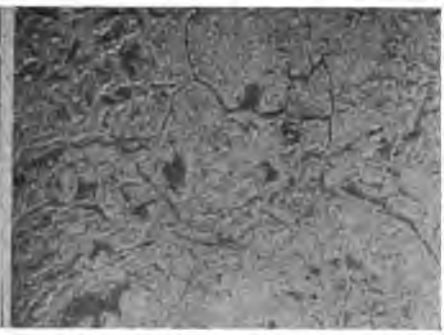


Fig. 10a.—Etched.
C, 0.08 per cent. ; O, 0.027 per cent.

Fig. 11.—Unetched.

Fig. 11a.—Etched.
C, 0.02 per cent. ; O, 0.048, 0.049 per cent.

PLATE III.—ALL SECTIONS MAGNIFIED 140 DIAMETERS.

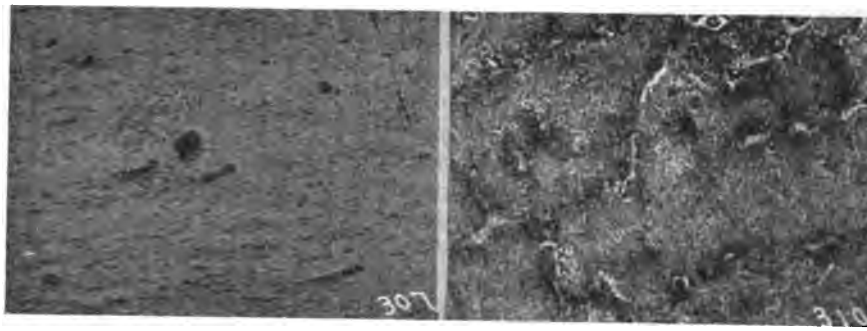


Fig. 12.—Unetched. Fig. 12a.—Etched.
C, 0.648 per cent. ; O, 0.030 per cent.

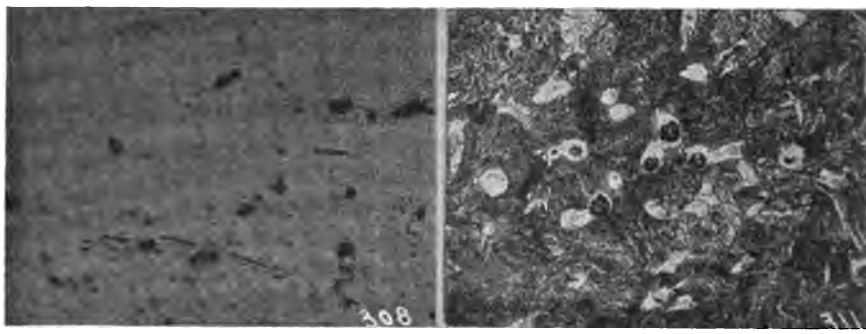


Fig. 13.—Unetched. Fig. 13a.—Etched.
C, 0.610 per cent. ; O, 0.023 per cent.

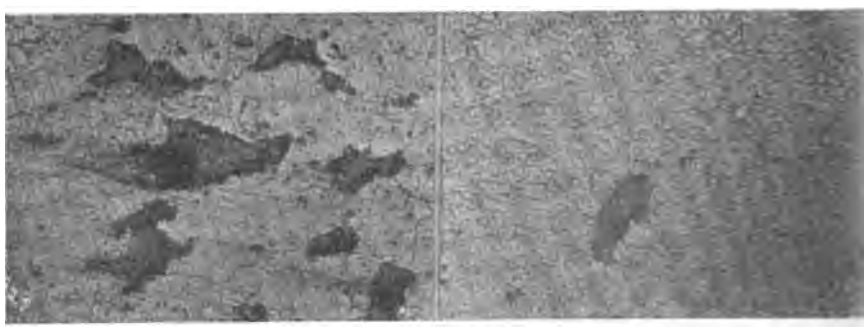


Fig. 10f.—Sample 10 forged and etched. Fig. 11f.—Sample 11 forged and etched.

PLATE IV.—ALL SECTIONS MAGNIFIED 140 DIAMETERS.

14 analyzed: C, 0.08 per cent.; O, 0.024 per cent. The blow just previous to this one (Heat D) and the one following (Heat F) each took 14 minutes to blow soft, while this heat, E, took only 12 minutes, a reduction of two minutes. From the oxygen content we see that the steel was not over-oxidized. No microscopic samples were taken of Heat E.

Heat F:—50,440 lb. of metal were blown to 0.25 per cent. carbon and 1,000 lb. of magnetic ore was added to the ladle. A violent reaction took place in the ladle. The ladle was taken to the open hearth and Test 15 taken while pouring into the furnace. The time from filling the ladle to catching the test was 30 minutes, thus giving the ore time to act and the bath to clear. The analysis of Test 15 gave: C, 0.13 per cent.; O, 0.017 per cent.

It is interesting to note how low the content of oxygen is in this steel, 30 minutes after the ore addition to ladle. No microscopic samples were taken of Heat F.

Heat G:—We then blew a heat dead soft before adding ore and, further, added ore to the vessel, in order better to take care of the reaction, or boiling over. To carry out this experiment, 48,000 lb. of metal were blown soft and Test 16 taken, which analyzed: C, 0.08 per cent.; O, 0.024 per cent.

While the vessel was resting on its side, 1,000 lb. of ore were added and five minutes allowed for the reaction to take place. A large piece of mixer scull (about 5,000 lb.) remained in the vessel from a previous heat and was partly melted away by the bath while holding the metal in the vessel for the action of the ore. Test No. 17 was then taken. Test 17 gave the following analysis: C, 0.14 per cent.; O, 0.029 per cent.

Slag from this heat contained 48.42 per cent. SiO_2. The carbon content here increased, instead of decreased, as would have been expected from the action of the ore. This was caused by the presence of the high-carbon iron scull. Therefore the same experiment was repeated when the vessel was clear and the iron hot.

See micrographs 16, 17, and 17d, Plate V.

Micrograph 16, taken from Test 16, does not show an excessive amount of oxides, the large spots being slag inclusions.

Nos. 17 and 17d were both cut from Test 17; 17d was cut from that part of the block from which the drillings for analysis were taken. The steel is low in oxides and the impurities shown in the micrographs are manganese silicate, formed from the mixer scull contained in the vessel. An analysis made on Test 17 gave 0.058 per cent. manganese silicate.

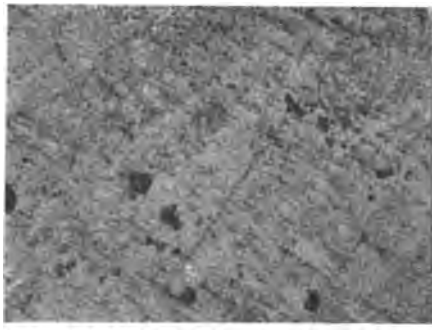


Fig. 16.
C, 0.08 per cent. ;
O, 0.024 per cent.

Fig. 17.
C, 0.14 per cent. ;
O, 0.029 per cent.

Fig. 17d.
SiO_2, 0.058 per cent.

Fig. 18.
C, 0.07 per cent. ;
O, 0.038, 0.037 per cent.

Fig. 19.
C, 0.06 per cent.
O, 0.028, 0.029 per cent.
SiO_2, 0.052 per cent.

Fig. 20.
C, 0.04 per cent.
O, 0.033 per cent.

PLATE V.—SAMPLES FROM HEATS G AND H. ALL SECTIONS MAGNIFIED 140 DIAMETERS.

Heat H:—47,000 lb. of metal were blown soft and Test 18 was taken. An analysis on Test 18 showed: C, 0.07 per cent.; O, 0.038 and 0.037 per cent.

As in Heat G, 1,000 lb. of ore were added to the vessel, and Test 19 was taken two minutes after.

Test 19 analyzed: C, 0.06 per cent.; O, 0.028 and 0.028 per cent.

After holding five minutes longer, the ore still working in the bath, Test 20 was taken, which gave the following analysis: C, 0.04 per cent.; O, 0.033 per cent.

Slag taken from this heat contained 37.36 per cent. SiO_2, showing that the ore was passing into the slag as fast as melted. Ordinary Bessemer slags contain 60 to 62 per cent. SiO_2.

Note micrographs 18, 19, and 20, Plate V. The oxides found with the microscope check with those found by chemical analysis. Large spots of manganese silicate are seen in print No. 19. Test block No. 20 contained segregations of this impurity, but they were not photographed.

The drillings from Tests 17, 18, 19, and 20, after removing the oxygen by the hydrogen method, were used for determining oxides by the iron-iodide method. Only a trace of FeO was found, but residues of 0.058, 0.054, 0.052, and 0.050 per cent. were obtained, which, on examination, proved to be composed entirely of manganese silicate; hence the manganese silicate which is shown in micrographs 17, 17d, and 19.

Heat I:—The opportunity presented itself of blowing a heat of Bessemer iron (phosphorus 0.090 per cent.). This was first blown for 0.75 to 0.85 per cent. carbon, for comparison with Tests 6, 7, 9, 12, and 13, and Test 21 was taken. An analysis of Test 21 showed: C, 0.830 per cent.; Mn, 0.30 per cent.; P, 0.120 per cent.; S, 0.031 per cent.; O, 0.017 per cent.

The heat was then blown soft, a violent reaction taking place, and Test No. 22 was taken. This test analyzed; C, 0.16 per cent.; Mn, 0.10 per cent.; P, 0.120 per cent.; S, 0.030 per cent.; O, 0.020 per cent.

While the vessel was held in the inclined position, 1,000 lb. of ore were added and, after allowing five minutes for reaction, Test 23 was taken. Test 23 analyzed: C, 0.10 per cent.; Mn, 0.03 per cent.; P, 0.120 per cent.; S, 0.030 per cent.; O, 0.040 per cent.

See micrographs 21a, 22a, 23a, 21f, 22f, and 23f, Plate VI. Microscopic sample No. 23 showed when polished the highest oxygen content. All these micrographs were taken after etching the samples.

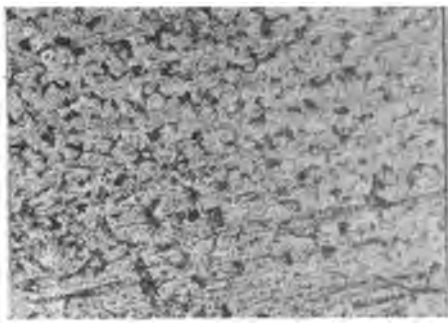


Fig. 22f.—Sample 22 forged and etched.
O, 0.020 per cent.

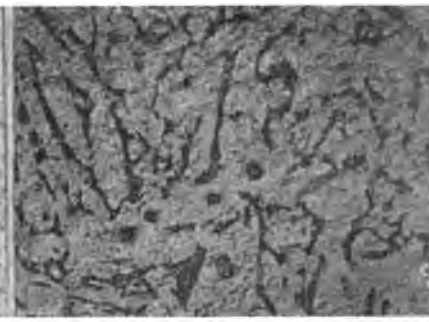


Fig. 22a.—Etched.
C, 0.16 per cent. ;
O, 0.020 per cent.

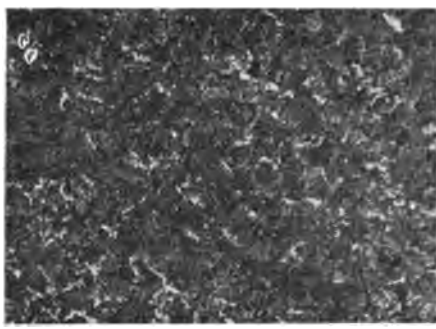


Fig. 21f.—Sample 21 forged and etched.
O, 0.018 per cent.

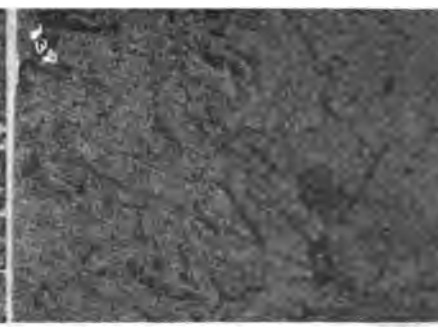


Fig. 23a.—Sample 23 etched.
C, 0.10 per cent. ;
O, 0.040 per cent.

Fig. 23f.—Sample 23 forged and etched.

Fig. 21a.—Sample 21 etched.

PLATE VI.—SAMPLES OF HEAT I ON BESSEMER IRON. ALL SECTIONS MAGNIFIED 140 DIAMETERS.

Note the absence of iron phosphide in the high-carbon Test 21a, this being because the phosphorus is low as compared with all the previous heats.

These tests, 21, 22 and 23, all forged well. The high-carbon Test 21 was the only one in this high-carbon series which we were able to forge, due to the absence of phosphide of iron out of solution. Micrographs 21f, 22f, and 23f show the etched structures of the respective forged test blocks; 23f contains a forged-out blow-hole.

For the purpose of verifying our analytical results, we ran check determinations for oxygen on the forged bars. The results are as follows:

Oxygen Content.

	As Cast.	As Forged.
	Per Cent.	Per Cent.
Test Block No. 8	0.064	0.064
Test Block No. 10	0.027	0.045 0.044
Test Block No. 21	0.017	0.018
Test Block No. 22	0.020	0.020
Test Block No. 23	0.040	0.058 0.058

Forged tests 10 and 23 were very rough, and full of seams and laps (the blocks, as cast, being of a bad shape and difficult to forge properly), and the drillings were not clean. It was impossible to avoid these cracks and seams in drilling, which accounts for these tests (10 and 23) not checking with the original oxygen content in the blocks, as cast.

It will be noted how closely tests 8, 21, and 22 check; these forged tests were free from seams, and, therefore, the drillings were clean.

Basic Open-Hearth Heat, E–14,294:—Test 24 was taken from a basic open-hearth heat, just before adding ferro-manganese. The bath had been boiled down "flat" and held for some time. Following is the analysis: C, 0.05 per cent.; Mn, 0.05 per cent.; P, 0.023 per cent.; S, 0.055 per cent.; O, 0.024 per cent. It will be noted that the oxygen content of this heat is low, in spite of the fact that the heat was boiled down flat and held some time at a high temperature, to find to what extent this condition will cause over-oxidation.

Micrograph 24 shows the oxygen in the sample as polished. This checks satisfactorily with the chemical analysis for this element.

A few results on finished open-hearth steel might be of interest; the drillings for these determinations were not taken from test blocks, but from the finished product.

Rail Steel.

Heat No.	Oxygen. Per Cent.
E-21,276	0.018
E-17,244	0.015
	0.016
E-22,377	0.018
E-17,441	0.017
E-21,428	0.019
E-24,202	0.019
E-26,065	0.017

Structural Steel (about 0.19 p. c. C.)

Heat No.	Oxygen. Per Cent.
E-14,096	0.024
E-17,104	0.022
E-22,068	0.019
E-22,277	0.015
E-24,063	0.014

FIG. 24.—SAMPLE FROM A BASIC OPEN-HEARTH HEAT.
C, 0.05 per cent.; O, 0.024 per cent. No manganese silicate.

The following are a few results taken while working basic open-hearth heats, making structural steel:

Heat No.	Preliminary Test No. 1, Taken ½ Hour Before Tapping.	Preliminary Test No. 2, Taken at Tapping, Just Before Adding FeMn to Furnace.	Drillings Taken from Finished Beam.
	Per Cent.	Per Cent.	Per Cent.
14,334—oxygen	0.037	0.025	0.022
24,092—oxygen	.....	0.030	0.019

Another basic open-hearth heat of 0.60 per cent. carbon was rolled into 4-in. square billets from a 10-in. square ingot. A section 1 in. thick was cut from the top, middle, and bottom of this billet, corresponding to top, middle, and bottom of the ingot, and drillings were taken from each section at one-third the distance from the surface. These drillings were run over a 20-mesh sieve to separate the fine from the coarse; oxygen was determined in each, and the average oxygen content calculated from the proportionate weight of fine and coarse.

	Drillings Remaining on 20-Mesh Sieve.	Drillings Passing Through Sieve.	Calculated Average.
	Oxygen. Per Cent.	Oxygen. Per Cent.	Oxygen. Per Cent.
Top	0.017	0.027	0.019
Middle	0.010	0.020	0.013
Bottom	0.011	0.015	0.012

Oxygen determinations were run on a few 0.60 per cent. carbon crucible steels, and showed the following oxygen contents:

Sample No. 1.—Test dipped out of a 90-lb. crucible and cast into a small test block: Oxygen, 0.014 per cent.

Sample No. 2.—Test dipped out of a 1-ton ladle; same heat as sample No. 1: Oxygen, 0.014 per cent.

Sample No. 3.—Finished, rolled, bar: Oxygen, 0.010 per cent.

Sample No. 4.—Rolled bar picked out, at random, from the stock rack: Oxygen, 0.010 per cent.

Samples 1 and 2 were dipped out of the bath and cast into small molds, and were, therefore, somewhat porous, which no doubt accounts for their showing slightly higher oxygen than the finished bars, which were solid.

Summary.

Every effort at over-oxidation was made, both by over-blowing and by ore addition.

1. The highest oxygen contents obtained by over-blowing, were 0.074 (in Test 2), 0.064 per cent. (in Test 8), and 0.049 (in Test 11), notwithstanding that the tests were taken immediately, in order to get the highest oxygen possible. Also the tests were cooled quickly so that no gas could escape during casting.

2. While oxidizing with ore, we obtained 0.029, 0.028, and 0.033 per cent. oxygen, by taking test samples immediately after the ore begins working through the metal. When time was allowed for the oxygen to escape after the ore addition (as in Test 15), a content of 0.017 per cent. oxygen was found.

3. A basic open-hearth heat, under oxidizing conditions, gave only 0.024 per cent. oxygen (Test 24).

4. Judging from the results obtained from heats *G* and *H*, it appears that either an unfinished heat or an over-ored heat will produce slag inclusions, such as manganese silicate, instead of iron oxide.

5. From these results, considering the manner in which they were taken, we may conclude that excessive oxygen leaves a bath of molten steel in a very short time. Also that deoxidation is readily effected by the addition of hot metal.

6. It will also be noticed that, under the same conditions, the higher the carbon the lower the oxygen.

7. Finally, it seems highly improbable that, under the usual conditions of Bessemer and open-hearth practice, with the addition of recarburizers, that a steel of over 0.030 per cent. of oxygen can be obtained, and that the highest obtainable under any conditions, without recarburizing, can hardly be over 0.075 per cent. Results higher than this must be from drillings improperly taken, or from oxidation that occurred during or after teeming.

Discussion.

Allerton S. Cushman,* Washington, D. C.:—I have been much interested in Mr. Shimer's valuable contribution. The apparent fact that the oxides of certain metals which are not reduced by hydrogen *per se*, are reducible when the oxides are dissolved in iron in small quantity, is brought out for the first time, and is worthy of the careful attention of investigators. The Ledebur method has been subjected to considerable criticism, owing to the alleged fact that it did not measure the oxygen combined with impurities. Personally I have found the method extremely helpful in spite of the minute quantities of oxygen with which it deals.

Mr. Shimer points out that the highest oxygen he obtained by over-blowing was 0.074 per cent., while the minimum quantity in a properly finished steel was in the neighborhood of 0.02 per cent. The Ledebur method is used as a constant check on the quality of heats made in practice under my direction. In dealing with a very pure commercial iron, we should expect to find and would find difficulty in galvanizing sheets made from a heat running 0.07 per cent. oxygen. In fact, we do not like to have the oxygen rise above 0.03 per cent.

I am inclined to agree with the author that excessive oxygen leaves a bath of metal in a very short time, but nevertheless I believe it takes only a very little combined oxygen to begin to make its presence felt in the pickling and galvanizing department, when sheet metal is the product manufactured. As Mr. Shimer points out, the higher the carbon, the lower the oxygen danger, and it is partly for this reason that the manufacture of carbon-free metal in the open-hearth furnace has to be so very carefully watched and controlled from the furnace through to the finished product. It is quite possible that there is a critical temperature of dissociation between iron and

* Non-member.

oxygen, although the critical points may vary in baths of different carbon content. Unfortunately, the accurate determination of the temperature of a molten bath at the time of pouring or teeming is a very difficult matter. For a long time I have been wishing that there were some reliable data which would indicate the effect of the temperature of the bath on the oxygen content. It is quite possible that at the highest temperature at which a heat is properly finished in the furnace, it is pretty thoroughly deoxidized, but that during the cooling which takes place while the pouring and teeming operations are going on oxygen begins to be taken up again.

We must not forget that iron is eager to combine with oxygen and will do so unless it is prevented, and, moreover, that we are working at the bottom of an atmospheric ocean consisting of one-fifth oxygen, and under a fairly constant pressure of 15 lb. to the square inch.

In the making of Bessemer and open-hearth steels under the usual conditions of practice and with the addition of recarburizers, I quite agree with Mr. Shimer that a metal running over 0.030 oxygen should not be encountered. In the manufacture of very pure iron in the open-hearth furnace, the matter is not so simple, and constant care and watchfulness are necessary. Even in this case, however, a heat finishing much over 0.05 per cent. in oxygen by the Ledebur method would not be considered to have been properly treated.

If an ordinary Kipp generator is used in the laboratory and mossy zinc is the source of the manufacture of the hydrogen, of course that mossy zinc goes into the Kipp generator full of air and therefore the oxygen included in the zinc will be gradually carried along with the hydrogen, and, going over the hot iron in the tube, would form water and bring in an error. If, however, the hydrogen is electrolytic hydrogen that is used in the analytical method and is carefully dried, then of course the preheating tube can be safely done away with.

J. E. Johnson, Jr., New York, N. Y.:—If the Ledebur method, as used by Mr. Cushman, does not give the oxygen from any oxide except that of iron, then that fact is greatly in its favor. If we have a method which gives oxygen from whatever source, even slag, then we get a result which has no necessary connection with the fundamental quality of the metal, but only with the casual or accidental impurities which it contains, for a small quantity of slag will contain more oxygen than a very large quantity of steel.

We have other methods of finding slag and the oxides of other substances than iron, because they are not soluble, or at least only to a very slight degree, in the bath, whereas the oxide of iron is highly

soluble in the bath and exercises a chemical influence on the character of the metal far more important than that exercised by particles of slag or other oxides undissolved.

I would like to confirm the statement of Mr. Cushman in regard to the importance of the preheating tube when using the Kipp hydrogen generator. At the Ashland plant of the Lake Superior Iron & Chemical Co., at which I was Manager for several years until recently, we made a great many determinations of oxygen and we found the preheating tube to be an absolute necessity.

There is a feature of this subject which appears to have been very generally overlooked. The affinity of carbon for oxygen rises very rapidly with the temperature, and for this reason a large quantity of carbon in the bath has a much smaller deoxidizing influence than a very small quantity of carbon at a high temperature.

I recently saw a Stoughton converter turned down when the silicon had been blown out and the metal still contained about 3 per cent. of carbon. That metal was what a furnaceman would call a wild white iron. It was bubbling actively in the converter and the metal when poured was full of blow-holes. It was evidently full of oxygen and the reaction between the oxygen and the carbon was still going on, although slowly. A similar heat blown until the carbon was down to 0.04 gave a metal as quiet and almost as free from bubbling as so much cream. It poured quietly and appeared to be absolutely free of oxygen before the addition of the deoxidizers. I am convinced from this and from the results of the experiments made by Mr. Shimer that the temperature of the bath is the principal element in controlling the amount of oxygen the bath will retain. If the temperature is high enough a very small amount of carbon remaining in the bath is sufficient to react with the oxygen and remove almost all of it; whereas, if you keep the temperature low a much larger quantity of carbon is entirely unable to accomplish this result. In my judgment, this is the reason, rather than any obscure or mysterious chemical phenomena, why the electric furnace will give a metal freer of oxygen than any other. These furnaces have at command an enormous temperature which they can maintain for an indefinite time in any sort of atmosphere that may be desired, and this high temperature enables them to cleanse the bath of oxygen as they could not do in any other way.

HENRY D. HIBBARD, Plainfield, N. J.—This serious attempt to chase elusive oxygen to his lair and find out how big he is and something of what he does there is most commendable and instructive. Some

of the authors' interpretations of the phenomena observed may, however, be open to criticism. The word steel is used in the title and elsewhere in the paper when iron would perhaps be more correct, as it deals for the most part with oxygen in decarburized iron, which of course is not steel, but which forms a step in the manufacture of steel. Determinations of oxygen in steels are indeed given, but only for comparison, not being made on over-oxidized steels. Every one now agrees that steel, to be steel, whatever other properties it has, must be usefully malleable or ductile.

Oxygen is the chief reagent used in the oxidation processes of making steel, being needed in the acid processes to oxidize and thereby eliminate carbon, silicon, and manganese from the stock, and in the basic processes phosphorus, and to some extent sulphur as well. To do so at a fair rate requires that an excess of oxygen be introduced to the molten charge. To make commercial steel demands that the excess at the end of the operation be decreased to a certain unknown content, which the determinations given in the paper fix at something under 0.02 per cent.; probably the less the better for the quality. The removal of this excess of oxygen, or the oxides it forms, is done partly by furnace treatment and partly by the addition of elements having, at steel-melting temperatures, greater affinity for oxygen than iron has. Chief of such elements is manganese. In every case enough time must also be allowed for the removal of the undesired excess of oxygen and its products. The determinations of oxygen by Walker and Patrick, by their vacuum method, indicate that the method used by the authors does not give the whole content of oxygen, but perhaps only that combined with iron. Indeed, on comparing the analyses of the blown irons with those given of finished steels it might be supposed that the difference between 0.019 and 0.029 per cent., or 0.01 per cent., of oxygen, was all there was to account for one being so red-short as to break in forging at the first blow of the hammer, while the other was of a commercial quality. It is hard to believe that such was the case.

Walker and Patrick always obtained much more oxygen by their vacuum method than by Ledebur's method, usually a number of times as much. Nevertheless, the paper under consideration is valuable in giving the comparative amounts of oxygen in iron blown in various ways and with subsequent additions of molten pig iron.

If we surely knew the total oxygen content of a steel or decarburized iron we then would need to know the proximate analyses or the compounds such oxygen formed, including oxides of manganese, silicon, and iron and their mixtures and derivatives. Knowing these,

we would still need to know their modes of occurrence, concentration, and distribution in the steel and then how to interpret such modes, particularly in terms of physical properties.

In order to show the relations of the tests and results given in the paper to actual steel making, it would have helped greatly if the usual amounts of manganese for the different grades of steel had been added and the resulting steels examined and tested. As it is, the effect of oxygen in steel is left unknown to nearly the same extent as before.

The recurrence of about 0.025 per cent. of oxygen in the tests which by reason of decreased blowing or additions of pig are less strongly oxidized than the others, raises the question as to whether or not there is a state of equilibrium when that amount is present.

The assumed probability in regard to test No. 9, that the sonim globules (solid non-metallic impurities) are iron phosphide, seems open to objection. The converter evidently had an acid lining, and if so no separation of phosphide would take place in the presence of an acid slag. The sonims in this test would appear more likely to be chiefly mixed silicates of manganese and iron, the silicon and manganese being derived chiefly from the added pig iron and being oxidized by a part of the oxide of iron contained in the over-blown metal. It would be interesting to know how the authors determined the gray centers of the ferrite bands in No. 9a to be phosphide of iron, and why they ascribe the red-shortness of Nos. 7, 9, 12, and 13 to that constituent when there is ample oxide and silicate present to cause it.

The manganese in some of the tests, notably in No. 10, is probably in the form of an oxide. This is practically proved by the fact that a 5 min. after-blow did not decrease the amount, which it must have done if it was in the metallic form, and also shows that there was little if any silica present to flux it, as practically all of that formed during the blow had presumably already entered the slag.

The forging capability of test No. 21 might perhaps have been due to its low content of oxygen, as low as that of some of the determinations given of oxygen in finished steels (0.017 per cent.), instead of absence of phosphide of iron, as the authors state. The 0.3 per cent. of manganese present was enough to prevent the presence of any large excess of oxygen in the blown metal and was clearly, it would seem, the cause of the good forging property.

HENRY M. HOWE, New York, N. Y.:—Mr. Hibbard, I believe, thinks that a dead man must not be spoken of as a man, and that over-blown metal must not be spoken of as steel. I do not know

that I can accept that. There was the old idea very deeply rooted among very intelligent observers that if you once very badly over-oxidized metal you never could get it back so that it would be really very good metal. It would pass all common commercial tests, but never would be very good metal. Many of the things that have been reported from the electric furnace would seem to indicate that that was a mistake, and that the reason you did not get good metal was that as the open-hearth furnace was actually constructed and operated, you did not have an opportunity to get the oxygen out. You could not pig back effectively, because the re-oxidation followed so fast on your de-oxidation. Very likely Mr. Acker can tell us how that is from his experiments. Having over-oxidized so very far, farther almost than any one else, was he then able to make really good steel out of it?

I should like to ask about the effect of temperature on the oxygen. If I understand Mr. Cushman, it is with falling temperature that the solubility of oxygen increases, in his opinion. It has been asserted that when you rabbled the bath and got enormous evolution of gas, that evolution was due to the lowering of the temperature by the cold rod, implying that lowering the temperature favored the removal of oxygen by forming CO. Is he really prepared to say that the evolution of gas on stirring is purely mechanical and is not due to the lowering of the temperature by the rod?

ALLERTON S. CUSHMAN: I think it will be seen that I merely raised a question in regard to the deoxidation of a metal bath in the open-hearth furnace. I had no intention of making any definite statement in regard to the subject. Most of my experience in iron metallurgy has been in connection with the development of pure commercial irons as made in the basic open-hearth furnace, which called for a bath several hundred degrees hotter than an ordinary steel bath, and the heat is kept on for a longer time. Certain things that have come under my observation have led me to question whether, under the conditions in which we are working, our metal is not more thoroughly deoxidized before tapping off than it is in the finished metal. It is quite possible that under the slightly reducing conditions of the open-hearth furnace and at a very high temperature, toward the end of the heat the superoxidized condition of the bath disappears and the metal is finished fairly well deoxidized, before any deoxidizing agent has been added. In other words, I question whether, as a heat is conducted in the open-hearth furnace, a certain portion of it does not represent a superoxidation and another portion of it a

subsequent reduction as the temperature rises. I believe this would be an interesting point to investigate in a systematic manner.

P. H. GRIFFIN, New York, N. Y.:—I would like to ask if any record has been made of the difference in oxygen content between the steel made at the present time and the steels which were made 15 or 20 years ago.

ALBERT SAUVEUR, Cambridge, Mass.:—We should be thankful for any information added to the little we really know regarding the presence of oxygen in steel, and we should be indebted to Mr. Shimer for his valuable contribution. It is, I think, surprising to note the slight difference in oxygen content between a normal heat and an excessively over-oxidized one; namely, 0.03 per cent. in the one case, and some 0.06 per cent. in the other, seeing that the first steel should be thoroughly malleable, and the latter, red-short. We have all been taught to believe that steel at a high temperature is readily oxidized, and still, according to Mr. Shimer, it will not take up more than 0.06 or 0.07 per cent. of oxygen. Surely the craving of the metal for oxygen is easily satisfied. Do chemists believe that they have an accurate analytical method for the determination of that element?

JOSEPH W. RICHARDS, South Bethlehem, Pa.:—When Professor Walker and Mr. Patrick gave a paper on the determination of total oxygen in steel before the International Congress of Applied Chemistry, in September, 1912, I thought that we had then arrived at the method which would be thenceforth used in all such investigations as this under discussion. I think this investigation must be regarded as incomplete, because the authors did not use the vacuum-furnace method for determining the total oxygen. I do not say that the Ledebur method should *not* be used, for it gives us some information. But it was absolutely determined by Walker and Patrick's tests that the steel which gave no oxygen by the Ledebur method gave oxygen by the electric vacuum-furnace method. Therefore, I think that any investigator at the present time who is looking for the oxygen in steel ought to use either the vacuum-furnace method or both methods. One method supplements the other, and each gives some information which the other does not give. But the proper one to use is necessarily the electric vacuum-furnace method, to give the total oxygen. There certainly appear to be some inconsistencies in the results, which I think would have been cleared up by having used both methods.

Some one has asked why the metal was not recarburized and finished. But at Bethlehem they are using the duplex process, which

gave an opportunity of over-blowing this steel and then taking it to the open-hearth furnace, so that the heat was not lost. Otherwise, these tests would not have been made. There is no Bessemer plant that would have over-blown this steel this way if it was not to be subsequently treated and used in the open-hearth or electric furnace. That explains why the steel was not finished in the ordinary way and the manganese added to it.

As to the reduction of the oxide of chromium, we must recognize the fact that the oxides which are in the bath and which will separate out as the metal cools are not necessarily there as pure oxides. They are probably there as combinations with the oxide with iron. We have oxide of iron and oxide of chromium in chromite, a naturally occurring compound, and it is altogether probable that we have in the bath there not chromium oxide, but something analogous to chromite, and this combination of iron oxide with chromium would be more easily reducible than the pure oxide of chromium.

Mr. Johnson's remark that a bath in a small converter was boiling quietly, with all its carbon in after the silicon had gone out, is easily explained by the fact that that was an evidence that the carbon was performing deoxidation of the bath. That boiling was evidently due to the action of the carbon on the iron oxide and the escape of CO gas. That brings us back to the fundamental course of oxidation of the bath, and that is certainly that the iron is oxidized continuously in large quantity, and then we have simultaneously, or nearly so, deoxidation all the way through the blow.

A. S. CUSHMAN:—I cannot agree with Professor Richards in his recommendation that the Arsem furnace method for determining the oxygen is either necessary or desirable. It is true that the Arsem furnace method determines total oxygen; that is to say, not only such oxygen as may be combined with silicates and included slag, but also any oxygen that may be present combined with aluminum if aluminum has been carelessly used as a deoxidizer. This method also includes oxygen that may be combined with segregated manganese or any other oxide which may be present as an impurity. As the Arsem furnace does not enable us to distinguish what the oxygen it determines is combined with, I do not think it yields data of the slightest value. It seems to me that what the iron and steel makers want to know is the condition of the oxidation of their bath, quite irrespective of how much slag may be included in the metal. In my opinion, the Ledebur method is the only one that gives this particular information, as it confines itself to determining, in the main, the oxy-

gen which is combined with iron and which, I take it, is a measure of the condition of the oxidation of the bath. If a given metal contains any considerable quantity of slag inclusion, the microscope will give the information not only qualitatively but to some extent quantitatively.

I have seen reason to think that the behavior of a given metal in the pickling operation depends to a very large degree upon its condition of oxidation, and I therefore think that the Ledebur method gives the manufacturer better control of his product, as it helps him to decide whether his heat treatment in the furnace has been running normally and regularly. The success of the galvanizing operation depends very largely upon the success of the pickling operation, and therefore I for one like to know what the Ledebur oxygens are running in normal and standard practice, whereas the results of the Arsem furnace method I should not be able to interpret.

JOSEPH W. RICHARDS:—What we want is both methods. I think Mr. Cushman will agree with me that the included slag is of probably as much importance as the included iron oxide, when it comes to pickling and other questions, and we should know its amount also.

P. WEILLER,* Perth Amboy, N. J.:—In manufacturing steel in the open-hearth process, or in any other, the steel bath is covered all the way through by a layer of slag containing a large amount of iron and oxide in solution.

We all know that iron oxide is soluble in metallic iron. It is obvious that the iron oxide formed in the steel-making process will divide between the slag and the bath. The amount of oxide in the finished steel bath will be dependent on the equilibrium between slag and steel bath; that is to say, the amount of oxygen in the metal will mostly depend upon the nature of the slag.

In the electric furnace process this idea is realized. In this process the reduction is completed in a very particular way. After using the ordinary reducing agents in lump form in the steel bath some finely ground ferro-silicon is thrown on the surface of the limy slag. This slag, being very stiff, does not allow the reducing agent to reach the metal bath. In this way all heavy metal oxides in the slag are reduced, the equilibrium is destroyed, a further portion of oxides is allowed to pass from the metal bath to the slag and is in turn reduced, and so on until all the oxide is reduced and the slag is perfectly white. In this way a nearly perfect reduction can be obtained.

* Non-member.

The amount of oxygen being dependent on the equilibrium between slag and metal bath, it is easily understood that this equilibrium will be greatly influenced by the temperature and by the composition of both metal and slag.

The only way to investigate what is called the over-oxidation of steel is to study the iron oxide-iron diagram and determine the equilibrium between slag and iron bath.

MESSRS. SHIMER AND KICHLINE (communication to the Secretary*): —Dr. Cushman's valuable discussion throws additional light on the subject under investigation. It is gratifying to the authors to know that their results are corroborated by the experience of this authority. No doubt the use of a preheating tube with the Kipp generator is an excellent precaution in a general sense. The authors used the preheating tube for a time, and then discontinued its use because it was found that the blank determination, after discarding the preheating tube, was not higher than before. We obtained 0.0003 to 0.0006 g. increase in weight as a blank, which was deemed entirely satisfactory. Blanks were run at the beginning of each day's work, so that if the generator was giving an impure hydrogen it would be detected immediately.

We can hardly accept Mr. Johnson's remark, that the hot metal, after the silicon has been taken out, containing about 3 per cent. of carbon, can be full of oxides, as they would immediately be taken up by this carbon at the high temperature of the metal at that period. His statement that the bath of metal containing 3 per cent. of carbon was "full of oxygen" gives us no conception as to the probable percentage he has in mind. In the strictest sense of the word the term "full of oxygen" would mean all that the bath would hold, which, of course, is correct, but from our investigation we have found it to be a comparatively low figure. When a Bessemer heat is at the point of 3 per cent. carbon there is quite an appreciable amount of manganese present, and this alone, even if carbon were not combining with the oxygen, is sufficient to eliminate the oxygen of the bath almost instantly.

Mr. Hibbard seems to believe that the difference in the forging qualities between tests Nos. 9 and 21 is due to the slight difference between the percentages of oxygen in them, 0.029 and 0.017 per cent. respectively. It will be recalled that test No. 9 contained 0.43 per cent. of carbon and about 0.450 per cent. of phosphorus, while test No. 21 was made of iron containing 0.120 per cent. of phos-

* Received Nov. 13, 1913.

phorus. Therefore the reason for No. 9 being red-short and breaking in forging and No. 21 forging readily, can hardly be ascribed to the difference in oxygen content, the main difference being in the phosphorus content of the two tests. Test No. 8, from the same heat as No. 9, contained 0.064 per cent. of oxygen and only 0.03 per cent. of carbon and forged readily. That the combination of high carbon (0.430 per cent.) and high phosphorus (0.450 per cent.) in the above test caused it to be red-short is quite evident; the oxygen content was only 0.029 per cent.

The samples containing iron phosphide in the ferrite boundaries were etched with cupric chloride and that part making up the dark centers in the ferrite network turned black. This, together with Stead's test for iron phosphide (heat tinting), shows it to be iron phosphide. That they are not sonims (non-metallic impurities) we are certain, or they would have been discovered on the polished surfaces before etching. The heat showing the greatest amount of "sonims" under the microscope was Heat *G*, with a skull in the vessel, which was determined and found to contain 0.058 per cent. of slag inclusions (sonims), while the iron phosphide content of this same metal runs from 2.90 to 3.10 per cent. The quantitative estimation of the dark elongated centers in the ferrite network, with the microscope, agrees with the percentage of iron phosphide rather than with the percentage of slag inclusions, which is so strikingly less in amount. The oxygen contents in the tests which were red-short are considerably lower than those in the tests which forged readily.

With regard to the nature of the *round* spots in the unetched test No. 9: as this was the only test containing spots of this nature, we merely called attention to them as differing from the regular oxide pits. Since they had no connection with the subject of our paper, we spent no time trying to learn their identity. We assumed that they probably were iron phosphide (out of solution), but we are not prepared to say so definitely and are quite willing to agree with Mr. Hibbard that they might be manganese silicate.

We were not investigating the physical effect of oxygen in steel, but wanted to learn to what extent oxygen could be retained in the steel after every effort was made to over-oxidize it. We endeavored to show, from our results, that this over-oxidation is very difficult and that in the usual process of making steel there is not much need to fear it, because the oxygen so quickly leaves the bath, and the amount retained by the steel seems to be well within safe limits.

We are glad to advise Mr. Griffin that it has been our good for-

tune to find a sample of old Bessemer rail steel, made about 20 years ago, which gave the following analysis: C, 0.600; Mn, 1.07; P, 0.060; S, 0.070; Si, 0.069; oxygen, 0.024 per cent.

While the determination of total oxygen by the Walker and Patrick method, suggested as desirable by Dr. J. W. Richards, would no doubt have a certain interest, we cannot help thinking that the results by the Ledebur-Cushman method give us much more nearly the exact information that we desire. This method, moreover, has been in successful use in many laboratories long enough to make it trustworthy, which perhaps is more than can at present be said of the vacuum method.

The statement of Dr. Weiller that the slag covering the steel bath contains a large amount of iron oxide, and that "the amount of oxygen in the metal will mostly depend upon the nature of the slag," does not agree with the results of our investigation. We have had to do with both acid and basic slags of both high and low content of iron oxide, all of which gave a low oxygen content in the steel. For example, in acid Bessemer heats, the usual content of iron oxide in the slag was from 10.50 to 11.00 per cent., with oxygen 0.030 per cent. or under in the steel, while one test (No. 17), with 0.029 per cent. of oxygen, was covered with a slag of 22 per cent. iron oxide. Test No. 19, with 0.033 per cent. of oxygen, had a slag of 34 per cent. iron oxide, and Test No. 15, with 0.017 per cent. of oxygen, had a slag consisting almost entirely of molten iron ore, with a small amount of Bessemer slag which inadvertently ran into the ladle. As to basic open-hearth practice, we have had, since our paper has gone to press, heats with slags whose iron oxide content varied from 10 to 25 per cent. (other elements remaining the same excepting silica), but the oxygen content of the steel was always around 0.020 per cent. This proves that the oxide content of the slag has no bearing on the oxygen content of the steel.

We have a private communication from Sir Robert A. Hadfield containing the following paragraph:

"Your conclusions largely confirm my own, that those statements in the past, where it has been mentioned that quite a considerable percentage of oxygen has been found in material, have been incorrect."

The Life of Crucible Steel Furnaces.

BY JOHN HOWE HALL, NEW YORK, N. Y.

(New York Meeting, October, 1918.)

THE recently announced run of three years, nine months and eleven days made by a crucible steel melting furnace of the Columbia Tool Steel Co., which is claimed as a world's record, brings forcibly to our attention the great improvements that have been made in the design, construction and operation of the Siemens regenerative crucible melting furnace since its introduction in this country. Credit for this advance should be given to the furnace designer, the manufacturer of refractories, and the operators of the furnaces, whose persistent efforts and intelligent co-operation in attacking the problem have resulted so satisfactorily.

There are three factors that determine the life of a crucible furnace, or any other furnace for that matter: Design, nature of refractories, and conditions of operation (including the nature of steel melted); and to some extent these factors are interdependent. Thus, for instance, if a crucible furnace is to be operated single turn and allowed to cool off considerably at night, in order to save fuel, the use of clay-brick melting holes may be advisable, since silica brick is not well adapted to endure repeated heating and cooling. But the total life of a clay-brick melting hole is necessarily short. Again, if local conditions such as danger of flood make necessary the building of the furnace largely above ground, so that the radiation of heat is promoted, the cooling down of the furnace over the week ends, when no steel is melted, will be considerable—and the contraction of the furnace that results tends to wrack it to pieces.

The character of the metal being melted affects the life of the furnace to a great extent. Low-carbon steels and steels made from iron and charcoal require higher temperatures and much longer melting time than high-carbon steel or steel made largely from scrap; the life of the furnace will be shorter, and the number of heats melted will be much less, when such materials are melted.

The figures available, showing the life of furnaces, are for this reason especially not strictly comparative, since the variations in the steel melted undoubtedly affect the life to a greater or less extent. Probably, however, the method of handling is as important a factor

as the material melted in determining the life of a furnace of given design and construction.

Originally the melting holes of the regenerative gas fired crucible furnace were lined with first quality firebricks. One of the leading builders of furnaces reports that when the Siemens furnace was first introduced in this country the life of a furnace was from four to six weeks; the short life was due in great part to lack of familiarity with the furnace on the part of the men in charge. As the workmen grew more skillful in handling the furnaces, this life was gradually increased to six months by retopping, and in some cases to nine months or one year, by retopping twice (by retopping is meant rebuilding the upper portions of the melting holes). These records were made a number of years ago in four or more leading Eastern shops.

A prominent manufacturer of refractory materials gives the following figures for the life of melting furnace in several different shops with melting holes lined with first quality firebrick.

Plant.	Life of Furnace in Months.
1	3 to 4
2	4
3	5 to 6
4	6
5	12

No details of design, or nature of steel being made, are given, but the average very short life of furnaces with clay-brick melting holes is quite evident.

When the melting holes are lined with silica brick, the life of the furnace is greatly increased, as the figures in the following table show:

Authority.	Plant.	Life of Furnace Mo.	Days.	Heats.	No. F's.
Furnace builder,				2,000	Average
Crucible steel maker,	A	6–15			Average
Crucible steel maker,	B	14–18.5 16 average			Average
Crucible steel maker,	C	18			(1)
	C	19			(1)
	C	15–21 18 average			Average
Manufacturer of refractories,	D	14			(1)
Manufacturer of refractories,	D	19			(1)
Manufacturer of refractories,	D	24			(1)
Manufacturer of refractories,	E	12	0	1,418	(1)
Manufacturer of refractories,	E	14	12	1,720	(1)
Manufacturer of refractories,	E	16½	0	1,897	(1)
Manufacturer of refractories,	E	17	22	2,126*	(1)
Manufacturer of refractories,	E	18	4	2,186	(1)
Manufacturer of refractories,	F	28	...	.....	(1)
Manufacturer of refractories,	F	45	...	.....	(1)
Manufacturer of refractories,	G	11	...	.....	(1)

* Heats made as follows: Projectiles, 1,325 heats; high-speed steel, 325 heats; magnet steel, 250 heats; chrome-vanadium steel, 40 heats; nickel steel, 25 heats; castings, 15 heats; carbon steel, 70 heats; miscellaneous, 76 heats; total, 2,126 heats.

These figures are not improbably duplications, in some cases; that is, no doubt the same furnace may occur in the reports of both steel maker and maker of refractories.

Compared with these records we have that of the Columbia Tool Steel Co., of a continuous run of 45 months and 11 days, for a total of 6,290 heats; the material melted is said to have been "high-grade high-carbon tool steel." The high-carbon steel of course does not require as high a heat as, for instance, cast steel of 0.25 per cent. C or alloy steels (Ni or Cr-V.) of 0.30 per cent. C, but probably the reasons for long life of this furnace are first, good design; second, good refractories; third, careful handling.

There are several features of the design of this furnace that are worthy of notice. A strong foundation was provided to guard against settling and consequent wracking of the furnace; the furnace was set with the working floor only about 31 in. above the ground level, so that the radiation of heat was minimized, tending to discourage contraction of the furnace and consequent wracking of the brick work; and the walls between checker chambers, which are frequently but 12 in. thick, were made 18 in. thick to insure against the leakage of gas into air chambers and flues that commonly occurs when the furnace grows old. The holes were lined with silica brick of standard make; breast walls, flues between checker chambers and melting holes, checkers, etc., were of first quality clay firebrick.

The Columbia Tool Steel Co. states that great pains were taken in the operation of this furnace to maintain the temperature as uniform as possible at all times, especially over the idle week-end; and that although several new tops have been put on the melting holes, and a new middle wall built in, the furnace was when shut down for general repairs, still in fairly good shape. This is a remarkable record, after such a long campaign.

The Influence of Various Elements on the Absorption of Carbon by Steel.*

BY ROBERT R. ABBOTT, CLEVELAND, OHIO.

(New York Meeting, October, 1913.)

THE influence of various elements in retarding or accelerating the absorption of carbon during the process of carburization is fairly well known. In general those elements which form carbides accelerate the absorption, while those which do not retard it. It has sometimes been wrongly stated that those elements which raise the temperature of the critical range increase the absorption, while those which lower this temperature decrease it. Silicon is a notable exception to this rule, for its influence is to raise the critical range but to lower the absorption of carbon. Some elements which do not form carbides have no apparent influence upon the absorption rate. Carbon itself retards the absorption rate.

With a view to obtaining more exact data upon the subject the following experiments were undertaken: 208 steels of different chemical composition were selected to represent most of the types of modern constructional steels. Specimens $\frac{3}{4}$ in. in diameter and $\frac{3}{4}$ in. in length were weighed to milligrams. They were then packed in the special carburizing box shown in Fig. 1, and kept in the furnace for 10 hours at a temperature of 900° C. A duplicate experiment was conducted at 1,000° C. At equal intervals of time the box was rotated in the furnace so that two complete revolutions were made in the 10 hours. This precaution was taken to neutralize as much as possible any effect of unequal heating. At the end of the 10 hours the box was allowed to cool in the furnace with the doors open. The specimens were then weighed again and the increased weight (carbon) determined. In the following table are given the analyses of the various steels, together with the increased weight of carbon expressed in milligrams.

* Contributed by the Metallurgical Laboratory of the Peerless Motor Car Co.

Mark.	Chemcal Analysis.								Increased Weight in Mg.	
	C.	Ni.	Cr.	V.	P.	S.	Mn.	Si.	900° C.	1,000° C.
Group 1, 8 per cent. Nickel.										
U4	0.298	8.00	0.45	0.07	0.011	0.021	0.42	0.112	92	169
Group 2, 6 per cent. Nickel.										
MMM	0.292	6.00	0.26	.....	0.033	0.023	0.54	0.131	94	172
Group 3, 5 per cent. Nickel.										
BU	0.365	5.00	.....	.....	0.027	0.027	0.53	0.141	103	151
OO	0.151	5.20	0.08	.....	0.014	0.012	0.58	0.137	54	187
PPP	0.072	5.14	0.27	0.02	0.017	0.011	0.29	0.127	95	205
Group 4, 4 per cent. Nickel.										
KE	0.390	4.14	.....	.....	0.013	0.030	2.5	0.254	64	154
SI	0.185	4.35	0.20	.....	0.013	0.048	0.41	0.009	104	217
Group 5, Chrome-Nickel.										
BE	0.500	5.12	1.03	.....	0.009	0.035	0.27	0.192	29	135
T5	0.275	4.97	1.35	0.06	0.010	0.020	0.37	0.192	101	208
AE	0.208	5.21	1.22	0.04	0.009	0.017	0.41	0.173	68	213
SSS	0.168	4.61	1.65	0.03	0.018	0.010	0.36	0.291	86	213
Group 6, Chrome-Nickel.										
N2	0.364	4.40	1.37	0.07	0.009	0.022	0.42	0.240	85	190
N6	0.311	4.30	1.28	.....	0.014	0.021	0.30	0.196	87	194
N1	0.130	4.05	1.00	0.07	0.013	0.019	0.32	0.158	80	200
D5	0.120	4.32	1.31	0.08	0.009	0.027	0.47	0.159	141	206
Group 7, Nickel.										
GGG	0.332	3.75	.....	.....	0.015	0.020	0.61	0.037	40	157
B3	0.289	3.82	0.06	.....	0.035	0.023	0.36	0.239	87	181
&6	0.252	3.89	.....	.....	0.018	0.021	0.60	0.249	92	193
BH	0.238	3.78	0.27	.....	0.010	0.039	0.63	0.163	118	191
C9	0.192	3.72	0.07	.....	0.019	0.023	0.66	0.218	67	202
B2	0.184	3.75	0.10	.....	0.029	0.024	0.66	0.244	103	191
Group 8, Nickel.										
DD	0.392	3.27	0.08	.....	0.008	0.025	0.64	0.300	50	156
115	0.369	3.14	0.17	.....	0.012	0.020	0.65	0.197	83	171
&7	0.367	3.61	.....	.....	0.013	0.029	0.65	0.075	65	168
D1	0.356	3.20	0.11	.....	0.017	0.038	0.77	0.221	91	165
BB	0.342	3.35	0.05	.....	0.011	0.020	0.71	0.155	95	.....
Y5	0.336	3.17	.....	.....	0.019	0.019	0.55	0.188	39	190
CC	0.324	3.40	0.05	.....	0.008	0.023	0.55	0.126	48	161
EEE	0.322	3.58	.....	.....	0.012	0.019	0.53	0.050	99	181
&&	0.318	3.25	0.13	.....	0.011	0.013	0.63	0.310	58	153
CC°	0.311	3.18	0.15	.....	0.032	0.026	0.74	0.032	97	172
B1	0.299	3.54	.....	.....	0.034	0.023	0.70	0.244	77	174
B&	0.276	3.54	0.07	.....	0.011	0.031	0.71	0.175	67	182
BOX	0.248	3.10	0.07	.....	0.025	0.027	0.70	0 251	87	172
113	0.244	3.30	0.15	.....	0.009	0.017	0.66	0.211	96	188
BO	0.231	3.71	0.07	.....	0.041	0.028	0.70	0.225	84	199
HH	0.231	3.43	0.06	.....	0.009	0.010	0.67	0.169	85	207
K	0.216	3.50	0.19	.....	0.025	0.034	0.54	0.032	154	211
88	0.193	3.47	0.05	.....	0.012	0.028	0.70	0.155	83	192
99	0.184	3.34	0.05	.....	0.011	0.021	0.70	0.159	79	189
X7	0.176	3.52	.....	.....	0.036	0.028	0.39	0.080	102	190
CA	0.168	3.59	.....	.....	0.009	0.027	0.38	0.115	95	195
D3	0.164	3.43	0.09	.....	0.009	0.029	0.40	0.177	97	211
RO	0.165	3.24	.....	.....	0.036	0.036	0.49	0.052	96	190
KK	0.151	3.46	0.06	.....	0.008	0.028	0.53	0.141	97	192
1111	0.131	3.38	0.05	.....	0.014	0.025	0.67	0.197	109	188
D6	0.097	3.27	.....	.....	0.007	0.030	0.49	0.052	120	200

Mark.	Chemical Analysis.								Increased Weight in Mg.	
	C.	Ni.	Cr.	V.	P.	S.	Mn.	Si.	900° C.	1,000° C.
Group 9, Chrome-Nickel.										
CT	0.498	3.43	0.30	0.08	0.008	0.033	0.57	0.178	51	165
AK	0.459	2.63	0.61	.....	0.013	0.012	0.47	0.272	68	168
XO	0.406	3.35	0.77	.....	0.006	0.013	0.45	0.253	79	.....
SS	0.402	3 03	0.37	.....	0.017	0.023	0.74	0.202	58	157
DDD	0.362	3.27	0.54	.....	0.012	0.015	0.53	0.112	85	188
JJ	0.330	2.92	0.47	.....	0.023	0.038	0.63	0.282	69	184
CU	0.295	3.55	0.22	0.03	0.009	0.031	0.62	0.047	53	201
N4	0.276	2.75	0.36	.....	0.009	0.019	0.45	0.235	92	197
R7	0.281	3.27	0.68	.....	0.039	0.030	0.30	0.177	105	193
VVV	0.267	2.81	0.31	.....	0.020	0.017	0.45	0.216	78	191
N3	0.254	2.62	0.29	.....	0.013	0.017	0.60	0.302	104	199
S6	0.247	3.07	0.20	.....	0.036	0.033	0.53	0.008	80	208
AM	0.242	3.11	0.67	.....	0.019	0.012	0.37	0.324	111	176
NNN	0.223	2.78	0.24	.....	0.029	0.026	0.54	0.126	60	185
LL	0.220	2.81	0.65	.....	0.023	0.040	0.37	0.179	98	209
T7	0.224	3.33	0.64	.....	0.035	0.017	0.50	0.061	88	211
KA	0.129	3.07	0.39	.....	0.012	0.029	0.31	0.100	110	206
MO	0.096	2.75	0.50	.....	0.016	0.021	0.39	0.110	93	223
Group 10, Chrome-Nickel.										
C1	0.439	3.16	1.12	.....	0.025	0.027	0.48	0.230	42	179
C8	0.402	3.45	1.20	0.20	0.017	0.027	0.74	0.244	121	226
RRR	0.345	3.34	1.04	.....	0.024	0.018	0.47	0.249	112	178
11	0.323	3.40	1.35	0.03	0.009	0.018	0.19	0.126	110	220
YYY	0.312	3.50	0.99	.....	0.027	0.017	0.43	0.206	74	187
D4	0.288	3.91	1.42	.....	0.012	0.023	0.42	0.197	66	195
UUU	0.229	3.27	0.94	.....	0.018	0.016	0.40	0.216	58	209
KB	0.197	3.51	1.84	0.07	0.034	0.038	0.22	0.153	92	218
C3	0.196	3.52	1.11	.....	0.023	0.028	0.41	0.225	112	218
CV	0.184	3.26	1.29	0.03	0.023	0.029	0.73	0.145	92	245
D2	0.090	2.94	0.85	.....	0.009	0.038	0.55	0.139	108	244
Group 11, Chrome-Nickel.										
E2	0.645	1.66	0.46	0.15	0.023	0.037	0.72	0.115	29	155
&1	0.567	0.93	0.64	.....	0.018	0.032	0.60	0.235	83	169
E5	0.555	2.36	0.41	.....	0.027	0.039	0.52	0.201	61	150
H7	0.546	1.57	0.51	.....	0.010	0.022	0.46	0.060	83	188
E4	0.496	1.27	0.38	0.38	0.029	0.034	0.35	0.182	86	190
JJJ	0.401	1.16	0.23	.....	0.011	0.038	0.50	0.098	71	198
A7	0.382	1.60	0.49	.....	0.004	0.038	0.59	0.052	105	200
F	0.370	1.51	0.26	.....	0.011	0.039	0.60	0.118	82	182
C8	0.340	1.20	0.53	.....	0.006	0.024	0.51	0.172	70	201
N	0 326	0.99	0.11	.....	0.008	0.047	0.58	0.042	95	190
YY	0.293	1.08	0.18	.....	0.008	0.035	0.39	0.070	110	214
R5	0.274	1.20	0.41	0.08	0.007	0.032	0.56	0.057	123	205
R4	0.271	1.25	0.35	0.96	0.013	0.038	0.56	0.115	77	205
R6	0.258	1.25	0.3	0.86	0.007	0.033	0.57	0.159	106	189
A6	0.198	1.77	0.46	.....	0.007	0.032	0.40	0.084	97	189
C7	0.147	1.28	0.37	.....	0.005	0.025	0.34	0.134	74	201
A1	0.083	1.51	0.30	.....	0.004	0.037	0.44	0 041	116	219

Mark.	Chemical Analysis.								Increased Weight in Mg.	
	C.	Ni.	Cr.	V.	P.	S.	Mn.	Si.	900° C.	1,000° C.
Group 12, Chrome-Nickel.										
C2	0.552	1.53	1.14	.....	0.020	0.026	0.41	0.225	65	196
BY	0.504	1.57	1.19	0.03	0.009	0.032	0.50	0.180	109	183
M	0.503	1.93	0.99	.....	0.008	0.016	0.52	0.127	110	175
GG	0.442	2.12	1.06	.....	0.009	0.020	0.19	0.197	64	188
S7	0.415	1.59	1.11	.....	0.018	0.018	0.36	0.094	106	204
E7	0.394	1.25	1.12	.....	0.027	0.034	0.35	0.365	149	186
K2	0.377	1.67	1.31	.....	0.023	0.037	0.21	0.163	98	211
D	0.365	2.04	0.99	.....	0.008	0.019	0.15	0.103	106	196
A8	0.363	1.55	0.70	.....	0.004	0.036	0.53	0.112	113	196
BT	0.307	2.03	1.01	.....	0.032	0.034	0.53	0.150	98	224
A4	0.308	1.35	0.72	.....	0.005	0.036	0.67	0.070	89	199
C4	0.251	2.33	1.24	0.06	0.032	0.028	0.43	0.225	128	218
FF	0.226	2.06	0.97	.....	0.014	0.024	0.11	0.075	89	228
M6	0.228	1.05	0.99	0.22	0.023	0.025	0.32	0.149	124	229
S3	0.213	1.79	1.05	.....	0.016	0.020	0.42	0.092	114	207
CH	0.197	1.44	0.79	.....	0.029	0.033	0.23	0.245	87	215
FFF	0.186	2.03	0.66	.....	0.012	0.015	0.53	0.122	62	191
K8	0.178	1.28	1.59	.....	0.022	0.044	0.27	0.053	110	218
Group 13, Nickel.										
YO	0.660	0.25	.....	.....	0.027	0.019	0.43	0.235	90	146
BX	0.454	0.27	.....	.....	0.022	0.033	0.59	0.170	87	173
A	0.220	0.23	.....	.....	0.006	0.039	0.42	0.056	94	231
T9	0.190	0.22	.....	.....	0.023	0.020	0.45	0.042	102	221
Group 14, Chrome-Nickel.										
K7	0.559	0.20	0.07	.....	0.012	0.031	0.37	0.202	72	179
CD	0.530	0.26	0.11	.....	0.036	0.029	0.69	0.140	77	173
E6	0.515	0.56	0.14	.....	0.034	0.037	0.48	0.159	91	161
K4	0.443	0.27	0.22	.....	0.032	0.028	1.57	0.115	81	183
KKK	0.428	0.53	0.17	.....	0.012	0.030	0.49	0.075	92	172
K1	0.373	0.40	0.18	.....	0.017	0.032	0.48	0.230	85	186
CP	0.300	0.25	0.10	.....	0.015	0.025	0.62	0.112	104	209
C&	0.231	0.30	0.06	.....	0.012	0.034	0.60	0.169	84	195
KC	0.210	0.27	0.19	.....	0.048	0.046	0.47	0.244	150	203
U3	0.201	0.50	0.20	.....	0.009	0.058	0.47	0.004	114	207
L	0.170	0.52	0.13	.....	0.008	0.042	0.52	0.018	144	255
KD	0.150	0.16	0.05	.....	0.015	0.039	0.49	0.225	84	207
C1	0,127	0.13	0.07	.....	0.070	0.072	0.60	0.125	108	217
Group 15, Chrome-Nickel.										
HHH	0.703	0.24	0.47	.....	0.011	0.015	0.54	0.094	80	177

Mark.	Chemical Analysis.								Increased Weight in Mg.	
	C.	Ni.	Cr.	V.	P.	S.	Mn.	Si.	900° C.	1,000° C.
Group 16, Carbon.										
&5	1.21	.....	.....	.....	0.007	0.032	0.32	0.163	72	73
A&	1.03	.....	.....	.....	0.011	0.032	0.37	0.141	55	79
BM	0.667	.....	.....	.....	0.009	0.018	0.77	0.168	59	152
CE	0.593	0.06	.....	.....	0.050	0.029	0.65	0.315	67	149
AH	0.447	0.11	.....	.....	0.014	0.017	0.60	0.206	102	200
E	0.414	.....	.....	.....	0.014	0.038	0.51	0.052	78	194
QQ	0.389	.....	.....	.....	0.012	0.032	0.57	0.159	85	216
X6	0.341	.....	.....	.....	0.047	0.025	1.61	0.009	86	223
B8	0.344	.....	0.06	.....	0.014	0.029	0.54	0.030	93	217
X1	0.305	.....	.....	.....	0.041	0.018	1.61	0.014	96	213
H8	0.300	.....	0.04	.....	0.039	0.024	1.60	0.098	126	225
EE	0.294	.....	0.04	.....	0.014	0.037	0.42	0.018	112	208
LLL	0.294	.....	.....	.....	0.023	0.040	0.44	0.037	94	190
U6	0.283	.....	0.08	.....	0.016	0.018	1.60	0.023	109	201
T3	0.276	.....	0.05	.....	0.058	0.029	1.35	0.173	86	201
D	0.267	.....	.....	.....	0.009	0.031	0.37	0.022	103	200
B9	0.244	.....	.....	.....	0.104	0.053	0.52	0.056	94	204
Y7	0.240	.....	0.05	.....	0.018	0.018	0.38	0.056	104	188
AX	0.228	.....	.....	.....	0.077	0.072	0.80	0.018	89	209
Y1	0.216	.....	.....	.....	0.013	0.019	0.30	0.084	113	.....
X4	0.219	.....	.....	.....	0.027	0.031	0.46	0.005	121	242
U7	0.217	.....	0.06	.....	0.029	0.022	0.72	0.051	97	229
AU	0.205	.....	.....	.....	0.010	0.038	0.54	0.014	105	216
X5	0.196	.....	.....	.....	0.023	0.027	0.71	0.008	116	220
B7	0.202	.....	.....	.....	0.010	0.023	0.51	0.030	113	218
M8	0.195	.....	0.06	.....	0.016	0.046	0.59	0.225	105	207
X3	0.186	.....	.....	.....	0.016	0.022	0.47	0.009	106	210
BV	0.166	0.06	0.04	.....	0.049	0.049	0.63	0.042	103	235
U5	0.167	.....	.....	.....	0.020	0.021	0.44	0.005	110	213
E1	0.146	.....	.....	.....	0.018	0.044	0.87	0.057	104	251
M2	0.139	.....	.....	0.07	0.013	0.033	0.16	0.225	130	227
TTT	0.111	.....	.....	.....	0.018	0.135	0.59	0.025	115	227
T1	0.077	.....	0.07	.....	0.013	0.019	0.60	0.075	91	235
OOO	0.022	.....	0.06	.....	0.012	0.022	0.10	0.025	95	215
Group 17, Chrome-Vanadium.										
H5	0.676	.....	0.75	0.11	0.014	0.033	0.67	0.197	108	170
V	0.569	.....	0.69	0.22	0.008	0.019	0.37	0.202	84	191
S2	0.479	.....	0.71	0.18	0.009	0.027	0.79	0.173	120	231
G	0.318	0.12	0.74	0.21	0.015	0.023	0.42	0.165	117	221
CX	0.266	0.24	0.62	0.17	0.032	0.037	0.26	0.108	129	222
VV	0.129	.....	0.33	0.10	0.008	0.030	0.33	0.108	116	214
BBB	0.127	.....	0.73	0.16	0.012	0.018	0.46	0.051	142	247

Mark.	Chemical Analysis.								Increased Weight in Mg.	
	C.	Ni.	Cr.	V.	P.	S.	Mn.	Si.	900° C.	1,000° C.
Group 18. Chrome-Vanadium.										
B4	1.06	.....	0.95	0.10	0.022	0.023	0.36	0.220	94	143
X2	0.882	.....	1.14	0.23	0.041	0.022	0.50	0.094	109	196
M3	0.708	.....	0.94	0.21	0.026	0.022	0.47	0.062	111	190
M5	0.617	.....	1.19	0.23	0.031	0.025	0.76	0.153	90	207
J	0.612	.....	0.82	0.19	0.009	0.021	0.39	0.145	85	210
AO	0.515	.....	1.01	0.19	0.006	0.017	0.70	0.286	123	200
4	0.520	.....	1.16	0.15	0.011	0.027	0.47	0.162	108	220
XX	0.491	.....	1.04	0.09	0.011	0.020	0.92	0.155	110	206
Y	0.474	0.10	1.19	0.18	0.009	0.014	0.80	0.159	134	244
I	0.457	.....	1.17	0.14	0.009	0.025	0.48	0.198	113	236
B5	0.453	.....	1.04	0.18	0.025	0.026	0.98	0.296	128	233
CO	0.406	0.34	1.23	0.20	0.050	0.035	0.32	0.174	126	217
O	0.411	.....	1.05	0.10	0.034	0.027	0.66	0.136	114	219
&	0.397	0.13	1.26	0.19	0.008	0.016	0.82	0.169	126	257
3	0.374	.....	1.04	0.15	0.009	0.029	0.62	0.148	145	230
Z	0.339	0.12	0.97	0.19	0.008	0.013	0.48	0.145	137	231
UU	0.338	.....	1.00	0.13	0.011	0.038	0.57	0.094	118	230
Y4	0.320	.....	0.94	0.19	0.020	0.027	0.62	0.094	120	230
AAA	0.310	.....	0.86	0.27	0.012	0.021	0.83	0.085	129	230
C5	0.204	.....	1.03	0.15	0.012	0.027	0.52	0.244	157	237
S	0.161	0.09	0.74	0.19	0.012	0.025	0.32	0.178	142	247
Group 19, Chrome-Vanadium.										
K6	0.754	.....	1.95	0.51	0.034	0.023	0.23	0.316	127	212
K5	0.382	0.26	2.09	0.06	0.010	0.037	0.19	0.187	121	227
Group 20, Chrome.										
SO	0.957	0.22	1.23	.....	0.022	0.016	0.26	0.188	103	150
S5	0.906	.....	0.92	.....	0.014	0.028	0.35	0.089	79	141
Group 21, Chrome.										
U1	0.673	.....	0.11	.....	0.020	0.009	0.66	0.267	76	164
U2	0.490	.....	0.12	.....	0.020	0.010	0.44	0.310	94	192
Group 22, Chrome-Silicon.										
HI	0.402	.....	0.92	.....	0.007	0.017	0.78	0.537	108	195
Group 23, Silicon.										
Q	0.672	.....	.....	.....	0.047	0.044	0.48	1.18	18	101
AD	0.617	0.14	.....	.....	0.013	0.009	0.54	1.60	14	109
QQQ	0.494	.....	0.05	.....	0.044	0.036	1.18	0.677	65	137
R	0.365	.....	.....	.....	0.025	0.035	0.57	1.27	77	49
RS	0.371	0.21	.....	.....	0.063	0.042	0.57	2.16	66	107
B6	0.307	0.53	.....	.....	0.049	0.026	0.48	2.39	79	125
B	0.225	.....	0.05	.....	0.028	0.036	0.54	0.972	70	126
2	0.222	.....	0.02	.....	0.009	0.047	0.58	0.996	82	.....

In order to correlate the value of the various elements in their carburizing effect a mathematical investigation was undertaken. From each steel an equation can be formed connecting the increased weight with the percentage of the various elements present. This will give us as many equations as we have steels, and the problem then resolves itself into an equalization of the errors of observation, analysis and weight entering into each equation by a consideration of the remaining ones. This can be done by the method of least squares. All the elements were first considered, and much time was spent in solving this problem. This proved a mathematical success, but a flat failure from a practical standpoint.

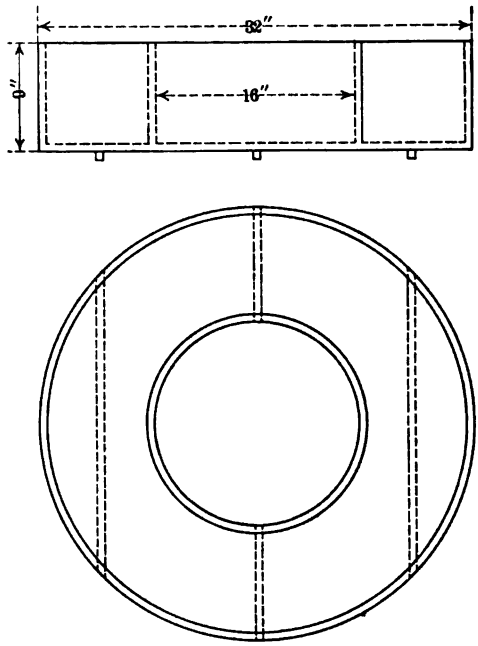


FIG. 1.—CARBURIZING BOX.

By the method of least squares a problem containing more equations than unknowns is solved by reducing the excess equations to a number equal to the unknown quantities. These final equations are known as normal equations; they are solved by the ordinary processes of algebra. In this case they were solved by the method of determinants. There are many difficulties in the way of the solution of such a problem, some of which are as follows:

(a) The correct formation of the original equations.

(b) The presence of elements occurring in nearly constant amounts.

(c) The presence of elements having little or no influence on the final result.

The formation of the original equation is the most important source of error. For example, the amount of carbon absorbed may not be a straight-line function of the percentage of some one element. Their relationship may be expressed by a curve or a broken line or a combination of the two. Again, element A in the absence of element B may be a straight-line function throughout its range, while in the presence of B it may not be. As an example of this it might easily be conceived that chromium existing as a carbide of chromium would have a different effect from chromium in solution with the iron. In all the mathematical work undertaken in this investigation it was assumed that the relations were represented by straight-line functions. Apparently some of the results indicate that this is an error.

In a solution of a problem by the method of least squares the final answer obtained is the most probable considering all the variables. Manifestly an element having no effect on the final result or one occurring in constant quantity should not have the errors of measurement of the other variables distributed over it. This is one source of error in the present problem. For this reason after the failure of the first solution sulphur, manganese, phosphorus, and silicon were left out of the calculation and the high-silicon steels were not used in further calculation involving more than two variables.

An equation containing three unknowns was formed from steels of groups 1, 2, 3, 4, 7, 8, 13, 16, omitting those containing any appreciable amounts of chromium and using high-temperature weights only. This equation was placed in the intercept form thus:

$$\frac{C}{a} + \frac{N}{b} + \frac{W}{d} = 1 \qquad (1)$$

in which C, N, and W represent the percentage of carbon, nickel, and increase in weight, respectively, while a, b, and d are the intercepts of the plane representing the equation upon the corresponding axes. The 76 separate equations thus formed were reduced to four normal equations, which, in turn, were solved, giving the following values:

$$a = \frac{C^2N^2W^2 + 2(CN)(NW)(CW) - (CW)^2N^2 - (CN)^2W^2 - (NW)^2C^2}{CN^2W^2 + (CN)(NW)W + N(NW)(CW) - WN^2(CW) - (CN)NW^2 - C(NW)^2}$$

$$b = \frac{C^2N^2W^2 + 2(CN)(NW)(CW) - (CW)^2N^2 - (CN)^2W^2 - (NW)^2C^2}{C^2NW^2 + C(NW)(CW) + (CW)(CN)W - N(CW)^2 - CW^2(CN) - C^2W(NW)}$$

$$c = \frac{C^2N^2W^2 + 2(CN)(NW)(CW) - (CW)^2N^2 - (CN)^2W^2 - (NW)^2C^2}{C^2N^2W + N(CN)(CW) + C(CN)(WN) - CN^2(CW) - (CN)^2W - C^2N(NW)}$$

In the above values of a, b, and c, the letters C, N, and W represent the summation of the carbon, nickel, and weight values of the total number of steels and not the values of the single steels as is represented in equation (1).

The equation determined from the above values is as follows:

$$\frac{\text{Carbon}}{1.13} + \frac{\text{Nickel}}{39.5} + \frac{\text{Weight}}{265} = 1 \qquad (2)$$

These values are very reasonable and have the following meaning: 1.13 per cent. of carbon will reduce the efficiency of carbon absorption the same amount as 39.5 per cent. of nickel, or, stated in a better form, 0.01 per cent. carbon is equivalent to 0.35 per cent. nickel. To make use of it in a practical problem a 3.5 per cent. nickel steel of 0.20 per cent. carbon will absorb carbon as rapidly and in the same amounts as a 0.30 per cent. carbon steel without nickel.

An attempt was then made to form an equation connecting carbon, nickel, and chrome with weight by using all the analyses except groups 22 and 23. The equations so determined were as follows:

For 900°:

$$\frac{\text{Carbon}}{0.56} + \frac{\text{Nickel}}{6.85} - \frac{\text{Chrome}}{53} + \frac{\text{Weight}}{100} = 1 \qquad (3)$$

For 1,000°:

$$\frac{\text{Carbon}}{0.56} + \frac{\text{Nickel}}{6.95} - \frac{\text{Chrome}}{30} + \frac{\text{Weight}}{199} = 1 \qquad (4)$$

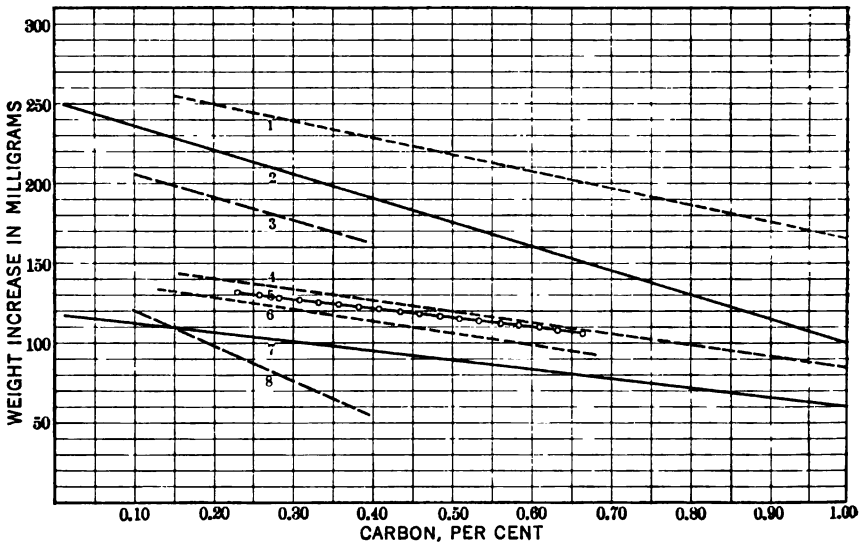
These are interesting; but while mathematically correct they are, from a practical standpoint, hard to interpret. They would indicate, for example, that a pure iron would absorb 199 mg. of carbon at 1,000° in 10 hours, and 100 mg. at 900°; as a matter of fact this is too low a value.

Equations were then worked out for the different alloy groups, using two unknowns, carbon and weight, and assuming the amounts of alloys to be constants. These results are plotted in Figs. 2 and 3, which are nearly self-explanatory.

Consider Fig. 2: Line 1 is the equation of group 18, the steels of which have an average chromium content of 1.04 per cent., and a vanadium content of 0.18 per cent. Line 2 is the equation of group

16. Line 3 of group 8, with an average nickel content of 3.38 per cent. (chrome 0.09 per cent.). Line 5 is group 23, with an average silicon content of 1.65 per cent. (Omit R.) It will be noted that the slope of the lines (except for silicon) is practically the same. In effect this means that the same increase in carbon in the original steels will lower the absorption the same amount, whether the steel be plain carbon, chrome-vanadium, or nickel.

Consider a 0.20 per cent. carbon steel: 1.00 per cent. chromium and 0.18 per cent. vanadium increase the absorption rate about 13 per cent. over a plain carbon steel, while 3.38 per cent. nickel decreases



1. Chrome-Vanadium. Carburized at 1,000° C.
2. Carbon. Carburized at 1,000° C.
3. Nickel. Carburized at 1,000° C.
4. High Chrome-Vanadium. Carburized at 900° C.
5. Silicon. Carburized at 1,000° C.
6. Low Chrome-Vanadium. Carburized at 900° C.
7. Carbon. Carburized at 900° C.
8. Nickel. Carburized at 900° C.

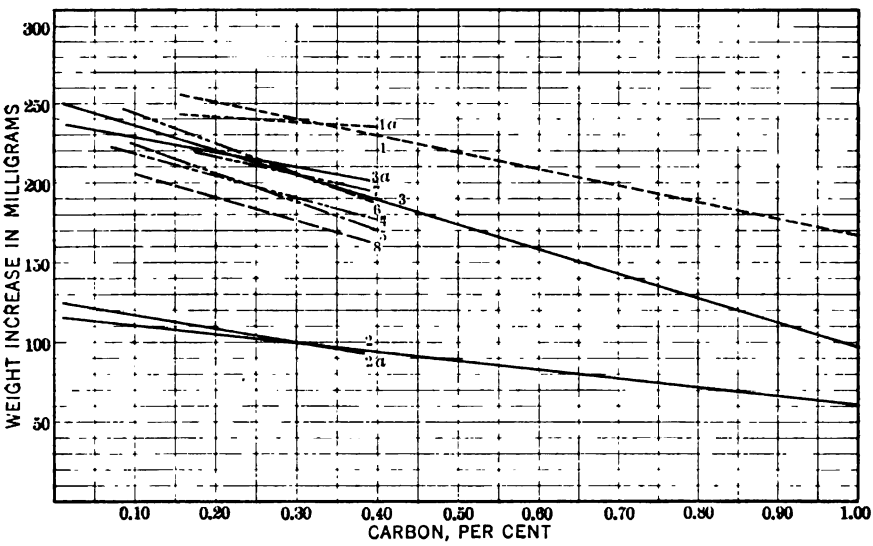
FIG. 2.—RESULTS OF CARBURIZING VARIOUS STEELS AT DIFFERENT TEMPERATURES.

this rate about the same amount; 1.65 per cent. silicon decreases it about 40 per cent.

Lines 4, 7, and 8 represent the same groups of steels as lines 1, 2, and 3, but carburized at 900°. Line 6 represents group 17, with an average chrome content of 0.62 per cent. and 0.16 per cent. vanadium.

It will be noted that with the low temperature an increase in carbon in the original steel decreases the rate of absorption more rapidly than at the high temperature.

In Fig. 3 some of the above lines are repeated and there are added lines for the chrome-nickel steels. Lines 1*a* and 3*a* were developed from the same groups of steels as lines 1 and 3, using, however, only those steels containing less than 0.40 per cent. carbon. The changed slope of these lines indicates that for a low carbon content originally in the steel, an increase in it is of less importance in influencing the



1. Chrome-Vanadium (Cr = 1.04, V = 0.18 average). All Carbons.
1*a*. Chrome-Vanadium (Cr = 0.98, V = 0.18 average). Carbon < 0.40 per cent.
3. Carbon. All Carbons.
3*a*. Carbon. Carbon < 0.40 per cent.
4. Chrome-Nickel (Cr = 0.38, Ni = 1.39 average).
5. Chrome-Nickel (Cr = 0.46, Ni = 3.03 average).
6. Chrome-Nickel (Cr = 1.20, Ni = 3.39 average).
7. Chrome-Nickel (Cr = 1.04, Ni = 1.70 average).
8. Nickel (Cr = 0.09, Ni = 3.38 average).
2. Carbon. All Carbons. Carburized at 900° C.
2*a*. Carbon. Carbon < 0.40 per cent. Carburized at 900° C.

FIG. 3.—RESULTS OF CARBURIZING VARIOUS STEELS AT 1,000° C.

rate of carburization than for a high initial content. It also indicates the probability of the carbon-weight equation being a curve. Line 4 represents group 11; line 5, group 9; line 6, group 10; line 7, group 12; line 8, group 8.

In Fig. 4 are plotted two manganese lines developed from steels X6, X1, H8, U6, T3, Y7, AX, X4, U7, AU, X5, B7, M8, X3, and M2. A correction was first made on these steels for carbon contents by reducing all to 0.20 per cent. carbon. At the lower temperature

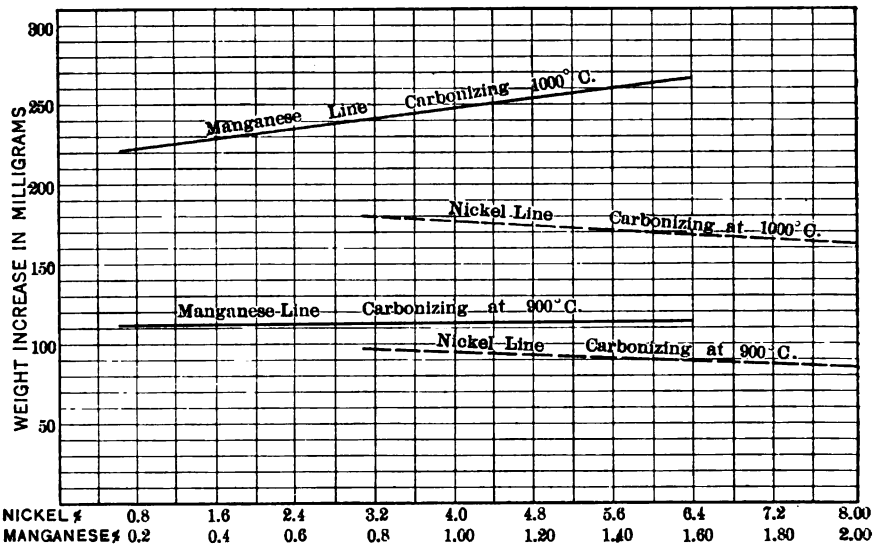


FIG. 4.—EFFECT OF MANGANESE AND NICKEL ON THE ABSORPTION OF CARBON BY STEEL.

manganese, within the range taken, apparently has no effect, while at the higher temperature it increases the carbon absorption.

In a similar manner nickel lines were plotted using steels U4, MMM, B3, CC°, B1.

DISCUSSION.

ALBERT SAUVEUR, Cambridge, Mass.:—Mr. Abbott has certainly made a strenuous effort to ascertain the influence of the various elements on the absorption of carbon by steel—a subject of great importance to those dealing with the case hardening of that metal. Because of the extensive and careful character of Mr. Abbott's experiments, one is all the more disappointed that his results, as he himself recognizes, are not more satisfactory; for, indeed they leave us just about where we were,—still entertaining uncertain views in regard to the carbon-absorbing power of various alloy steels. It might be asked whether Mr. Abbott's method of estimating the amount of carbon absorbed—namely, by weighing the specimens before and after the

carburizing treatment—was the best one to select. Are not serious sources of error inherent to such procedure because of the difficulty of satisfactorily removing the carburizing material attached to the specimen? The method of least squares has certainly failed to solve the problem satisfactorily. By manipulating Mr. Abbott's results in a different manner I have obtained the following expression:

$$P = 100 - 50\,C - 7\,Ni + 20\,Cr,$$

in which P represents the carbon-absorbing power; C, the percentage of carbon; Ni, that of nickel; and Cr, that of chromium. It is assumed that the carbon-absorbing power of pure iron, or what might be called its carburizability, is represented by 100. Applying the formula, for instance, to a steel containing 0.15 per cent. C, 3 per cent. nickel, and 0.50 per cent. chromium, its carbon-absorbing power would be

$$100 - 50 \times 0.15 - 7 \times 3 + 20 \times 0.50 = 81.50;$$

that is, 81.50 per cent. of the carbon-absorbing power of pure iron.

The formula was worked out by grouping together the carbon steels and figuring, roughly, the average retarding influence of carbon, which was found to be in the vicinity of 50 per cent. This, however, is true only of steels containing less than 0.50 per cent. carbon. In more highly carburized steels the retarding influence appears to be considerably greater. Similarly, the nickel steels containing nearly the same amount of carbon were examined and the retarding influence of nickel calculated, when it was found to be about 7 per cent. Finally, chromium, in a similar manner, was found to have a stimulating influence of 20 per cent. These values, however, can be only roughly approximate, because of the inconsistency of some of the figures. As a matter of fact, the results which appeared abnormally high or low were rejected. The formula, therefore, should be used cautiously if, indeed, it should be used at all. Were it possible to obtain accurate values for the influences of carbon, nickel, chromium, and other elements, a formula of the type here given would be of ready application and of great assistance.

Dr. John A. Mathews*, Syracuse, N. Y. (communication to the Secretary†):—I have very little to offer in reference to this paper. It represents certainly a great deal of very careful work in the analysis and tests of specimens and confirms, I think, the general opinion as

* Non-member.

† Received Nov. 12, 1913.

to the relative merits of different alloy steels in regard to the absorption of carbon.

In the paper given last year before the Mechanical Engineers by Mr. Lothrop, the same ratio was shown between the three types of steel: chrome-vanadium, carbon, and nickel. In that paper, however, this relation was indicated qualitatively and not quantitatively, as Mr. Abbott has attempted to do. Chromium steel shows the greatest tendency to absorb carbon, which I think is an advantage; that is, you can run your case hardening a little quicker with that class of steel. In chromium steels there is scarcely any tendency at all to the formation of lamellar pearlite, which is apt to remain after the steel is hardened. Of course, nickel and carbon steels with lamellar pearlite may be refined fairly well by a single quenching, and altogether if you give them a double quenching, but there are few plants that are regularly using the double quenching of case-hardened carbon or nickel steels; and there is a great deal of trouble in practice due to flaking, which follows either slag inclosures in the steel or the lamellar pearlite which has not been refined by a single quenching.

I wish Mr. Abbott would explain a little more clearly why he groups the steels in the way he does. He has several groups labeled nickel steel and several labeled chrome-nickel. It is not always plain on what he bases his division of the various steels into the groups that he gives.

The experimental data presented are of extreme value and very instructive to a great many people who are doing commercial case hardening. The value would have been fully as great had the mathematical part of the paper been omitted; yet I believe that if Mr. Abbott had been working with a definite series of experimental steels, made purposely of extreme uniformity and containing but one variable, he might have arrived at some formula which would define the influence of various elements in regard to the absorption of carbon. Making use of such a miscellaneous collection of alloy steels, furnished by different makers and manufactured by different processes, and far from uniform with respect to any one element in their composition, the results he has derived by his mathematical labors are of little value.

ROBERT R. ABBOTT, Cleveland, Ohio (communication to the Secretary *):—Dr. Mathews inquires regarding the grouping of the different steels. Since the method of least squares does not give satisfac-

* Received Nov. 14, 1913.

tory results as applied to this problem, when more than three unknowns are present, it became necessary to group the different steels into classes of approximately the same per cent. of nickel and chromium so that in these different groups these two elements could be considered as constant, leaving two variables only with which to work—namely, carbon, and increased weight. For example, groups 6 and 9 are both labeled chrome-nickel. Group 6 contains those steels having a nickel content of from 4 to 4.5 per cent. and a chrome content of from 1 to 1.5 per cent., while in group 9 the nickel runs about 3 per cent. and the chrome about 0.5 per cent.

Shock Tests of Cast Steel.

BY JOHN H. HALL, NEW YORK, N. Y.

(New York Meeting, October, 1913.)

THE Frémont test for measuring the energy consumed in breaking a notched bar of steel is not so well known in this country as it deserves to be. The test specimen used in this test is about $\frac{3}{8}$ by $\frac{1}{4}$ by about $1\frac{1}{2}$ in., notched with a hack saw on the lower side to cause the piece to break at a fixed point. A weight of 10 kg. falling 4 m., and provided with a knife or chisel which strikes the specimen, breaks the test piece by impact. The residual force in the falling weight after it has broken the specimen is recorded by a suitable device, and the difference between the total energy and the residual energy of the weight (in kilogrammeters) is the force consumed in breaking the specimen.

An experience of several years with the Frémont testing machine has shown its great value in readily detecting brittleness not revealed by tensile testing or slow bending testing. In this paper it is proposed to discuss (1) the supersensitiveness of the test as applied to cast steel and (2) its value in revealing the effect of heat treatment under certain conditions.

In testing coupons cut from castings heat treated by quenching and annealing, attention was drawn to the wide variations in the value of the Frémont test of specimens cut from different parts of the coupon. The latter was of the form and dimensions shown in Fig. 1, and Frémont test pieces were cut from two parts of this coupon, marked *A* and *B* in that figure. The results of the tests are shown below, tests *A* and *B* in each instance being from the same coupon:

Coupon.	*A.* Kilogram-meters.	*B.* Kilogram-meters.	Coupon.	*A.* Kilogram-meters.	*B.* Kilogram-meters.
1,	12.0	12.5	9,	27.5	32.5
2,	11.0	32.5	10,	17.5	31.0
3,	11.5	30.0	11,	14.5	31.0
4,	20.0	33.0	12,	15.0	27.5
5,	17.5	33.0	13,	19.0	30.0
6,	17.5	20.0	14,	17.5	25.5
7,	17.5	13.0	15,	15.0	30.0
8,	10.0	30.0			

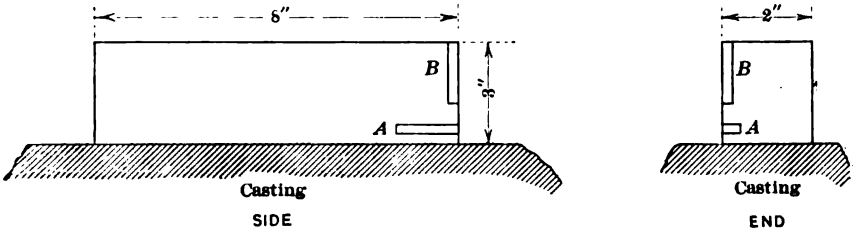


FIG. 1.

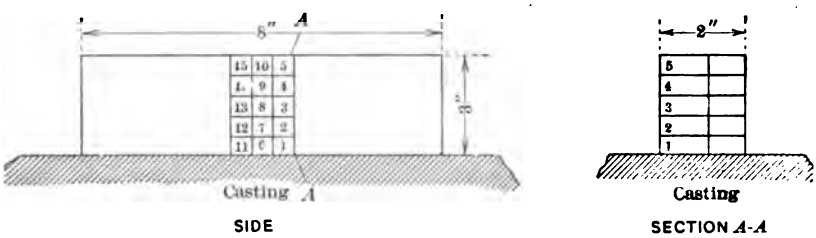


FIG. 2.

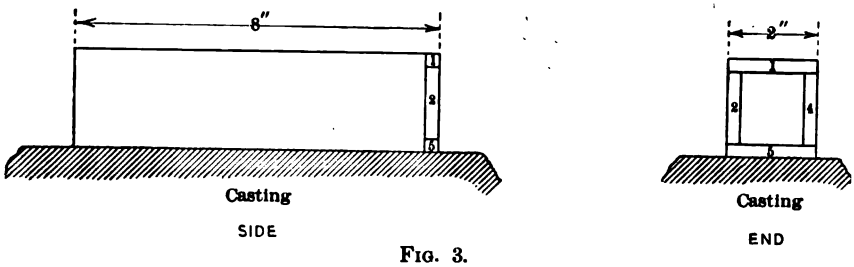


FIG. 3.

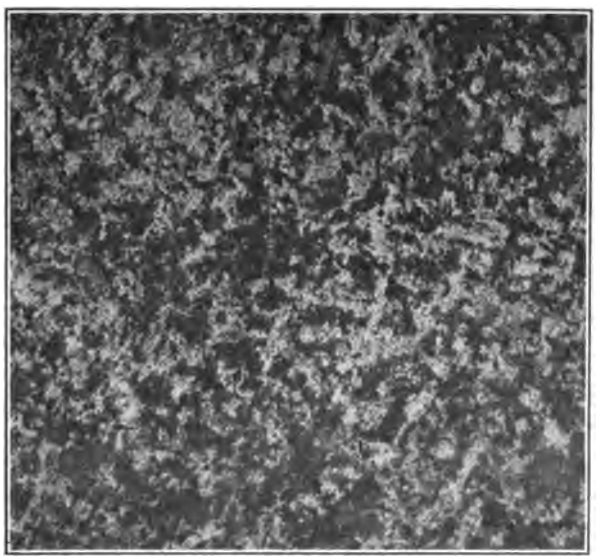


FIG. 4. × 60.

Evidently the toughness of the steel at the outside of the coupon is much greater than that of the steel from the part next the casting. In order to test this point, test pieces were cut from a single coupon, as shown in Fig. 2, and tested, with the following results:

Test.	Kilogram-meters.	Test.	Kilogram-meters.	Test.	Kilogram-meters.
1,	18.0	6,	10.0	11,	19.0
2,	18.0	7,	17.0	12,	19.0
3,	19.0	8,	19.0	13,	24.0
4,	31.0	9,	33.0	14,	24.0
5,	33.0	10,	31.0	15,	32.0

Four pieces cut from the end of the same coupon, as shown in Fig. 3, were tested, with the following results:

Test.	Kilogrammeters.	Test.	Kilogrammeters.
1,	32.0	4,	32.0
2,	30.0	5,	10.0

It is evident from these figures that the Frémont test is capable of detecting variations in the toughness of steel due to very slight variations in the rate of cooling in the first quench, inasmuch as, though no difference in microstructure can be detected in specimens from the inside and outside of the coupons, yet a marked superiority in toughness is shown by the outer $\frac{1}{2}$ in. or so of the metal. For this reason, the test as applied to cast steel in heavy sections is rather too delicate.

In spite of this drawback, the shock test has a great value in revealing the brittleness inherent in cast steels which have a coarse microstructure. Two small castings, the microstructures of which are shown in Figs. 4 and 5, were analyzed, with results as follows:

No.	C. Per Cent.	Si. Per Cent.	Mn. Per Cent.	S. Per Cent.	P. Per Cent.
4.	0.18	0.41	0.93	0.065	0.052
5.	0.20	0.45	1.02	0.057	0.049

Frémont tests of these castings gave:

Test.	Kilogrammeters.	Test.	Kilogrammeters.
4,	5.0	5,	7.0

To show the properties of similar steel quenched and annealed (heated to 900° C. for 5 hr. and quenched, reheated to 680° C. for 6.5 hr. and air cooled), the following test is given of a coupon from a casting of similar analysis (C, 0.21; Mn, 1.17 per cent.) whose microstructure is shown in Fig. 6:

Tensile Strength. Pounds per sq. in.	Elastic Limit. Pounds per sq. in.	Extension in 2 in. Per Cent.	Contraction. Per Cent.	Fracture.
91,900	71,650	20.37	50.25	Silky.

Bend ½ by 1 in. on 1-in. Mandrel.	Frémont Kgm.
180°	21.0

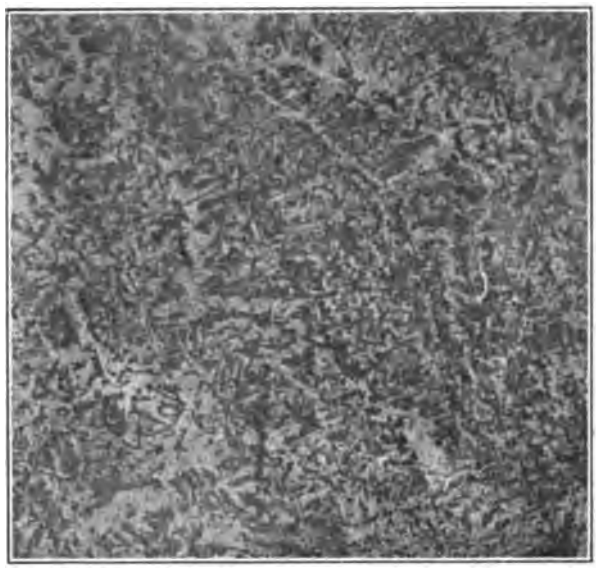


FIG. 5. × 60.

FIG. 6. × 60.

The casting whose microstructure is shown in Fig. 5 was afterward sawed into two approximately equal parts, one-half set aside for test, the other half heat treated in the same manner as the coupon which gave the tests just quoted (heated to 900° C. for 4 hr. and quenched, then reheated to 680° C. for 8 hr. and air cooled). The pieces were

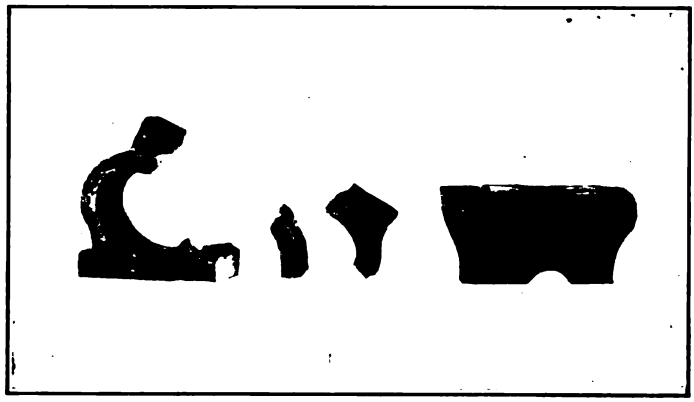


FIG. 7.

FIG. 8.

then subjected to the blows of a 500-lb. drop. The half in the original condition broke in three pieces with one blow of the drop from a height of 4 ft., while the heat-treated half endured without breaking one blow from 4 ft., one blow from 5 ft., one blow from 6 ft., one blow from 7 ft., and two blows from 8 ft., six blows in all. Fig. 7 shows the two halves, and Fig. 8 a back view of the heat-treated half, after testing.

FIG. 9. × 60.

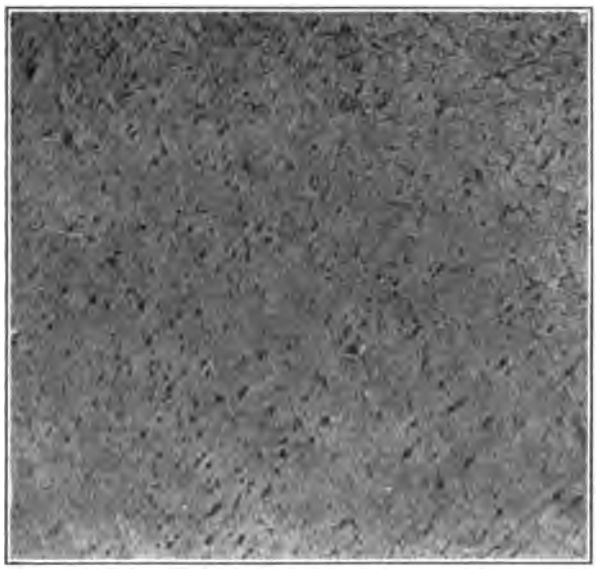


FIG. 10. × 60.

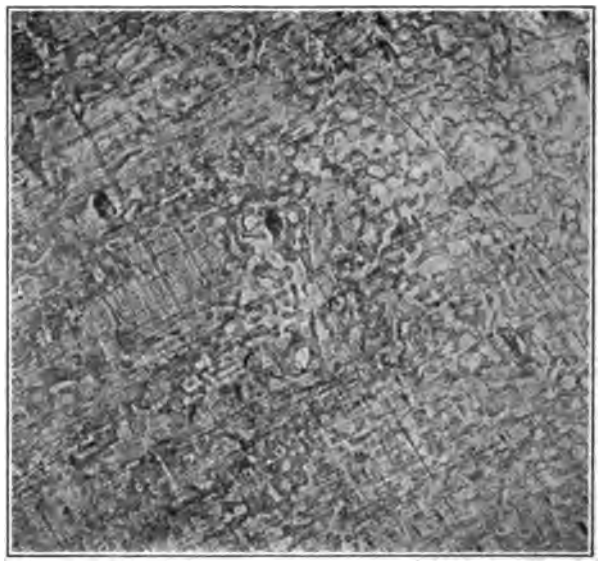


FIG. 11. × 60.

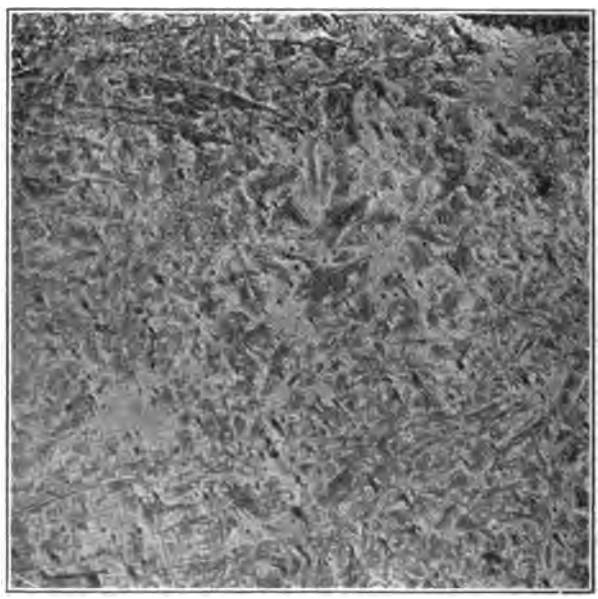


FIG. 12. × 60.

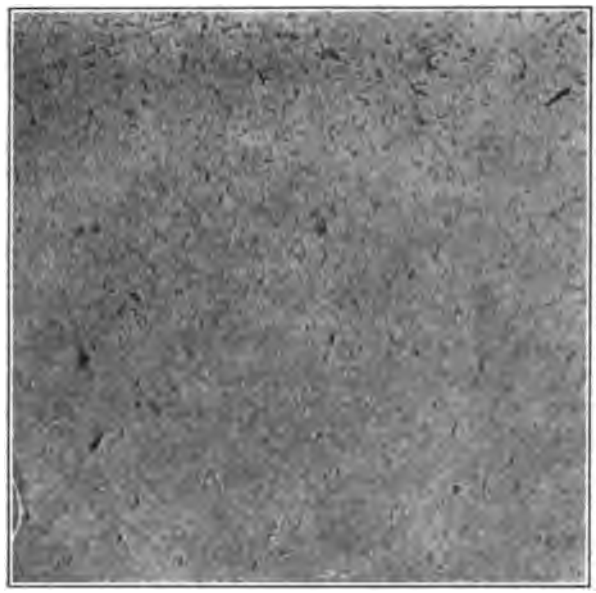


FIG. 13. × 60.

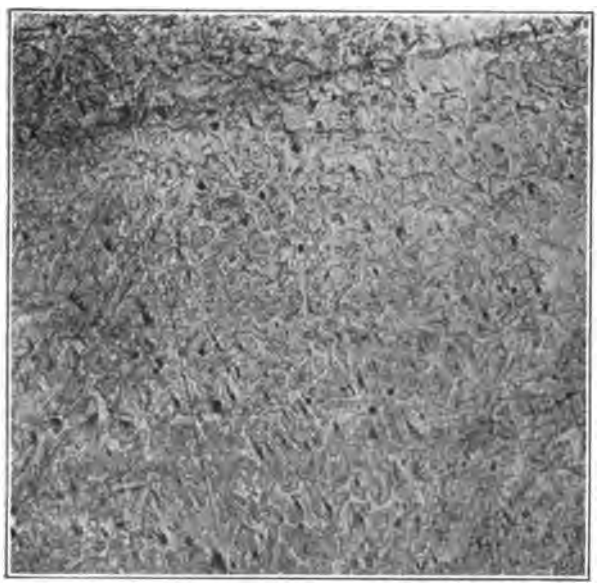


FIG. 14. × 60.

This casting had no section over 1 in. thick, and averaged ½ in. thick all over. The results of the drop test thus confirm the Frémont tests, which show that the steel annealed by heating and slow cooling, which gives the coarse microstructures shown in Figs. 4 and 5, is quite brittle under sudden shock.

The following tests, from coupons 2 by 3 by 8 in. cut from a single casting, and heat treated in several different ways, show strikingly the effect of sudden cooling upon the shock toughness of even very mild steel. The analysis of this steel was: C, 0.10; Si, 0.19; Mn, 0.23 per cent.

Unfortunately, two of the coupons were not sound enough to give tensile test bars of any value, and one bending bar contained a blow-hole. The microstructures of these coupons are shown in Figs. 9 to 14, and the heat treatments and physical properties in the accompanying table:

No.	Treatment.	Tensile Strength. Pounds per sq. in.	Elastic Limit. Pounds per sq. in.	Extension in 2 in. Per Cent.	Contraction. Per Cent.	Fracture.
9	None, ...	50,020	23,930	35.2	47.85	Coarse cryst.
10	900° 3 hr.; cooled in 40 min.,	55,390	28,750	38.5	67.0	Silky cup.
11	900° 3 hr.; cooled slowly, .	. . .	. . .	. . .	. . .	. . .
12	900°; quenched in water, . .	. . .	. . .	. . .	. . .	. . .
13	900° 3 hr.; cooled in air. 710° 6 h.; cooled in air, . . .	56,450	32,950	39.25	65.2	Silky cup.
14	900° 3 hr.; quenched in water. 680° 8 hr.; cooled in air, .	53,200	27,100	34.95	60.4	Silky cup.

No.	Bend ½ by 1 in. on 1-in. Mandrel. Degrees.	Frémont Kgm.
9,	50	2.5
10,	180	20.0
11,	45 (blow-hole)	10.5
12,	180	27.5
13,	180	17.5
14,	180	25.0

This steel in the cast condition, if judged only by tensile test would be rated fairly tough. The bending test gives a low value, and the Frémont test shows that under suddenly applied shock the steel is extremely brittle, and offers almost no resistance to fracture. The various heat treatments are of about equal value as judged by the tensile and bending tests. The Frémont values, however, are lowest in the slowly-cooled steel, considerably higher in the air-cooled and annealed bar and the bar cooled at an accelerated rate, and highest of all in the quenched bars, more than twice as high as in the slowly-cooled bar. That a steel so low in carbon, silicon, and manganese as

this should be so greatly improved in resistance to shock by quenching points to the great advantages of such treatment for castings that may be subjected to sudden severe stresses in service, such as machinery and automobile castings; and brings out strikingly the value of testing under sudden severe shock in revealing degrees of toughness in even a "dead soft" cast steel.

Discussion.

William R. Webster, Philadelphia, Pa.:—I would like to call attention to the fact that the results of the ordinary tension and bend tests do not always give us the information we want even if we figure out the amount of work done. You want a bending test of sufficient width and full thickness. In the case of eyebars, for instance, I have had experience with a 10- by 1-in. eyebar which gave perfectly good results in a full-size test of the finished bar. The 10- by 1.5-in. and 10- by 1.75-in. eyebars from the same heats of steel were absolutely worthless in full-size tests. The narrow bending tests from all these bars gave good results. But after going into the matter fully, we found that if pieces about 8 in. wide and full thickness were used for the cold bending tests, the heavier material would not stand this test, but failed. That is, in order to check the quality of the rolled bar you should have a wide piece of full thickness for cold bending, as the narrow pieces give misleading results.

J. E. Johnson, Jr., New York, N. Y.:—I would like to ask if there have been any tests made to compare the results of Charpy tests with the true dynamic test similar to the Wohler test, except that the piece is subjected to a tensile or compressive stress and to alternating stresses in addition, at the same time. This seems to me to approach more nearly than any other test to the conditions of actual service where the piece is under a fixed initial stress plus a highly variable and often alternating stress. Some tests were made on this plan in England within a year or two. The test piece was subjected to either a tensile or compressive stress of known amount and simultaneously to alternating stresses in addition. Such a test as this gives information which we cannot get by a tensile test or even from the tensile strength multiplied by an elongation. Such tests, however, are exceedingly tedious and require long periods of time in order to obtain valuable results. If material could be subjected to these dynamic tests and the same material to the Charpy shock test, and if some reasonable qualitative relation could be established between the results of the two tests, then we could assume that the shock test, which is so simple and easy to make, was a measurably accurate test

of the dynamic qualities of the material. At present it does not seem to me that we have any adequate ground for assuming this to be the case, though I do not by any means wish to intimate that it is not. I would like to know if these gentlemen who are familiar with this subject know whether anything has been done along this line.

BRADLEY STOUGHTON, New York, N. Y.:—I would like to say a word further as to the value of the shock test. I have used it in two separate ways. About two years ago I had occasion to measure the strength of gear teeth under shock. The usual way is to raise a heavy weight and allow it to fall on a tooth and see if it breaks the tooth off. If it does not break it off the first time, one allows it to fall again, and, after a certain number of blows, the tooth breaks. In some cases it is even more crude than that; a man taking a sledge-hammer and hammering on the point of a tooth until he breaks it. These seem to be the accepted forms of testing the strength of gear teeth. So a couple of years ago, having this problem under consideration, I made a special form of test piece and rigged up a special form of Frémont machine to break the teeth off at the base and measure the residual force left in the falling weight. We never got beyond a first investigation, which I think gave us some data to show that it did give an indication of the strength of the tooth in service. But I am hoping that the work may be continued.

More recently Professor Campbell, of Columbia, has made some Frémont shock tests for me on some steel castings and there we had a very remarkable result in one respect. The castings, which, under the tensile test, appeared to be excellent, and did not disclose any evidence of overheating in the tensile strength fracture, when tested in the Frémont machine gave almost no resistance to shock. Furthermore, the fracture was as coarse looking as if the castings had been badly overheated. Certainly those castings were not what they should have been, and yet the tensile test did not give us any indication of their dangerous character. Perhaps Professor Campbell would be willing to say something more about that.

WILLIAM CAMPBELL, New York, N. Y.:—I think one of the main objections to the shock test is because material that is thought to be very good often appears to be very bad under shock. The steels that Mr. Stoughton sent me, six different samples, were apparently very good, and I expected to get fairly decent readings on all of them; but three of the samples were so brittle that they broke almost like sugar would break. The fracture was absolutely different from the fracture you get under the tensile machine. And on cutting these

samples up and examining them under the microscope we readily found out what the trouble was. The microstructure was absolutely different from that of good material. It seems that the impurities, manganese, sulphide, slag, etc., had arranged themselves around the austenite grain during solidification, and under this form of test the weakness from such a structure is immediately shown up, whereas frequently it is not at all patent when we take the results from the ordinary tensile test piece. One objection to the drop testing machine of the Frémont type is the fact that it has to be calibrated pretty frequently. It appears that the springs vary somewhat, and the original calibrated card that is supplied with the machine does not remain accurate very long; but from the fact that the machine can be so very easily re-calibrated that feature does not seem to take much from its value.

K. W. ZIMMERSCHIED, Detroit, Mich.:—The remarks of Professor Campbell seem to have a strong bearing upon a new conception of the structure of steel, which is gaining a number of adherents. Lately there has been a good deal written about the greater strength of the intercrystalline material in a piece of steel than that of the intracrystalline part; that is, it is held that the crystals themselves are weaker than the amorphous material which lies between them. If this is the case, the more intercrystalline material there is present, the stronger will be the structure as a whole. Now when Professor Campbell shows that his sample, which broke under shock with very little resistance to fracture, contained a considerable amount of impurities around the grains, it may be argued that this weakness is due more to the lack of strong intercrystalline material than to any inherently greater weakness of the crystals themselves.

WILLIAM R. WEBSTER, Philadelphia, Pa.:—J. E. Stead, in discussing C. H. Ridsdale's paper on The Correct Treatment of Steel—read before the Iron and Steel Institute of Great Britain in 1901—gives a very good explanation of why steel that had given satisfactory results in the tension tests sometimes breaks unexpectedly under shock. He said:

"The author had pointed out the bearing the dimensions of the crystalline grain had upon the strength. In the tension testing-machine, they did not get much difference between a coarse-grained and a fine-grained crystalline steel when the strain was gradually applied; but under a falling weight the difference was most marked, and often the coarse-grained steel would snap like a carrot. Such

fractures were not due to intergranular deposits, but to true separation of the cleavage planes. The large crystal masses present large planes of weakness, and when a strain was brought to bear upon these crystals, they separated through their mass, and once the cleavage was started, it rapidly traveled from crystal to crystal through the whole section of the steel. When he was studying, many years ago, the crystalline structure of steel, he obtained very coarse crystalline steel, which elongated 30 per cent. in the testing machine, and yet when a small section was placed upon a V block, and a sudden blow was given so as to put the under surface in sudden tension, on examining the piece under the microscope, he found that one or two of the crystals in the centre of the piece in which the cleavages happened to be vertical or at right angles to the surface had fractured."

ALBERT SAUVEUR, Cambridge, Mass.:—I am glad to see Mr. Hall call attention to the usefulness of the shock tests for detecting that kind of brittleness existing in certain steels, which is not readily detected by the tensile test. I agree with him that shock tests are not used in this country as frequently as they should be. When in France, I visited a number of industrial laboratories and was impressed by the very general use of the shock tests and the excellent results they yielded. The close relation existing in castings between coarseness of structure and brittleness under shock is well brought out by Mr. Hall, as is also the strengthening influence of quenching very mild steel and the value of such treatment in increasing its resistance to shock.

HUGH P. TIEMANN, Pittsburg, Pa. (communication to the Secretary*):—Mr. Stoughton referred to isolated coarse crystals frequently found in large castings after annealing, and remarked that it would be of interest to have this subject investigated and its cause determined.

This is one of the effects of mass which is principally mechanical in its nature and perhaps for this reason has apparently received little attention; at least, judging by what has been published.

The structure of castings is greatly influenced by the rate of cooling, which with those of large size is extremely slow, due to the large mass itself, and materially assisted by the refractory (non-conducting) nature of the molds in which they are generally cast. The cooling is also retarded by increasing the initial (pouring) temperature, particu-

* Received Nov. 14, 1913.

larly during the critical period when crystallization occurs, since there is more heat to be dissipated and the interior of the mold is brought to a higher temperature.

Under these conditions the crystals may be $\frac{3}{8}$ to $\frac{1}{2}$ in or even larger across, which renders a casting unable to withstand any violent shocks or stresses to which a roll, for example, is subjected. Cases have come to the writer's attention where a handful of loose crystals could be secured by striking a casting a single blow with a light hammer. Such a casting was composed of coarse crystals throughout, either because it had escaped annealing through some accident, or because of excessive shrinkage strains during the initial cooling, to which reference is made later. "Ingotism" is the term which Howe has applied to this condition. To secure a durable casting, the crystals must be refined, usually by annealing. This consists in extremely slow heating to a temperature somewhat above the critical point (Ac3), at which the object is held for some time, and then slowly cooled.

Where there is no danger of setting up cracks or excessive strains, which is generally only true of relatively small pieces with simple sections, a finer structure can be secured by quenching in water or oil than by simple annealing, this latter treatment or a partial reheating (tempering) being subseqently employed to remove as far as possible any strains or brittleness, and thereby render the material tougher because more ductile

By either of these methods of treatment, even where repeated, there will almost invariably remain in castings of any considerable size some large isolated crystals, which stand out prominently in the surface of a fracture, being capable apparently of resisting any purely thermal treatment, and surrounded by fine-grained material resulting from the given treatment. Forging or rolling (mechanical working) at the relatively high temperature customary for these operations is the only method by which such crystals can be broken up and refined.

Such a condition is commonly explained as due to too high a pouring temperature producing coarse crystals which cannot be refined by heat treatment alone. This partial explanation is not restricted to ordinary workmen who are accustomed to observe an effect without studying the cause, but is also given by those whose training would naturally be supposed to lead them to study why such a condition should bring about this result.

A microscopic examination will show that the structure of the isolated crystals in a carefully annealed casting is the same as that of the surrounding fine-grained material, hence it is not a case where it

is impossible for the effect of the heat treatment to penetrate to all parts alike.

A casual examination will usually be sufficient to show that these isolated crystals may be actually separated in places from the surrounding mass, and the writer has found in some cases that the space was sufficient to permit the insertion of a penknife blade or other slender object to a depth of $\frac{1}{4}$ in. or even more. In other words, we are here concerned, not with one continuous mass, but with several more or less separate and distinct entities, and it would be as reasonable to expect two pieces of steel, one simply laid in imperfect contact with the other, to be united together by simple heating, as for these crystals to become incorporated with the surrounding mass by this treatment. Since oxidation is absent, forging or rolling will weld these surfaces together, as has been shown by Stead in the case of sufficiently deep-seated blow-holes.

The separation of the crystals is evidently due to the contraction after solidification, which is a different condition from that causing a pipe or other contraction cavity produced practically entirely during, and not after, solidification. Upon solidification the original external dimensions of a casting are determined by the internal dimensions of the mold, its subsequent contraction being progressively resisted by successive interior layers which are at a higher temperature. These interior layers will then continue to contract after the contraction of the exterior layers has practically ceased, and the strains thus set up may be sufficient, under extreme conditions, to bring about actual breaks in the continuity, whereupon the stresses at these places will fall. Where the strains have been insufficient to cause actual separation, suitable heat treatment will have a refining effect on the entire structure, unless perhaps where they are very great. This same condition is probably largely responsible for the brittle and crumbly condition of overheated (not burnt) steel.

WILLIAM C. CHANCELLOR, Pittsburg, Pa. (communication to the Secretary *):—In practice, it is usually the case that shock tests, fatigue tests, etc., are regarded as superfluous and belonging to the theoretical side of metallurgy. While bringing out the extreme sensitiveness of the Frémont test, Mr. Hall, in his paper, Shock Tests of Cast Steel, shows in a very conclusive way that tests of this sort yield practical information on the behavior of steels which is more vital, possibly, than the information obtained by the usual tensile tests.

I have had occasion to make some very simple shock tests, and the

* Received Sept. 10, 1913.

results are given here to further bring out the practical and useful nature of this class of test in cases where the usual tensile methods furnish little or no idea of the relative quality of steels. Unfortunately, these tests are not comparable with any other tests. The method is very crude, having no means of measuring the residual energy of the final blow. The test piece is a round bar, 0.75 in. in diameter and 12 in. long. It is notched with a cutter having a guard which prevents the notch from being over $\frac{3}{32}$ in. in depth. The test piece is held firmly in the machine by wedges and the blows are struck by a weight of 56 lb. falling 1 ft. The striking part of the weight is equipped with a knife edge. Even under these unfavorable conditions it is evident, from the following data, that the drop test throws a very important light on the behavior of cast steel, and that the results are much more conclusive and consistent than the results of the tensile tests.

In the first set are given the results of annealing mild cast steel at various temperatures. The figures for the drop test are the average of at least two tests, while the tensile tests are the results on one bar annealed at each temperature. This was thought sufficient on account of the slight variation in strength for a considerable variation in temperature. All specimens were from the same grade of steel and annealed under the same conditions except temperature.

Theoretically, the correct annealing temperature for this grade of steel is 875° C., and the results should be higher as the temperature increases up to this point and then fall off as the temperature rises from 875° to 1,200° C. In neither the drop nor the tensile tests is this strictly the case, as there are some variations in both, but much more in the latter than in the former.

	Tensile Tests.				Drop Tests.	
Annealing Temperature. Degrees C.	Elastic Limit. Lb. per Sq. In.	Ultimate Strength. Lb. per Sq. In.	Elongation in 2 Inches. Per Cent.	Reduction of Area. Per Cent.	Foot-Pounds.	Breaking Angle.
As cast.....	32,700	67,000	21.0	22.3	170	4.0
750	35,400	65,300	26.5	29.7	280	8.5
800	38,800	75,600	21.5	24.4	390	9.0
850	44,500	72,600	22.0	28.1	340	7.0
900	40,800	74,500	28.5	40.8	670	19.5
950	41,500	79,100	30.5	44.6	450	15.0
1,000	41,500	79,700	33.8	56.0	560	15.0
1,100	39,300	76,900	25.0	33.1	450	12.0
1,200	39,500	69,700	26.5	37.0	450	15.0

The main point brought out by the comparison is that while the tensile tests show only an indifferent change in strength for a consid-

erable change in temperature, the drop tests show a radical change for each change in temperature. The highest ultimate strength, 79,700 lb. per square inch, is only 19 per cent. greater than the strength of the unannealed steel, 67,000 lb. per square inch, while the greatest resistance to shock, 670 ft-lb., is 293 per cent. greater than the unannealed steel, 170 ft-lb.

The superior value of the drop test in detecting the relative quality of steels which show similar tensile results is again brought out in the following tests to determine the effects of oxidation on the steel as manufactured in the open-hearth furnace.

A series of heats, made with no special precautions against oxidation, gave the following results:

Tensile strength, lb. per sq. in.	72,000
Elastic limit, lb. per sq. in.	34,000
Drop test, foot-pounds	429

A second series of heats were made, using special precautions against oxidation in melting and working the heats. These gave the following results:

Tensile strength, lb. per sq. in.	73,000
Elastic limit, lb. per sq. in.	38,000
Drop test, foot-pounds	616

The variations in the tensile tests of the two series are no more than could be expected in steel of the same quality, but the drop test shows that there is 43 per cent. greater resistance to shocks in the specially treated steel over the regular product.

In both of the sets of tests given here, the conclusion to be drawn from the tensile tests alone is that there is very little difference in the quality of the steels tested, but the drop test leaves little doubt as to the superiority of certain steels over others.

HENRY M. HOWE, New York, N. Y. (communication to the Secretary *):—Mr. Hall does well to bring up the impact test, for American engineers have given it neither the attention which it has received in Europe nor that to which it is entitled. I will reinforce what he says by showing further that, though in many cases it shows nothing more than the tensile does, yet in other and very important cases it measures the fineness of structure and the resultant valuable service qualities where the tensile test fails to.

If a specimen, *a*, Fig. 15, notched on its lower side midway of its

* Received Oct. 9, 1913.

length and resting on massive supports at both ends, is broken by the impact of a falling ram, *c*, delivering a blow by means of a knife edge, *d*, the resistance which its bending and breaking offer to the progress of the ram reduces the energy of the blow. We know from the mass of the ram and the height from which it falls, its energy at the instant of impact, and we measure by appropriate means the energy left in the ram after it has broken the specimen. Subtracting the latter from the former we get the energy consumed in bending and breaking the specimen. This quantity is called variously the "resilience," a name the propriety of which is at least questionable,[1] the "energy absorbed," and "the resistance to impact."[2] This latter, which seems to me descriptive and indicative enough, I shall use, abbreviating it to "impact-resistance."

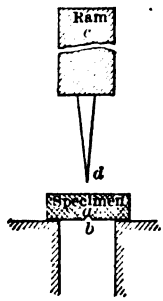


FIG. 15.—THE NOTCHED BAR IMPACT TEST, AFTER FRÉMONT.

Fragility.—Though "fragile" taken generically means "easily broken," whether by slow or sudden stress, in discussing the properties of the materials of construction it is habitually used in the sense of "easily broken by shock." As in general it is more convenient to speak of the strength rather than of the "weakness" of our materials, as we specify a certain minimum of strength, elasticity, and ductility rather than a certain maximum of weakness, inelasticity, and brittleness, specifying the positive rather than the negative, I

[1] Resilience.—"The quantity of work given back by a body that is compressed to a certain limit, then allowed freely to recover itself, as a spring under pressure suddenly relaxed." (Standard Dictionary of the English Language.) This would seem to cover only the work done in the elastic deformation of the specimen, and not to include that done in its plastic deformation prior to rupture, whereas what the impact test determines is the sum of these two quantities. In French one generally finds "Resilience" used; in German one often meets "Specifische Schlagarbeit," the impact resistance for conventional conditions. Its literal translation, "Specific blow work," though short and clear, would be jargon.

[2] Capt. H. Riall Sankey, Sixth Report Alloys Research Committee, *Proceedings of the Institution of Mechanical Engineers*, Feb., 1904, p. 160.

once again coin a word "infragility," the ability to resist shock, as convenient in discussing this subject.

Thus the property determined by the impact test is the "infragility," and the measure of that property is the "impact-resistance," as "specific gravity" is the measure of density, "dollars" of money, and "calories" of heat. Some good writers have indeed measured the infragility by the deformation up to the point of rupture; but, though this is related to the work done in rupture, yet the relation is far from fixed, and it is the latter that should be taken as the true measure of infragility.

The Various Impact Test Machines.—In the machine of Frémont, to whose initiative the existence of the notched-bar impact test is due, the energy which the ram retains after breaking the specimen is shown by the degree to which it compresses some springs which it strikes immediately thereafter. The Howe and Levy tests referred to in these remarks were made with the Frémont machine.

In *Charpy's* machine,[3] now in wider use, and in that of Sankey, the specimen lies at the bottom of the course of a pendulum, and the energy which the pendulum retains after breaking the specimen is shown by the height to which it swings up, as it then keeps on its way.

In the *Guillery* machine the energy absorbed in breaking the specimen is measured by the lessening of the velocity of a fly-wheel carrying the knife edge which delivers the impact.

In the methods of Seaton and Jude, Brinell, and Kirkaldy the impact-resistance is measured by the number of blows needed to break the test piece. The arrangement of the test piece in the Seaton and Jude method is like that of Frémont. But Brinell and Kirkaldy notch their bars in the middle of two opposite sides, support them at one end only, and break them by a blow delivered on the overhanging end, sometimes with the notches in the upper and lower sides of the test pieces, and sometimes on the vertical sides. Izod's method is like Charpy's except that his test piece is supported at one end only, and is struck on the overhanging end.[4]

Dimensions of Test Pieces.—In spite of the great advantages which would result from having all test pieces for the impact test of one set of standard dimensions, practical considerations have thus far prevented this. Hence, with the hope of making results comparable,

[3] *Engineering*, vol. lxxxix, p. 78 (Jan. 21, 1910).

[4] For a description of these methods and of extensive tests of their concordance see Harbord, *Proceedings of the Institution of Mechanical Engineers*, Nov. 20, 1908, p. 921, and *Engineering*, vol. lxxxvi, p. 736 (Nov. 27, 1908).

two standard sizes have been proposed, geometrically similar, one 30 by 30 by 180 mm., with a distance of 120 mm. between supports, with a nick 15 mm. deep, rounded at its bottom to a radius of 2 mm., and the other with one-third of these dimensions (Mesnager).[5]

Unfortunately the expectation that these two sets of dimensions would give comparable results has not thus far been fulfilled. The results of Bartel, Ehrensberger, and Charpy show concordantly that the smaller test piece gives less impact-resistance. The difference increases with the impact-resistance, being smaller in the case of fragile and larger in the case of infragile metals. Indeed, on reflection we see that this expectation was not well founded. That part of the resistance which counts most is that of the fiber at the top of the notch. Once this fiber has parted, rupture will tear back through the remainder of the section. On one hand it is not at all clear that the concentration of stress on this fiber is proportional to the depth of the test piece; and on the other hand for given concentration of stress there, it is far from clear that doubling the depth of the test piece will double the resistance to tearing back, once a crack has started. What happens under these dynamic conditions is not to be traced out beforehand with confidence.

But these dimensions are often departed from. Test pieces 30 by 10 by 8 mm. with a 1-mm. notch are often used, *e. g.*, in the tests of Wolff[6] and of Howe and Levy, and Frémont himself advises that they should be 35 by 10 by 8 mm.[7]

How Harmonious are the Results of the Impact Test?—Though an exaggerated idea of the lack of harmony among the results of the impact test is about, we must admit that their real harmony, under some conditions as yet unexplained, is less than might be expected.

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[5] Charpy, Report of Committee 25 on Impact Tests, *Proceedings of the International Association for Testing Materials*, II, 6 Congress, IV, 1, p. 2, 1913.

"The French navy uses a test piece 30 mm. square and 160 mm. long resting on knife edges 120 mm. apart. The notch consists of a hole 8 mm. in diameter, tangential to the axis of the bar and joined to the surface by a saw-cut; thus the intact section of the bar is to be half the total section. The hole to be pierced with a drill and to be finished with the aid of a reamer.

"The tup, of the weight of 18 kg., shall fall in the direction of the axis of the saw-cut; the hammer shall form an angle of 50° rounded off to a radius of 2 mm; the height of fall is always to be 1 m.

"To pass, the bars must stand at least five blows without breaking; after the fifth blow they should be bent under an angle of at least 147°, corresponding to a mean deformation of $\frac{180-147}{5} = 6° 30'$ maximum per blow." (Charpy, *id.*, V Congress, 1909, III, 1.)

[6] E. B. Wolff, *Proceedings of the International Association for Testing Materials*, II, VI Congress, 1912.

[7] Frémont, *id.*, II, IV, 2, p. 4.

Indeed the harmony is so poor in some cases that only after averaging a large number of results can any accurate inferences be drawn. The discord is not to be explained away by the incompetence of the operator, because it occurs even under most competent observation. I learn privately of marked discord in the results reached by a very competent French experimenter, under conditions which would naturally be expected to yield harmonious results. Harbord's results, too, are less concordant than we should expect in view of his well-known accuracy, as is shown by my analysis of his data in columns 2, 4, and 6 of Table I. The "Mean deviation in percentage of the mean resistance" given in these columns is found by calculating (1) the mean resistance of each steel in a given physical condition, (2) the deviation of each result from this mean in percentage of this mean, and hence (3) the mean of these percentages. These mean deviations run from 1 to 24 per cent., and the mean of all these means in these three columns is nearly 9 per cent.

TABLE I.—*Harmony of Impact-Test Results, Analysis of Harbord's Data.*

Carbon, per cent.		Deviation from the Mean in the Untreated Steel.						Total Amplitude of Deviation.								
		0.262.		0.325.		0.409.		0.262			0.325			0.401.		
		Deviation Per Cent. of Mean Resistance.														
METHOD.		Mean	Max.	Mean	Max.	Mean	Max.	Untreated.	Overheated.	Restored.	Untreated.	Overheated.	Restored	Untreated.	Overheated.	Restored.
	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	14.	15.	16.
Seaton and Jude...		4	14	10	19	21	32	16	68	60	35	55	48	48	50	92
Frémont *b*		7	13	8	11	7	39	25	30	22	20	14	78	39	16	109
Izod.....		2	4	19	31	4	40	7	35	22	49	9	19	64	31	38
Kirkaldy (notch horizontal).....		15*a*	15*a*	6*a*	6*a*	4*a*	4*a*	30	28	24	12	58	29	8	60	29
Kirkaldy (notch vertical)		1	1	4	8	1	2	1	27	37	13	20	37	4	47	50
Average	.....	4.8	.....	9.4	.....	11.4	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....

NOTE:—This table is based on Harbord's data, Table IX, *Engineering*, vol. lxxxvi, p. 769 (Dec. 4, 1908). I have corrected certain apparent misprints in the original.

a Two tests only.

b The notch in the Frémont test is often if not usually rounded as here shown. In Mr. Harbord's test the notch was a sharp inverted "V."

TABLE II.—*Harmony of Impact-Test Results in Certain Tests of Howe and Levy, Compared with Tensile Tests by Norsworthy on the Same Specimens.*

Steel No. IV. Carbon, 0.21; Silicon, 0.039; Manganese, 0.05; Phosphorus, 0.013; Sulphur, 0.010 per cent.

Heat Treatment. *a*			Impact-Resistance.			Tenacity.			Yield Point.			Elongation in 4 Inches.			Contraction of Area.			
Cooled from 900° to 690° in	Held at 690° to 710° for Hours.	Cooled from 690° to 300° in	Individual Tests Kgm. per Sq. Mm.	Mean Kgm. per Sq. Mm.	*b* Mean Deviation. Per Cent.	Individual Tests. Lb. per Sq. In.	Mean. Lb. per Sq. In.	Mean Deviation. Per Cent.	Individual Tests. Lb. per Sq. In.	Mean. Lb. per Sq. In.	Mean Deviation. Per Cent.	Individual Tests. Per Cent.	Mean. Per Cent.	Mean Deviation. Per Cent.	Individual Tests. Per Cent.	Mean. Per Cent.	Mean Deviation. Per Cent.	Ordinate in Fig. 16.
1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	14.	15.	16.	17.	18.	19.
Air	Nil	Air	0.360			56,370			40,610			21.25			66.08			
Air	Nil	Air	.....			58,080			41,600			20.25			64.08			
			0.360	0.373	4.8		60,800	5.9		44,300	7.2		20.5	3.		63.16	3.0	*A*
Air	Nil	Air	0.400			64,240			47,986			19.5			60.54			
Air	Nil	Air	.....			64,535			47,086			21.0			61.95			
Furnace	Nil	Air	0.029															
			0.021	0.025	16.0	57,870			37,480			17.75			58.46			
Furnace	Nil	Air	.....	.....	.....	58,500	57,150	2.0	37,800	37,260	2.2	18.50	18.02	1.9	58.00	55.84	2.8	*B*
Furnace	Nil	Air	.....	.....	.....	54,825			35,610			18.25			53.37			
Furnace	Nil	Air	.....	.....	.....	57,412			38,150			17.60*c*			55.52			
Furnace	Nil	Furnace	.....	.....	.....	53,833			36,008			22.25			57.66			
			0.054				54,570	1.4		34,930	3.1		20.52	8.4		58.06	0.7	*C*
Furnace	Nil	Furnace	0.085	0.070	21.4	55,317			33,858			18.80*c*			58.46			
Furnace	1.5	Air	.....	.....	.....	55,270			35,970			19.25			57.57			
Furnace	1.5	Air	.....	.....	.....	55,360	55,500	0.4	34,840	35,260	1.3	20.50	20.58	4.6	58.96	57.87	1.8	*D*
Furnace	1.5	Air	0.018															
			0.018	0.018	Nil	55,860			34,974			22.00			57.09			
Furnace	23.75	Air	.....	.....	.....	59,400			40,510			18.50			57.54			
Furnace	23.75	Air	.....	.....	.....	57,060	57,580	2.	38,270	38,150	4.3	21.00	20.23	5.7	59.61	59.25	1.9	*E*
Furnace	22.5	Air	.....	.....	.....	56,285			35,660			21.20			60.61			
Furnace	22.5	Furnace	.....	.....	.....	56,055			37,108			22.2			60.45			
			0.032															
Furnace	22.5	Furnace	0.036	0.034	5.9	56,310	56,130	0.6	33,558	35,565	2.8	21.0	21.55	2.6	59.94	60.29	0.4	*F*
Furnace	22.5	Furnace	.....	.....	.....	56,620			35,750			22.0			60.19			
Furnace	22.5	Furnace	.....	.....	.....	55,550			35,846			21.0			60.58			
				Mean...	9.6			2.0			3.5			4.4			1.7	

a Below 300° all bars were cooled in air; after machining they were reheated to 300° and cooled in the furnace.

b The "mean deviation" refers to the mean deviation from the mean in percentage of that mean. This quantity tends to increase as the absolute quantity of any given property decreases.

c Elongation measured on 5 inches instead of 4 inches.

The deviations in the impact-resistance results of Howe and Levy, Table II, column 6, are of this same order of magnitude, running from 0 to 21.4, or on an average 9.6 per cent. These deviations are much greater than those in the tensile tests made by Prof. L. D. Norsworthy with these same specimens, as shown in columns 9, 12, 15, and 18 for comparison. These run from 0.6 per cent. in the case of tenacity to 8.4 per cent. in the case of elongation, and with a general mean of 2.9 per cent., or less than one-third that of the mean deviations of the impact-resistance. The order of harmony is as follows:

Property.	Mean Deviation from the Mean. Per Cent.
Contraction of area	1.7
Tensile strength	2.0
Yield point	3.5
Elongation	4.4
Impact-resistance	9.6

The manner in which Harbord presented his data, shown in columns 11 to 16 of Table I, seems to me to exaggerate the discord. The mean deviation from the mean seems to me a better index of harmony than the total amplitude of deviation, at least when the number of observations varies.[8] It is clear that the total amplitude is likely to increase with the number of observations, though the mean deviation is likely to decrease.

Again the Paris-Lyons-Mediterranean railroad reports that the impact-resistance may vary even as 4:1 in the most homogeneous steels.[9]

But these discords seem referable rather to some peculiarity of the specimens tested than to the method itself, for Goerens and Hartel's results are extremely harmonious, as is shown by my analysis of them

[8] It is quite true that the mean deviation in percentage of the mean is a false measure of accuracy when the quantities to be measured differ greatly. For instance, these percentages are incomparably greater if we are determining manganese in a wrought iron which contains only 0.02 per cent. of that metal than if we are determining it in an 80 per cent. ferro-manganese. But the quantities to be measured by the tensile and by the impact test are of the same order of magnitude. The fact that in the tenacity numbers are in tens of thousands while those of the impact-resistance are only in fractions has nothing whatsoever to do with it, as we see when we compare results reported in tons with the same ones when reported in pounds, or those reported in kilograms with the same results reported in milligrams. The scale which we choose for reporting our results has absolutely no influence on the deviation from the mean in percentage of that mean.

[9] *Proceedings of the International Association for Testing Materials*, II, VI Congress, 1912, IV, 5, p. 10.

"Even among those metals which are regarded as the most homogeneous (crucible steels, electro-steels), the resiliences vary from one value up to double that value in distances of a few cm. Within parts a metre or two apart the variations may even be four times as large and more."

in Table III. To explain, they tested 117 pairs of test-pieces, in the form of rolled bars 1.18 by 0.39 in. (30 by 10 mm.), some from the top and some from the bottom of two common commercial steel ingots. The results are given with three significant figures. In 19 pairs, or about 16 per cent. of the whole, the two specimens of each pair gave identical results. In the 98 other pairs the two results of each pair were closely alike. In one series of 25 pairs the greatest deviation in any one pair of tests from the mean of that pair was only 2.8 per cent. of that mean. The average of such deviations was only 0.6 per cent. in this series, and only 0.8 per cent. in another series of 30 pairs, a degree of harmony which compares favorably with that reached in the tensile test in the most careful hands.

TABLE III.—*Harmony of Impact-Test Results, in Data of Goerens and Hartel.*[a]

Chemical Composition.			Table No. (G.+H.)	Steel No.	Part of Ingot.	Number of Pairs of Specimens.	Number of Pairs which Agreed Exactly	Average Deviation from Mean.		Maximum Deviation from Mean.	
C.	Si.	Mn.						Kgm. per Sq. Mm.	In Percentage of the Several Means.	Kgm. per Sq. Mm.	In Percentage of Our Mean.
0.050	Nil	0.46	2	1	Top	31	5	0.0034	1.8	0.013	7.4
0.089	Nil	0.38	3	1	Bottom	25	4	0.0020	0.6	0.006	2.8
0.084	Trace	0.38	4	2	Top	30	8	0.0018	0.8	0.006	4.1
0.085	Trace	0.38	5	2	Bottom	31	2	0.0053	2.1	0.021	10.7

[a] Goerens and Hartel, *Zeit. Anorg. Chem.*, 1913, *81*, p. 130.

Cause of Discordance.—Why should the impact results be so much more discordant in some cases than the tensile results? One reason which lies close at hand is the greater effect in the former of any heterogeneousness. The particles at the very crotch of the notch have a disproportionate influence on the resistance which the test piece as a whole offers to rupture. Should it happen that a small particle of slag abuts on this crotch, or that the crotch communicates with a weak cleavage plane, a crack will here start readily. Should the crotch of a duplicate test piece be better situated the resistance to rupture should be much greater. In the tensile test local defects and local misfortunes of environment should have no such serious results.

It is antecedently probable that the impact results should be more harmonious in very fine-grained steel than in that which is coarse-grained, because in the former there is less difference between specimens in regard to what is present at the top of the notch than in the latter. To take an extreme case, in a steel made up of grains all of which are alike, if those grains are infinitely small, then the condi-

tions of the notch-top will be the same wherever that notch happens to fall in the test piece; but if those grains are extremely large, the direction of the cleavage of the grains in which the notch ends might have very great effect. If those cleavages in several of the grains ran directly across the bar the impact-resistance would be much less than if they were inclined at 45°. In the data in Fig. 16 there is a straw

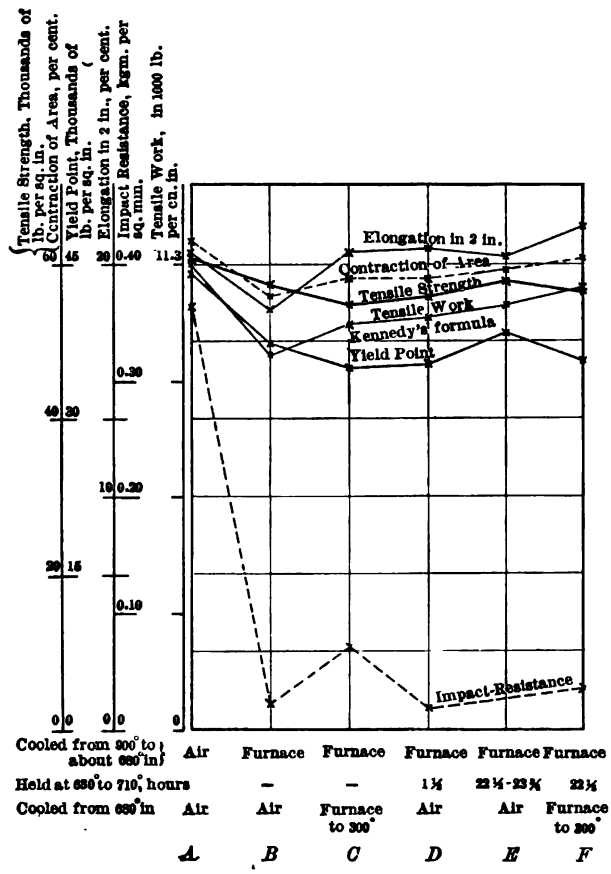

FIG. 16.—THE IMPACT TEST SHOWS THE INFLUENCE OF HEAT TREATMENT MORE CLEARLY THAN THE TENSILE TEST.

Data of Howe and Levy. Influence of Prolonging the Stay in the Divorcing Range on the Mechanical Properties of Steel No. IV, 0.21 per cent. of carbon, 0.05 of manganese, 0.013 of phosphorus, 0.039 of silicon, and 0.010 of sulphur.

which blows in this direction. The finest grained specimens are those of ordinate *A*, air cooled from 900°, and the impact tests of these are very harmonious, as shown in Table II.

Further effects flowing from the special mode of attack in the impact test are pointed out on p. 507.

The opponents of the impact test may point to the discordance of its results as a disadvantage, calling for many repetitions of the test itself. Its advocates may reply that this capacity for discordance is a great advantage, because it detects heterogeneousness, and they may propose that future specifications for important objects shall specify a maximum which the deviation shall not exceed, to the end of excluding heterogeneous material, questioning, in passing, whether the needed repetitions of the impact test need cost more than a single tensile test.

Effect of Variations in the Speed of Impact on the Results of the Impact Test.—In the tensile test, as the straining rate increases, the tensile strength increases moderately with it according to the great bulk of evidence, and the elongation also increases in many cases, though in others this increase is so slight as to escape observation. How is it with the impact test?

Duguet found that "the extreme deformations are, in the case of soft steel submitted to bending, very sensibly the same, whether they be produced slowly by hydraulic pressure or by the impact of a tup." [10]

Kick's [11] experiments indicated that "the velocity of fall has only an insignificant influence on the magnitude of the deformation in impact bending tests.[12]

Charpy reported that the impact-resistance was independent of the height of drop, and that the results of notched bar impact tests differed but little from those of tensile tests.

On the other hand, Frémont, admitting that the former law is true for many steels, finds that for others the resistance varies materially with the height, and therefore recommends either a minimum drop of 4 m., or an equivalant impact speed. Beyond this he advises that the anvil weigh at least 40 times as much as the tup.

Again, the Paris-Lyons-Mediterranean railroad [13] shows that the observed impact-resistance may be about twice as great with a fall of 4 m. (13 ft. 1 in.) as with one of about 1.25 m. (4 ft. 11 in.)

Finally Pérot and Levy [14] found the deformation in the transverse notched bar impact test was inversely as the speed of impact, becoming nil, so that the specimen broke without any permanent set, if impact and rupture were extremely rapid.

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[10] *Proceedings of the International Association for Testing Materials*, I, 1909, III, 1, pp. 6 and 7.

[11] *Proceedings of the International Association for Testing Materials*, V Congress, 1909, III, 1, p. 8.

[12] *Id.*, VI Congress, 1912, IV, 2, p. 4.

[13] *Proceedings of the International Association for Testing Materials*, II, VI Congress, 1912, IV, 5, pp. 5, 11.

[14] *Comptes rendus*, vol. cxxxix, p. 1198 (1904).

Thus the present evidence as to the influence of the rate of straining is contradictory, suggesting that accelerating the straining has more than one immediate effect, and that the sign of these effects is not constant, so that the resultant effect may vary in sign or may be nil.

One's impression is that a rapid impact is especially trying to fragile—*e. g.*, phosphoric—steel. The discrepancies in the evidence may be due to differences in the fragility of the metals tested, the deformation of the fragile metals decreasing and that of the infragile ones increasing with the speed of impact. To test this hypothesis is an easy and useful line of investigation.

Has the Impact Test Reason to Exist?—This question is divisible into two: (A) Does it teach anything that cannot be learnt more readily from other tests? (B) If it does, is its special teaching of value?

Breuil [15] is not alone in believing that the impact test teaches nothing that cannot be learnt as easily from a proper interpretation of the tensile test, and hence that in view of its reproducing the conditions of service much less closely than the tensile test, it should be abandoned.

Admitting freely that there are many materials which would be classified in much the same way by the impact and by the tensile test, I will now inquire whether there are not other materials which are classified differently by these two tests, or in short whether the impact test is not a specific test for certain properties.

The impact test measures the work done in rupture, whereas the tensile test as usually reported measures not the corresponding work, the tensile work, but stresses and strains which are the components of work, and plasticity. The elastic limit and the tensile strength are two stresses. The elongation measures the plastic strain. The contraction of area measures plasticity. Hence the impact-resistance, measuring work, must needs teach something which none of these quantities in and by itself can teach, or in short the impact test does teach something which the tensile test, as reported, does not teach.

The question then arises, (C) Does the impact test teach anything which the tensile test is incapable of teaching? There is another question equivalent to this: (D) Is the impact resistance proportional to the tensile work? If it is, then the tensile work, as found by measuring the tensile stress-strain diagram, teaches all that the impact test teaches. This last is the important question for our present attention.

[15] *Journal of the Iron and Steel Institute*, Supplement to vol. lxv, p. 90 (1904.)

Kennedy's Formula. — Unfortunately the tensile work is rarely recorded and more rarely reported. Autographic stress-strain diagrams are rarely taken, and still more rarely accessible. To plot the diagram from direct observations would be most laborious, indeed quite impossible under existing industrial testing conditions.

To meet this need Kennedy[16] proposes a formula which gives a rough approximation to the tensile work, which is the product of the total strain, elastic plus plastic, into the average stress. It is

$$\frac{\text{Yield point} + 2\ \text{Tensile strength}}{3} \times \frac{\text{Elongation}}{\text{Measured length}} = \text{Energy per cubic inch.}$$

Here "elongation" means the final elongation as usually measured, which is the plastic elongation. This formula leaves out of consideration the elastic elongation. Its assigning to the maximum stress or tensile strength double the value assigned to the stress at the yield point is of course only a very rough approximation, but quite close enough for the sharp contrasts which I will now point out.

In order to learn whether the impact test teaches anything which the tensile test teaches or can thus be made to teach, by asking whether the impact-resistance is proportional to any of the four tensile properties or to the tensile work, I plot in Figs. 16, 17, and 18 these six properties in a way which must needs show at once whether they vary proportionally or not.

This I do by selecting for each property such a scale for ordinates as will bring the beginning of the six curves to approximately the same height, and by making the origin equal to zero for all. If under these conditions the impact-resistance varied proportionally to any tensile property, work included, the impact-resistance curve would necessarily be closely parallel to the curve of that property. In fact there is not the first shadow of parallelism. The impact-resistance discloses losses of infragility which are hardly hinted at by the tensile properties. This is especially striking in Figs. 16 and 17.

Each of the series of tests which I thus analyze is made on a given steel after varying heat treatments. Figs. 17 and 18 represent tests by Roberts-Austen and Sankey on two of the former's steels, and Fig. 16 represents tensile and Frémont impact tests by Norsworthy, and Howe and Levy. Sankey showed in 1904 that his impact tests disclosed fragility which was undetected by the tensile test, in these and other cases.[17]

[16] Sir Alexander Kennedy's formula (Blount, Kirkaldy, and Sankey, *Engineering*, vol. lxxxix, p. 730 [June 3, 1910]).

[17] Sixth Report Alloys Research Committee, 1904, *Proceedings of the Institution of Mechanical Engineers*, Feb., 1904, p. 167.

Again the variations in impact-resistance in Figs. 19 and 20 differ greatly from those in any of the tensile properties and in the tensile work. So in Fig. 21 the loss of impact-resistance with increasing carbon content bears little resemblance to the decrease of tensile work.

Finally Wolff[18] raised the impact-resistance of a steel of 0.035 per cent. of carbon from an extremely small quantity, the impact-resistance of some pieces being only between 0.2 and 6 kgm., to between 20 and 25 kg.; yet the tensile properties were affected but little, the tensile strength rising about 12 per cent. from 33.8 kg. per square millimeter, with a maximum of 34.5, to 38 kg., and the elongation and contraction of area remaining practically unchanged. This was done simply by hastening the cooling so that it took 4 hr. from 1,100° to 20°, and thereby preventing not only pearlite divorce, which was complete in the slowly cooled specimens, but even the formation of pearlite. The description is not very full, but our inference is that this slightly hastened cooling left the eutectoid in the form of sorbite.

Detailed Examination of Some of these Cases.—The damage done to the 0.13 carbon steel, Fig. 17, by the divorcing annealing during the 12-hr. holding at 620°, ordinate *E*, is not clearly shown by the tensile tests, for many would hold that the gain in elongation and contraction of area nearly offsets the loss in elastic limit and the very moderate loss in tenacity. But this treatment destroys the impact-resistance almost completely.

Again, compare ordinates *D*, *E*, and *F* of Fig. 18. The inferiority of the specimens held ½ hr. at 1,100° and 12 hr. at 720°, and the superiority of that held 12 hr. at 620°, are hardly hinted at by the tensile-strength curve; and though they are indeed disclosed by the curves of elastic limit, elongation, and contraction of area, yet they are much more prominent in the impact-resistance curve.

In passing we may refer the inferiority caused by the 1,100° ½-hr. holding to coarsening, and that caused by the 720° 12-hr. holding to pearlite divorce; and the superiority caused by the 620° 12-hr. holding to its being a quasi-sorbitizing drawing of the effects of the presumable cool rolling of the steel as received from the maker, a cool rolling indicated by the very small initial elongation and contraction of area, 6.25 and 18.76 per cent. respectively. It is good practice to improve such steel by rolling it cool or even cold, and then drawing it in this range of temperature. For the high elastic limit and impact-resistance thus obtained, the accompanying softness and cheapness of machining

[18] *Proceedings of the International Association for Testing Materials*, II, VI Congress, 1912. The steels contained C, 0.035; Si, 0.008; Mn, 0.32; P, 0.035; S, 0.056; Cu, 0.018; As, 0.027; N, 0.007.

are much greater than they can be made by hardening and tempering. So with Fig. 16. The passage through the transformation range was a sorbitizing air cooling for ordinate *A*, but for the other ordinates it was a furnace cooling, resulting in pearlitizing, and even in more or less advanced divorcing annealing, which was augmented by further exposure to divorcing annealing temperatures in the treatment of or-

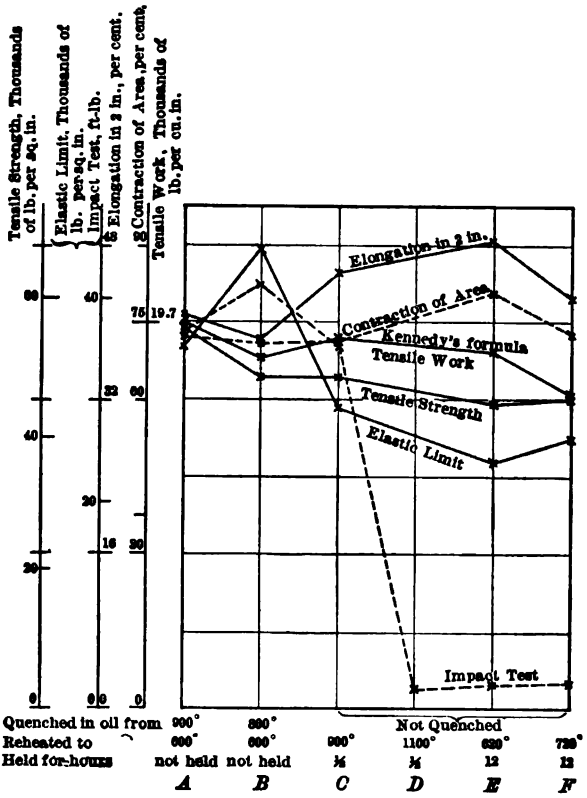

FIG. 17.—THE IMPACT TEST SHOWS THE INFLUENCE OF HEAT TREATMENT MORE CLEARLY THAN THE TENSILE TEST.

Properties of Steel of 0.13 per cent. of Carbon, Roberts-Austen and Sankey. Sixth Report Alloys Research Committee, Institution of Mechanical Engineers, London, 1904.

The steel contained: carbon 0.130, silicon 0.020, manganese 0.185, phosphorus 0.011, sulphur 0.020, arsenic 0.024.

dinates *C* to *F* inclusive. The influence of this slow transformation and of the resultant de-sorbitizing and divorce is shown incomparably more clearly by the impact-resistance than by any or all of the tensile properties.

In Figs. 19 and 20 the strong maximum of impact-resistance when the tempering temperature reaches the 600°–700° range represents the sorbitic state, and the sharp decrease at 740°, on rising past Ac1,

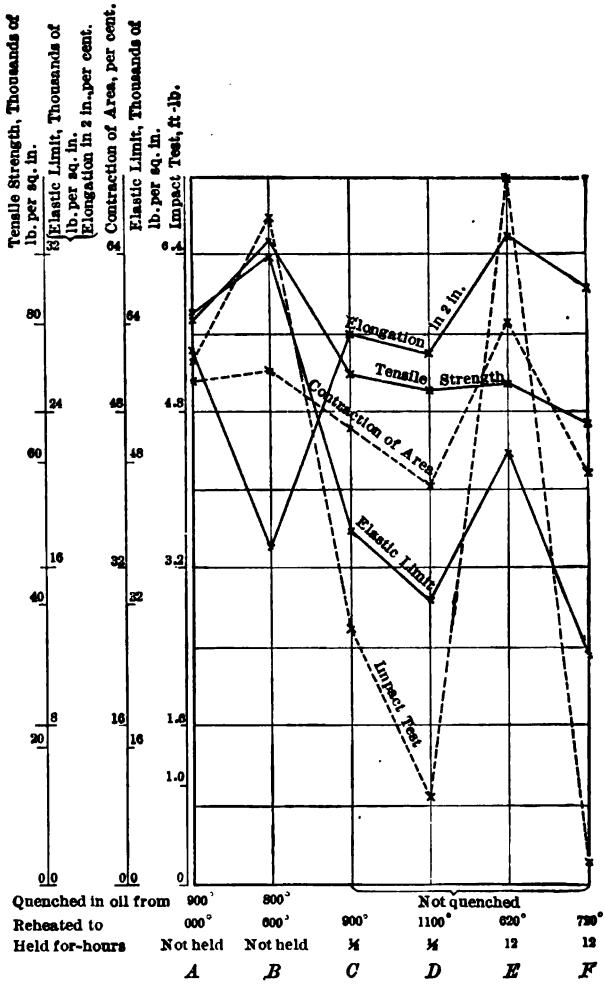

FIG. 18.—THE IMPACT TEST SHOWS THE INFLUENCE OF HEAT TREATMENT MORE CLEARLY THAN THE TENSILE TEST.

Properties of Steel of 0.468 Carbon, Roberts-Austen and Sankey. Sixth Report Alloys Research Committee, Institution of Mechanical Engineers, London, 1904.

The steel contained: carbon 0.468, silicon 0.072, manganese 0.160, phosphorus 0.016, sulphur 0.025, arsenic 0.007.

represents the substitution of pearlite for sorbite. These changes are indeed reflected in the tensile properties but less strikingly.

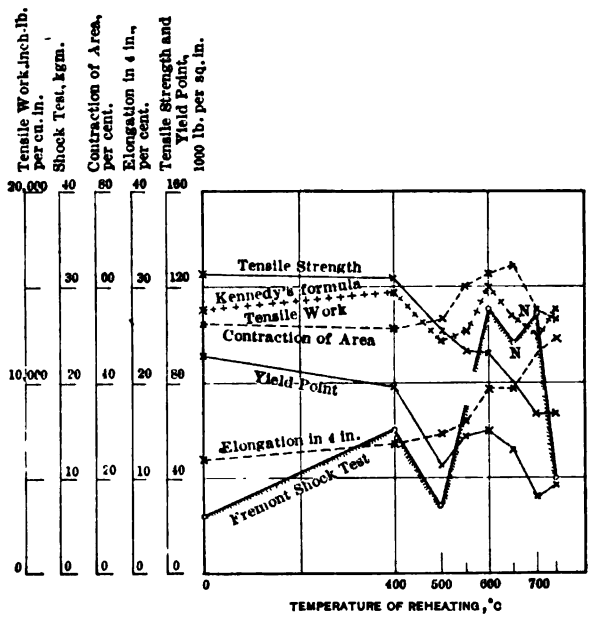

FIG. 19.—VARIATIONS IN THE TENSILE PROPERTIES AND THE IMPACT-RESISTANCE IN THE TEMPERING OF STEEL OF 0.40 CARBON, HOWE AND LEVY.

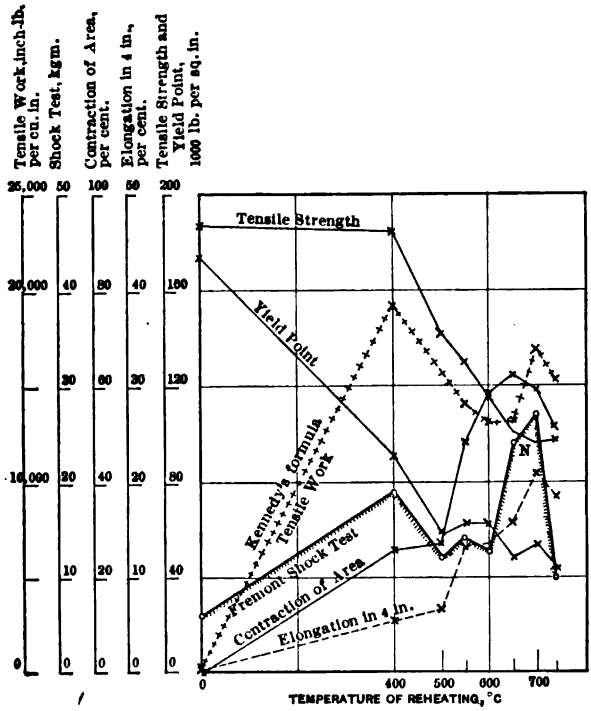

FIG. 20.—VARIATIONS IN THE TENSILE PROPERTIES AND IMPACT-RESISTANCE IN THE TEMPERING OF STEEL OF 0.46 CARBON AND 1.21 PER CENT. OF MANGANESE, HOWE AND LEVY.

thus its value for hypo-elastic uses might be increased greatly. A modification of it which should make this discrimination more general might be of great importance. The angle bent, as found by fitting the broken fragments together, might serve as a basis for calculating the plastic work roughly, and the remainder would represent the elastic work. I throw this out as worthy of thought.

It is on this account that the impact-resistance decreases so very abruptly as the carbon content increases. With this increment of carbon content the elastic limit indeed rises, and with it the elastic work increases; but this increment is completely overshadowed by the simultaneous decrease in plastic deformation. Thus the increase of the elastic limit with increasing carbon content, *e. g.*, in hot-rolled steel, is relatively slight, whereas the decrease in the plastic deformation, as indicated by the elongation, is very great.

This same consideration certainly influences the variations of the impact-resistance for given steel with varying heat treatment. But as we have seen the variations in the impact-resistance with the heat treatment are too great to be explained thus, and they must be referred in large part to variations in the fineness of structure.

Are the Special Teachings of the Impact Test Valuable?—The evidence which I have presented goes far to show that they are. There is the strongest reason to hold that the properties of the sorbitic state, which according to this evidence are detected so much more clearly by the impact than by the tensile test, are of the greatest value. Yet it is very important that the volume of this evidence should be increased greatly, for it is indeed rather scanty, though strikingly harmonious.

The evidence collected by Charpy [20] may here be considered, because it tends to show that the teachings of the impact test are valuable. The value of this evidence is indeed only indirectly contributory, because it does not show that the impact test teaches anything which the tensile test would not teach. Pierrard, the chief engineer of the Belgian naval construction, finds that the endurance of marine engine shafts increases with the impact-resistance of the material. The French navy adopted the impact test for machinery, especially for marine shafting, in 1893, and has extended its use, a fact which certainly suggests that this use has been found instructive. D'Ableiges, for many years President of the French armor plate commission, reported that this navy had found the impact test a very good guide to the ballistic resistance of armor plate. At Montlucon the use of a heat treatment shaped so as to give great impact-resistance lengthened the life of the rods of one steam hammer from an average of less than three months

[20] *Proceedings of the International Association for Testing Materials*, I, 1909, III, 1, p. 16.

to over six years, and of another from six months to three years. The experience with the heat-treated rods lasted some six years.

On the other hand it is reported that the Dutch navy found that steel cast armor plate, which showed but moderate resistance in the impact test, yet was "not fragile" in the ballistic tests. Without further information this result cannot be interpreted with confidence.

Promising as the impact test is for comparing different conditions of the same steel brought about by varying heat treatment, it may be very misleading if used for comparing steels of different carbon contents. For instance, low-carbon steel because of its plasticity excels that of higher carbon in impact-resistance not only very greatly, but to a degree which is wholly misleading as regards the most important class of uses, the hypo-elastic ones. In other words, in comparing steels differing materially in carbon content, the greater plasticity of the lower-carbon ones, though of secondary importance for most uses, yet gives them superiority as regards impact-resistance which has no corresponding service superiority. This is shown in Fig. 21.

For like reasons in comparing steels of different carbon contents the impact test is wholly misleading as to the endurance of repeated relatively light stresses or blows, of which a very great number is needed to cause rupture. The impact test reflects chiefly the work of the plastic deformation, whereas the resistance to indefinitely repeated stresses, whether suddenly or gently applied, depends not on the plasticity but on the elasticity of the material. For uses in which a very small number of severe blows is to be endured a low-carbon material should be used, but for those which imply a very great number of repetitions[21] of light blows a high-carbon material with high elastic limit is better fitted.

Summary.

1. *Harmoniousness.*—The results of the impact test are usually much less harmonious than those of the tensile test. The deviations in one series are about five times as great as those in the tensile strength, and about double those in the elongation. Yet in one extended investigation the mean deviations are less than 1 per cent., *i. e.*, not greater than in tensile testing. The discordance may be due to heterogeneousness of any kind, and especially to coarseness of structure.

2. *Speed of Impact.*—Though in many cases the impact-resistance is affected but little by the speed of impact, yet in others it increases

[21] See Stanton, *Proceedings of the International Association for Testing Materials*, VI Congress, 1912, V, 1, p. 6.

materially with that speed, reminding us that the tensile strength also increases slightly with the rate of straining, on the average of a large series of tests.

3. *Special Teaching.*—The impact test excels the tensile test very greatly in distinguishing between fine and coarse material, a distinction often of very great importance. It also excels the tensile test as a means of measuring the local injury done by flaws, sonims, segregation, etc.

4. *Limitation.*—Because it is affected very greatly by the plastic deformation, it is misleading as a basis for comparing steels of different carbon contents intended for hypo-elastic uses. Its use is rather for comparing varying states of a given steel due to varying heat treatments.

5. *Modification.*—It may be possible to increase its usefulness greatly by comparing the angle bent with the work done, and thus arriving at the hypo-elastic work, instead of as at present reporting the hypo- and hyper-elastic work together.

JOHN H. HALL, New York, N. Y. (communication to the Secretary *):—The discussion of the paper, especially by Professor Howe, is so full that it leaves very little to say. As one of our members once said, when Professor Howe contributes a paper or discussion, it is so complete, and covers so thoroughly all possible aspects of the subject, that further discussion is far from easy.

Several points brought out are of great interest in their bearing upon the question of the applicability of the impact test to steel castings. In Professor Howe's discussion, he states that it has been found that the smaller tests give less impact resistance than the larger ones. The great difference in the impact resistance of tests cut from different parts of the same coupon of cast steel which has been subjected to heat treatment, has led me to think that a much larger test than any that has been yet used might be of value in examining such material. In particular, I have found that two steels which gave approximately equally good results in the Frémont test, were shown to be considerably different in their resistance to repeated heavy shocks when tested in a bar some 2.5 in. square. This was a very rough test, more or less in line with that used by Mr. Chancellor in making the experiments summarized in his most interesting discussion.

Professor Howe's point that steels of different carbon content should not be compared by the impact test, on account of the deficiency of plastic deformation in high-carbon steel, is well taken. As he says,

* Received Nov. 7, 1913.

for most uses the greater plasticity of the low-carbon steels is of secondary importance; and the property most desirable is the ability to resist relatively light stresses constantly repeated. Thus, for instance, we know that in locomotive axles a medium high-carbon steel will give a longer life than a low-carbon steel; and from this we might infer that it is best to use a high-carbon casting in machinery parts which in ordinary service will not be called upon to endure heavy stresses. In some special cases, however, this conclusion is not necessarily correct. For instance, some manufacturers of taxicabs use cast-steel front axles, and make a part of the casting so light that it will bend or break, and thus save the rest of the car, when the vehicle comes into collision with some heavy object. In service, many of these axles have been seriously bent in collisions, straightened and replaced in service, several times. Under ordinary conditions of service, a high-carbon steel in one of these axles will give longer life than a low-carbon steel, because, as ordinarily heat treated, the high-carbon steel possesses much greater strength and elastic limit, and hence a greater resistance to repeated light stresses. By proper heat treatment, however, a soft steel can be made to possess as much strength and endurance under repeated stress as a harder steel treated in the usual manner; and at the same time, the soft steel will possess a resistance to shock much greater than the hard steel will exhibit in any condition. The great plastic deformation of the soft steel in this case will not be of value under ordinary conditions of service; but will be of great value in case of a collision, in two ways: first, because the soft steel axle will be very much less apt to break and upset the car, to the discomfort of the occupants; and second, because such an axle can be bent and straightened more times than one made of more brittle steel. This, of course, is a very special case, but it is probable that many similar instances might be found.

With regard to Mr. Johnson's question as to whether a parallelism has been found to exist between the impact test and the shock test, I am not aware of any figures that have been published on this subject. Professor Howe's citations of the greatly increased life of marine engine shafts and of steam-hammer piston rods, so treated as to give great impact resistance, at least points to the probability that impact resistance and endurance to repeated stresses go together in some cases. The accompanying table of tests of cast steels, made in the White-Souther endurance testing machine, is based upon entirely too few bars to be much of an answer to Mr. Johnson's query. However, as far as they go, these figures are of value in illustrating what has just been said about the possibility of obtaining simultaneously

great impact resistance and great endurance to repeated stresses, in a soft cast steel. It will be seen by these figures that the softer steel has by suitable heat treatment been given a strength and elastic limit about equal to that of the harder steel, and that it possesses at the same time great impact resistance. In the endurance test, as the table shows, four bars of this steel so treated endured an average of 10,373,050 revolutions at a fiber stress of 28,270 lb. to the square inch, and an average of 823,950 revolutions additional at a fiber stress of 38,870 lb. to the square inch. Compared with this, we have on ten tests of harder steel, all treated alike (leaving out test C), an average of 3,805,000 revolutions at 28,270 lb. to the square inch for five bars; and an average of 10,451,240 revolutions at 28,270 lb. per square inch, and of 590,560 revolutions additional at 38,870 lb. per square inch, for five bars. It will also be seen that with one exception the softer steel in every case endured more revolutions than any test of the harder steel. No doubt, by heat treating the harder steel in the same way as the softer, both the tensile properties and the endurance figures for this steel can be improved; and therefore, if we are considering only the resistance to repeated light stresses, we should use the harder steel treated in the best way. But if we desire good resistance to repeated light stresses coupled with great impact resistance, we would use the softer steel heat treated, and kill two birds with one stone.

Mr. Stoughton and Dr. Campbell state that certain cast steels, which they tested, exhibited comparatively good tensile figures but gave very poor impact resistance; and that the fracture of this steel was coarse, as though the steel were badly overheated. My experience has been that in hypo-eutectoid cast steel cooled slowly from the annealing temperature (and by this I mean cooled at what is a slow rate in a large annealing furnace, a matter of a good many hours), the impact resistance is not particularly high, even in soft steel, and the microstructure consists of comparatively large grains of ferrite and pearlite. These steels so annealed habitually give quite good tensile properties, but rather poor impact resistance; and the impact tests break with the fracture described. This condition of course is often greatly exaggerated by extreme coarseness of microstructure, by under-annealing, or by great segregation of impurities. But the general type of fracture is typical, and very different from that of steel heat treated in such a way as to give higher impact figures, which, as Professor Howe has pointed out, are always an indication of truly fine microstructure. Of course, the lower the carbon and the

greater the plastic deformation of the steel in the impact test, the more striking the difference in the fracture of the steel.

As Professor Campbell says, the Frémont machine requires frequent calibration; indeed its mechanical construction is not very good, so that it frequently gets out of order, and when a great many tests are being made every day, a good deal of time is wasted in tinkering with the machine. Not having had experience with other designs of impact testing machines, I am unable to say whether they are open to the same criticism; from my general knowledge of their design, I imagine that they do not give as much trouble in handling as the Frémont machine, at least in its present form.

In conclusion, I wish to express my sincere thanks to the gentlemen who have so ably contributed to the discussion.

No.	Cut from	C.	Si.	Mn.	S.	P.	Treatment, Heated to	Treatment, Cooled.
		Per Cent.	Per Cent.	Per Cent.	Per Cent.	Per Cent.		
A	4 in. sq.	0.42	0.46	0.73	.....	.....	900° 5.5 hr.	Air
							700° 5.5 hr.	Slowly
B	4 in. sq.	0.44	0.38	0.59	.....	.....	900° 5 hr.	Air
							690° 3 hr.	Slowly
C	4 in. sq.	0.46	0.42	0.71	.....	.....	960° ? hr.	Slowly
D	4 in. sq.	0.36	0.54	0.71	0.049	0.051	920° 5 hr.	Air
							690° 3 hr.	Slowly.
E	4 in. sq.	0.47	0.56	0.73	.....	.....	900° 5.5 hr.	Air
							700° 11 hr.	Slowly
F	4 in. sq.	0.43	0.46	0.65	.....	.....	900° 4 hr.	Air
							700° 5 hr.	Slowly
G	1.25 by 2.5 by 12 in.	0.23	0.31	1.07	0.037	0.047	900° 6 hr.	Water
							680° 8 hr.	Air
H	4 in. sq.	0.27	0.46	0.71	.....	.....	900° 5 hr.	Water
							680° 8.75 hr.	Air

No.	Tensile Strength. Lb. per Sq. In.	Elastic Limit. Lb. per Sq. In.	Extension in 2 In. Per Cent.	Contraction. Per Cent.	Frémont.
A	82,100	45,990	8.41	9.79	7.5
B	76,170	42,990	14.64	15.11	5.0
C	76,730	42,750	12.05	15.81	5.0
D	73,200	40,630	14.08	19.79	10.0
E	79,590	44,020	10.94	11.49	8.5
F	73,200	40,200	12.5	16.4	7.5
G	67,200	44,400	14.19	31.3	32.0
H	78,710	46,400	22.8	24.21	17.0

Endurance Tests.

No.	Fiber Stress.	Deflection.	Revolutions No. 1 End.	Revolutions No. 2 End.
A	28,270	0.06	10,709,600	10,709,600
B	28,270	0.06	4,685,000 (B)	6,708,000 (B)
C	28,270	0.06	2,796,800 (B)	10,000,000 a
D	28,270	0.06	3,474,300 (B)	2,368,700 (B)
E	28,270	0.06	10,158,400	1,789,400 (B)
F	28,270	0.06	10,339,300	10,339,300
G	28,270	0.06	10,475,100	10,475,100
H	28,270	0.06	10,271,000	10,271,000

No.	Fiber Stress.	Revolutions No. 1 End.	Revolutions No. 2 End
A	38,870	280,800 (B)	302,000 (B)
B	.....	.....	.....
C	38,870	.....	288,700 (B)
D	.....	.....	.....
E	38,870	1,828,600 (B)	.....
F	38,870	366,000 (B)	175,400 (B)
G	38,870	393,900 (B)	845,500 (B)
H	38,870	834,100 (B)	1,222,300 (B)

B signifies bar broke.
a Exact number uncertain.

The Influence of Copper Upon the Physical Properties of Steel.

BY G. HOWELL CLEVENGER,* PALO ALTO, CAL., AND BHUPENDRANATH RAY, CALCUTTA, INDIA.

(Butte Meeting, August, 1913.)

FORMERLY great divergence of opinion existed in regard to the influence of copper in steel, as affecting its various physical properties. More recently the investigations of Stead,[1] Breuil,[2] Wigham,[3] Burgess and Aston,[4] Ball and Wingham,[5] Müller,[6] Dillner,[7] and others have shown fairly good agreement in the major points. Copper steel has been shown by these investigators to possess special properties which make it valuable for certain uses.

The general range of composition of the useful alloys of copper and iron, and of copper, iron, and carbon, has been quite definitely established, for the former by Burgess and Aston,[8] and for the latter by Breuil[9] and Stead.[10]

Stead[11] has removed one difficulty in reconciling certain of the earlier data by calling particular attention to the influence of carbon in copper steels. Another point which caused confusion in the earlier investigations, particularly in regard to "red-shortness," was the presence of sulphur. Failure to note the presence of sulphur often led to attributing "red-shortness" to the presence of small amounts of copper. Turner[12] has called attention to the fact that where copper exists in an iron ore it often occurs as the sulphide, so that the presence of copper in steel made from such ores might also, necessarily, mean the presence of sulphur. It is evident that the effect in this case might be quite different than where pure copper was added.

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[1] *Journal of the Iron and Steel Institute*, vol. lx. (1901, II), p. 104.

[2] *Idem*, vol. lxxiv. (1907, II), p. 1.

[3] *Idem*, vol. lxix. (1906, I), p. 222.

[4] *Transactions of the American Electrochemical Society*, vol. xvi., p. 241 (1909).

[5] *Journal of the Iron and Steel Institute*, vol. xxxiv. (1889, I), p. 123.

[6] Wedding: *Stahl und Eisen*, vol. xxvi., No. 23, p. 1444 (Dec. 1, 1906).

[7] *Idem*, vol. xxvi., No. 24, p. 1493 (Dec. 15, 1906).

[8] *Transactions of the American Electrochemical Society*, vol. xvi., p. 241 (1909).

[9] *Journal of the Iron and Steel Institute*, vol. lxxiv. (1907, II), p. 1.

[10] *Idem*, vol. lx. (1901, II), p. 104.

[11] *Idem*, p. 114.

[12] *Idem*, p. 138.

Unsound ingots, which are sometimes complained of with copper steel, may be due to the method of adding the copper. The well-known property which copper possesses of absorbing gases when molten, which are given off during solidification, and which makes the production of sound castings of pure copper difficult, has led us to think that the presence of copper might confer upon molten steel somewhat the same property. Our experience in making the crucible steel for this investigation points strongly to this conclusion; but it also indicates that this difficulty can be overcome by taking proper precautions in adding the copper.

In the past a most peculiar condition has existed as regards copper steel, in that while very little of it has been made intentionally, thousands of tons of both pig iron and steel have been placed upon the market, which contained copper, on account of copper occurring in the iron ores from which the pig iron or steel was made. All of this steel has apparently proved satisfactory in every way.

Frequently, where specimens of ordinary steel have shown unusual strength and hardness, and resistance to corrosion, in excess of what could be expected from ordinary carbon steels, it has been found upon analysis that the steel contained copper, present as an accidental constituent, but nevertheless effective in producing a superior grade of steel. This has often been the case where rails have shown unusual wear. In this connection copper steel would appear to approximate nickel steel, while, due to the smaller amount of copper necessary and its lower price, copper steel could be made at a less cost.

The purpose of this investigation has been to assist in removing the unfounded prejudices which have existed in the past in regard to copper steel.

Experimental.

Making of Ingots.—A small circular furnace, lined with silica brick and fired by means of a low-pressure burner, using California crude oil as a fuel, and not employing the regenerative principle, was used for heating the No. 25 graphite crucibles in which the steel was melted.

The raw materials used for making the steel consisted of mild steel punchings containing 0.23 per cent. of carbon, the best grade of electrolytic copper, 80 per cent. ferro-manganese, and a good grade of commercial aluminum.

The general method of procedure was to charge 30 lb. of punchings into the crucible and fire until the metal was melted. It generally required about 2 hours before the charge began to melt and

about one-half hour more for the metal to become perfectly fluid. The molten metal was then "killed" for from 20 to 25 min., after which 2.5 oz. of ferro-manganese was added and the metal thoroughly stirred. After from 5 to 7 min. more the required amount of copper was added and the metal again stirred. About 5 min. later 0.096 oz. of aluminum was added and then, after again stirring, the metal was at once poured into a previously heated ingot mold. Sufficient slag was added to each charge to form a protective coating over the molten metal. This was done with the idea of preventing, to a large extent, the absorption of gases by the metal after it had become fluid.

At first we encountered considerable difficulty in making sound ingots; but, after experience had taught us to proceed in the manner outlined above, every heat produced sound ingots free from blow-holes. While we did not have the opportunity of thoroughly investigating this phase of the subject, the two points which appear to us to be the most important in making copper steel by the crucible process are: 1. To thoroughly kill the metal before adding the copper. 2. To add some deoxidizing agent, as aluminum, just before pouring.

Each ingot weighed approximately 30 lb. and measured 3 by 3 by 12 in., with the exception of A0, which weighed 22.5 lb. and was 3 by 3 by 9 in.

After having determined the proper method of melting, eight ingots were made. In the making of this series of ingots all the conditions were maintained as nearly the same as possible, with the exception of the proportions of copper added as noted below.

Designating Mark.	Per Cent. of Copper Added.
A0	0.00
A0.25	0.25
A0.5	0.50
A1	1.00
A2	2.00
A3	3.00
A4	4.00
A5	5.00

All of these ingots were sound and free from blow-holes. Each ingot was sawed to a depth of 1 in. on two opposite sides, 2 in. above the bottom. After breaking, a fracture area of approximately 1 by 3 in. was shown. The appearance of these fractures is shown in Fig. 1. In sawing and breaking the ingots the following facts were observed: A0 appeared to be rather soft and tough—about what would be expected of ordinary machine steel. The brittleness and fineness of grain became more evident with increasing copper content, except in the case of A5, which had a coarser grain than any of the other specimens.

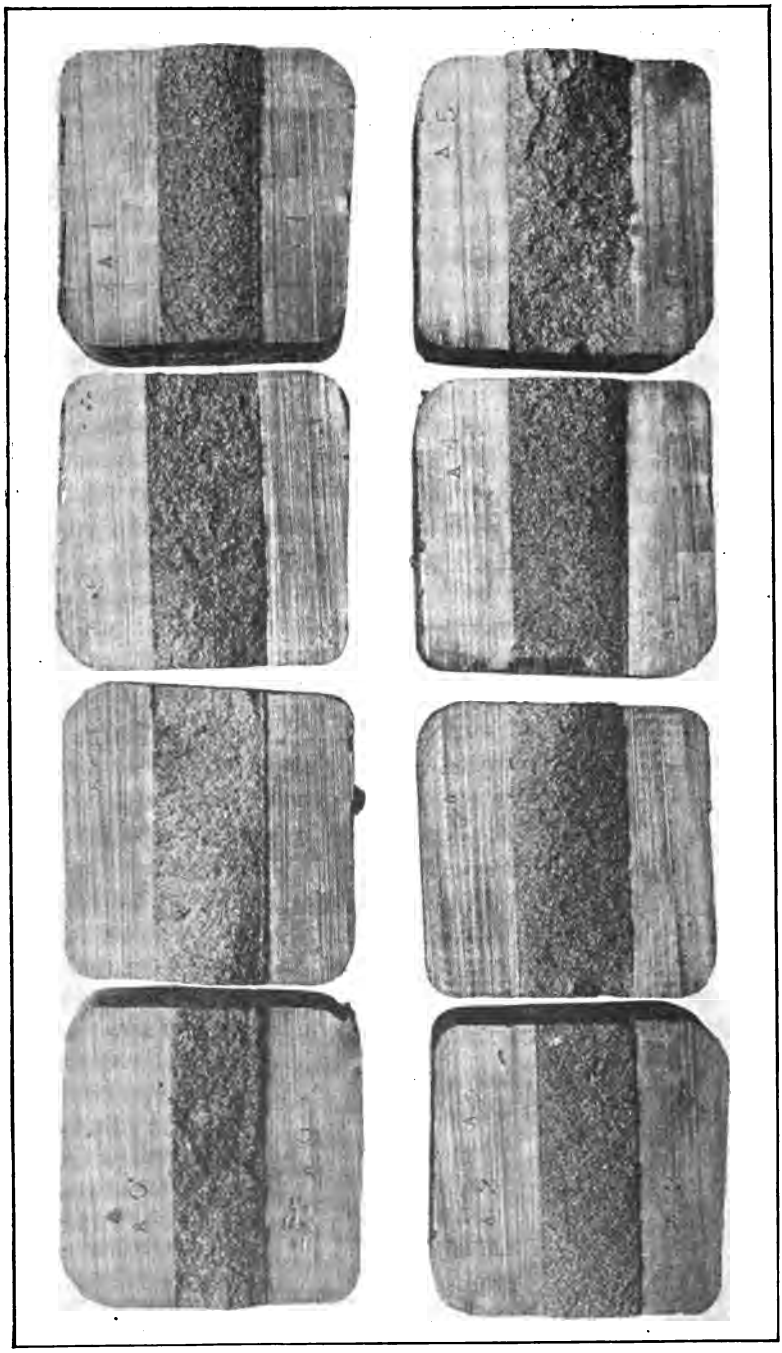


FIG. 1.—FRACTURES OF INGOTS.

Segregation was not noticeable to the eye in any of the ingots; all were white, lustrous, and uniformly grained in the fracture. There were reddish streaks of copper oxide, in the form of very thin films, on the surface of ingots A4 and A5.

The slags resulting from the steels containing the higher percentages of copper were slightly tinged red, presumably by the presence of copper oxide.

Forging of Ingots.—The eight ingots were heated in a large forge and reduced to 1⅛-in. octagonal bars by means of a steam hammer. All forged about like ordinary machine steel. A2 appeared to be somewhat softer than the rest. A5 shows traces of red-shortness and, after cooling, the surface of the bar showed minute cracks.

Welding.—Lap welds were tried upon the eight bars, with the following results:

A0—Seemed to weld perfectly at a bright red heat, almost yellow, similar to "machine steel."

A0.25—Appeared rather overheated but welded easily.

A0.5—Could not be welded with the hand hammer at yellow heat, and welded with difficulty under the drop hammer at bright yellow.

A1—Welded moderately well under the drop hammer at bright yellow.

A2—At bright yellow, scintillating, welded with difficulty under the drop hammer.

A3—Could not be welded at any heat from red to almost white.

A4—The ends slipped off and crumbled to pieces at bright yellow under the drop hammer Metallic copper showed on the surface of the heated ends when cold.

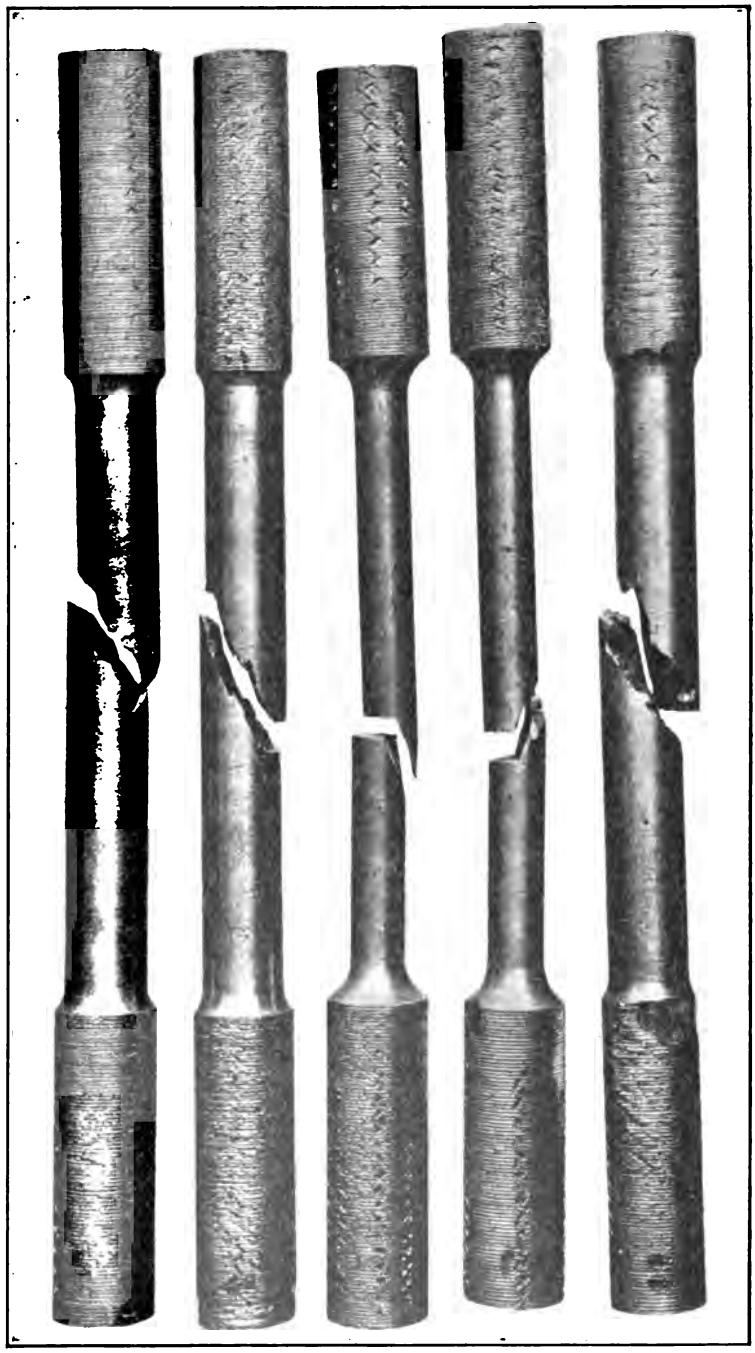
A5—The ends did not stick at any heat, resisted all attempts to weld; copper showed on the surface of the heated ends as in A4.

The failure to weld the steels containing the higher proportions of copper may have been due to the formation of a film of copper oxide upon the surface of the weld.

Test pieces were turned from the welded specimens and the ultimate breaking strength of these determined, with the results given below:

Bar.	Ultimate Breaking Strength of Welded Test Pieces. Pounds per Square Inch.	Ultimate Breaking Strength of Unannealed Test Pieces. Pounds per Square Inch.
A0	22,300	86,700
A0 25	25,500	99,500
A0.5	88,200	99,000
A1	83,000	97,400
A2	39,600	120,660

A0 broke along the weld.
A0.25 broke along the weld.
A0 5 broke along and across the weld.
A1 broke along and across the weld.
A2 broke along the weld.



A0 A0.25 A0.5 A1 A2

FIG. 2.—WELDED SPECIMENS AFTER FRACTURE.

Fig. 2 is a photographic view of the welded specimens after fracture.

Different authorities do not agree as to the limit of copper content beyond which the steel is not capable of being welded.

The most remarkable welds of copper steels were those made by Colby [13] upon steels containing in most cases about 0.565 per cent. of copper. All his specimens were said to be capable of being successfully welded, and only one of them broke at the weld and this under a load of 61,630 lb. per square inch.

Definite conclusions in regard to the welding of copper steel cannot be drawn from the small number of results which we are able to present; but our results, together with those of Colby, would seem to indicate that it might be possible that steel containing from 0.5 to 1.0 per cent. of copper, while more difficult to weld, when once welded seems to produce a stronger union than a similar steel containing no copper.

Chemical Analyses.—The drillings for the chemical analyses were taken from the sides of the ingots, as indicated by the shaded areas in Fig. 3.

Copper was determined by ether separation followed by the iodate volumetric method. Carbon was determined by the total-combustion method, sulphur by the evolution method, phosphorus by Handy's volumetric method, manganese by Ford's method, and silicon by the regulation gravimetric method.

TABLE I.—*Analyses of Steel.*

	A0.	A0.25.	A0.5.	A1.	A2.	A3.	A4.	A5.
	Per Cent.	Per Cent.	Per Cent.	Per Cent.	Per Cent.	Per Cent.	Per Cent.	Per Cent.
Carbon	0.46	0.605	0.523	0.471	0.533	0.437	0.442	0.462
Manganese	0.445	0.477	0.320	0.329	0.305	0.433	0.386	0.436
Sulphur	0.0385	0.0295	0.039	0.036	0.022	0.0206	0.021	0.0195
Silicon	0.076	0.078	0.094	0.060	0.052	0.066	0.060	0.066
Phosphorus	0.0172	0.0160	0.0189	0.0172	0.0210	0.0172	0.0203	0.0147
Copper (*B*)	.....	0.165	0.493	0.846	1.857	2.773	3.574	4.512
Copper, top of ingot (*A*)	.....	.....	.....	0.838	.....	2.609	.....	4.371
Copper, bottom of ingot (*C*)	.....	.....	.....	0.849	.....	2.796	.....	5.640
Copper added	.....	0.25	0.50	1.00	2.00	3.00	4.00	5.00

Effect of Copper in Eliminating Sulphur.—Inspection of Table I. shows a regular decrease of sulphur with increasing copper content. Breuil's [14] analyses of his semi-mild steels (Table II.) show the same phenomenon, although certain of his other analyses seem to be contradictory. The results of certain other investigators seem to show the same tendency, but we are unable to quote them on account of not

[13] *Iron Age*, vol. lxiv., No. 22, p. 1 (Nov. 30, 1899); also *Journal of the Iron and Steel Institute*, vol. lvii. (1900, I), p. 412.

[14] *Journal of the Iron and Steel Institute*, vol. lxxiv. (1907, II), p. 4.

knowing the exact conditions under which their steel was made, and on account of the further uncertainty of the effect of other factors, which might have had an influence upon the sulphur. However, it seems probable that copper does have an important effect upon the elimination of the sulphur. This point is worthy of further investigation.

TABLE II.—*Analyses of Semi-Mild Steel—Series B (Breuil).*

	B0.	B0.5.	B1.	B2.	B4.	B8.	B16.	B32.	
	Per Cent.	Per Cent.	Per Cent.	Per Cent.	Per Cent.	Per Cent.	Per Cent.	Per Cent.	
Carbon	0.336	0.390	0.400	0.389	0.368	0.372	0.412	0.282	
Manganese	0.150	0.139	0.164	0.175	0.139	0.135	0.160	0.230	
Silicon	0.316	0.323	0.307	0.242	0.217	0.316	0.298	0.214	
Phosphorus	0.020	0.020	0.022	0.023	0.022	.....	.....	.....	
Sulphur	0.017	0.013	0.012	0.010	0.011	.....	.....	.....	
								Outside.	Center.
Copper, bottom	0.0	0.505	1.005	2.025	4.009	7.960	16.015	24.4	74.8
Copper, top	.....	.....	.....	.....	3.985	7.910	15.985	21.2	34.2

Segregation of Copper in Ingots.—Within the limits investigated, there was no segregation of the copper which was noticeable to the eye or which was apparent in the specimens examined under the microscope. This seems very strange in view of the fact that copper became evident upon the surface of A4 and A5 when an attempt was made to weld them. Determination of the copper in the samples taken at *A*, *B*, and *C*, Fig. 3, shows a slight tendency toward segregation in A1. In A3 this is more marked and in A5 it becomes serious. The tendency for copper to segregate toward the bottom of the ingot is marked.

Breuil [15] also noted higher percentages of copper in the bottom of his ingots than in the top, as will be seen from Table II. He failed to discover any difference in copper content between the bottoms and tops of ingots containing less than 4.0 per cent. of copper. He also found that very high copper steels formed an exterior shell with a lower percentage of copper and an interior core very rich in copper. He tested the homogeneity of the ingots by a method based upon the principle of Brinell's method of testing hardness, and found that ingots containing less than 30 per cent. of copper possess fairly uniform hardness from the skin toward the center. "Generally speaking, the hardness is greater at the bottoms of the ingots than the tops." "Ingots of steel containing less than 8 per cent. of copper show no coloration at their fractures. Between 8 per cent. and 20 per cent. of copper they are fairly uniform physically, and do not display marked segregation. Above 30 per cent. of copper the segregation is marked."

[15] *Journal of the Iron and Steel Institute*, vol. lxxiv. (1907, II), p. 6.

Stead [16] says it was found that "steel with 1 per cent. carbon would dissolve and retain in solution about 7 per cent. copper, and that when this amount is exceeded the excess is thrown out of the solution at the setting point and appears as globules." "When the proportion of copper is increased above 10 per cent., the suspended globules also increase, and when 25 per cent. copper is melted with 75 per cent. steel, a portion of the copper separates from the steel before it solidifies, and is found at the bottom of the ingot in a separate layer. It is, however, not pure, but is associated with about 10 per cent. iron, part of which is in solution and part as dendritic crystallites." Stead made an alloy of equal parts of copper and iron by melting them together in a charcoal-lined crucible and allowing the alloy to cool in the crucible. "It separated into two conjugate layers of the following composition:

	Top Layer. Per Cent.	Bottom Layer. Per Cent.
Iron	87.00	9.60
Copper	10.34	90.02
Carbon	2.07	0.08
Silicon	0.45	Nil
Etc	0.14	0.30
	100.00	100.00"

Corrosion Tests.—Cylindrical test pieces were turned from each forged bar, 0.5 in. in diameter and 0.5 in long. These were thoroughly cleaned and weighed, and then completely immersed in glass beakers containing 400 cc. of 1:3 sulphuric acid which was maintained at 22° C. In order that sediment which formed might settle to the bottom of the beaker, and thus keep a fresh surface constantly exposed to the action of the acid, the small steel cylinders were supported upon tripods made of thin glass rod. The beaker was covered with a watch glass to prevent evaporation. Fig. 4 shows the general arrangement. The cylinders were left in the acid for 160 hours, after which they were taken out, washed, and thoroughly cleaned of all sediment, dried, and accurately weighed. The following table gives the percentage of loss suffered in each case at the end of 160 hours:

Steel.	Copper Content. Per Cent.	Loss by Corrosion in Acid. Per Cent.
A0	0.0	22.20
A0.25	0.165	7.14
A0.5	0.493	5.35
A1	0.846	6.93
A2	1.857	11.31
A3	2.773	32.60
A4	3.574	37.00
A5	4.512	42.60

[16] *Journal of the Iron and Steel Institute*, vol. lx. (1901, II), p. 114.

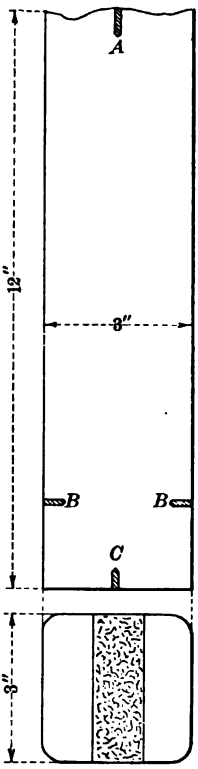


FIG. 3.—SHOWING POINTS DRILLED FOR ANALYSES.

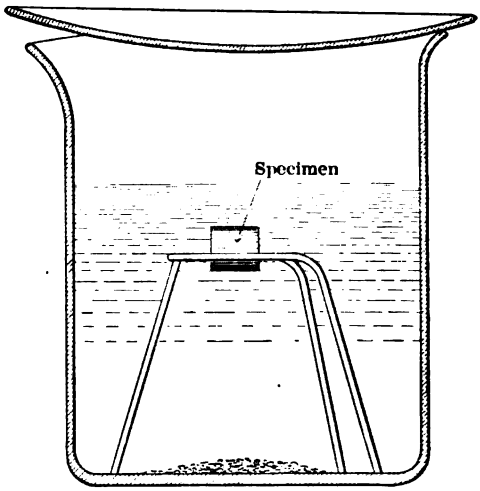


FIG. 4.—SPECIMEN SUPPORTED ON GLASS RODS FOR CORROSION TESTS.

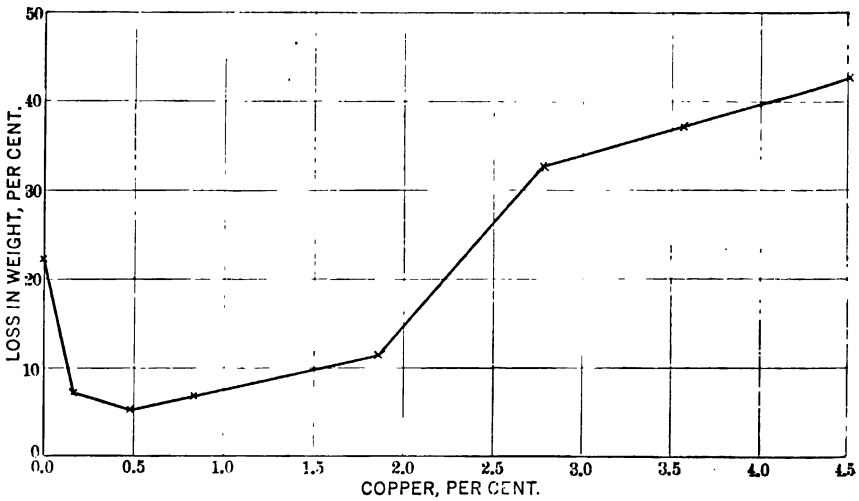


FIG. 5.—CURVE SHOWING CORROSION BY 1 : 3 SULPHURIC ACID.

Reference to the accompanying table and Fig. 5 will show that when no copper is present the loss in weight is 22.2 per cent.; 0.165 per cent. of copper lowers the loss to 7.14 per cent. The minimum loss, 5.35 per cent., occurs when 0.493 per cent. of copper is present. With 0.846 per cent. of copper the loss in weight is slightly increased, to 6.93 per cent.; 1.857 per cent. of copper raises the loss in weight to 11.31 per cent. Increasing the copper content to 2.773 per cent. causes an abrupt rise in the loss of weight to 32.6 per cent. From this point up to 4.512 per cent. of copper the rise in the loss of weight is regular and much less abrupt.

Sample A0 corroded very irregularly and was pitted and riffled over the whole surface. All the copper steels corroded uniformly and evenly, leaving an almost smooth surface; the edges remained sharp and remarkably well preserved.

Breuil's [17] samples containing 0.5 per cent. of copper, in both the series containing 0.15 and 0.35 per cent. of carbon respectively, gave the minimum loss due to solvent action of the acid. This agrees with the results given above.

Walker [18] says, "In investigating samples of iron and steel which had withstood corrosion for years, and which notwithstanding dissolved in acid very readily, together with samples which withstood solution in acid in a remarkable manner, and yet were rusting at the ordinary rate, it was found that the acid resistant specimens contained in every case a substantial amount of copper; that is, the presence of copper seemed to be the controlling factor in the resistance to solution in acid."

He made tests upon basic open-hearth steels and obtained the following results:

	Carbon. Per Cent.	Manganese. Per Cent.	Sulphur. Per Cent.	Phosphorus. Per Cent.	Copper. Per Cent.	Loss in Weight After 3 Hours in 20 Per Cent. Sulphuric Acid. Grams.
A.....	0.08	0.50	0.018	0.017	.....	0.2235
A3.....	0.08	0.50	0.018	0.017	0.21	0.0075
B.....	0.1	0.41	0.027	0.026	0.19	0.0082
C.....	0.09	0.31	0.031	0.063	0.19	0.0095

The effect upon acid corrosion of small quantities of copper is strikingly shown by these results. He further observes that iron and steel in contact with metallic copper will dissolve more readily than when they form perfect mixtures, and he brings out the analogy of the case of zinc and copper, which are very actively attacked by acids if the two metals remain in contact with each other, but which

[17] *Journal of the Iron and Steel Institute*, vol. lxxiv. (1907, II), p. 61.

[18] *Metallurgical and Chemical Engineering*, vol. ix., No. 9, p. 453 (Sept., 1911).

are very little affected by the solvent action of the acid when melted into bars of brass.

The effect of atmospheric corrosion was studied by Williams.[19] Samples of Bessemer steel and wrought iron were frequently dipped in water and allowed to dry in the air. This operation was carried on for one month. As the percentage of copper increased the loss decreased. He gives the following table:

Loss from Atmospheric Corrosion.

	Loss. Per Cent.
A. Soft Bessemer steel	1.85
B. Soft Bessemer steel with 0.078 per cent. Cu	0.89
C. Soft Bessemer steel with 0.145 per cent. Cu	0.75
D. Soft Bessemer steel with 0.263 per cent. Cu	0.74

Steel and Wrought Iron.

Soft Bessemer steel	1.65
Wrought iron, Sample 1	0.76
Wrought iron, Sample 2	0.80
Wrought iron, Sample 3	0.87
Wrought iron, Sample 4 (containing 0.393 per cent. Cu.)	0.53

His results with copper steels are comparable with those of Howe[20] upon nickel steel.

Buck[21] has made an elaborate and convincing investigation of the effect of atmospheric corrosion upon steels containing small percentages of copper. In order that the conditions, except the copper content, should be the same in all cases, copper was added to portions of three heats, while the other portions were poured directly into ingot molds. The steel was made by the basic open-hearth, basic open-hearth (rephosphorized), and Bessemer processes. The steel to which copper was added actually contained 0.15, 0.24, and 0.34 per cent. of copper respectively. The steel was rolled into the regulation 16 and 27 gauge corrugated sheets. In addition there were purchased in the open market 16 and 27 gauge sheets which contained from 0.06 to 0.07 per cent. of copper.

Several sheets of each grade were placed in roofs at testing stations located in the Pennsylvania coke regions, where the air contains sulphuric and sulphurous acids and unprotected roofs corrode very rapidly; at the sea shore, where the air contains salt; and in a rural district where the air was free from corrosive agencies.

"In every case the steels with copper additions have shown a

[19] *Iron Age*, vol. lxvi., No. 22, p. 16 (Nov. 29, 1900); also *Engineering and Mining Journal*, vol. lxx., No. 23, p. 667 (Dec. 8, 1900).

[20] *Metallurgy of Steel*, p. 371 (1890).

[21] *Journal of Industrial and Engineering Chemistry*, vol. v., No. 6, p. 447 (June, 1913).

marked resistance to corrosion as compared with the non-copper steels, having on the average nearly twice the life. There appears to be very little difference between the grades containing 0.15 copper and those with 0.24 to 0.34, while the material with low carbon and manganese and with 0.06 to 0.07 copper takes an intermediate position between the copper-bearing steels and those without copper."

Accelerated acid tests were made on 2 by 4 in. test pieces, cut from the same sheets as were used for the atmospheric corrosion tests, with 25 per cent. sulphuric acid at 35° C. "The copper-bearing Open-Hearth and Bessemer steels resist the acid from 50 to 100 times as well as the non-copper steels, and within the limits of the copper content of the steels used in this test, the resistance to the acid is directly proportional to the amount of copper present. In this regard the acid tests differ from the actual weather tests."

Burgess and Aston[22] have investigated the corrodibility of a number of alloys of electrolytic iron with other metals, in the making of which every precaution was taken to exclude other impurities. In their series of alloys of electrolytic iron with copper, the copper content ranged from 0.089 to 7.05 per cent. All of these show a high resistance to attack by acid, and also a low atmospheric corrosion for the whole series, and it is of particular interest to note the beneficial results from even small additions of copper. "The quality, both mechanically and from the corrosion point of view, is entirely comparable with that resulting from the use of corresponding amounts of nickel and, of course, there is a materially lessened cost for the addition."

Microstructure.—Two series of specimens were prepared for microscopic examination, one with the steel as forged, and the other annealed. The annealing was done at a temperature of 820° C. and at the same time that the bars were annealed for the annealed tensile test pieces. The etching agent employed was a 10 per cent. solution of concentrated nitric acid in absolute alcohol. In the case of some of the higher-copper alloys a subsequent application of a saturated solution of iodine in absolute alcohol was necessary. The specimens were polished and etched and the photographs were taken by H. M. Boylston in the laboratory of Sauveur & Boylston, Cambridge, Mass.

Upon examination of the micrographs, Figs. 6 to 21, it will be seen that in the case of the unannealed specimens the ferrite is permeated with filaments of cementite more or less in proportion as the percentage of copper increases. Larger patches of free ferrite appear in the specimens with decreasing copper content and the pearlite is less

[22] *Journal of Industrial and Engineering Chemistry*, vol. v., No. 6, p. 458 (June, 1913).

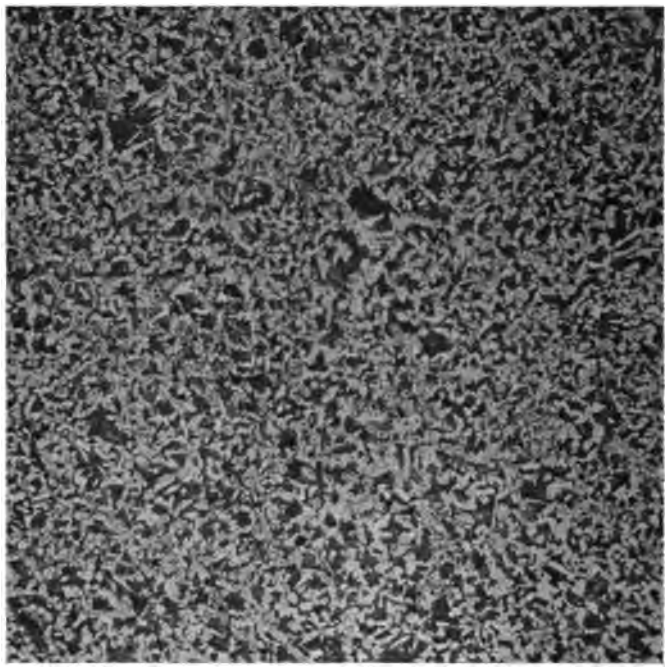


FIG. 6.—SPECIMEN A0. × 100.

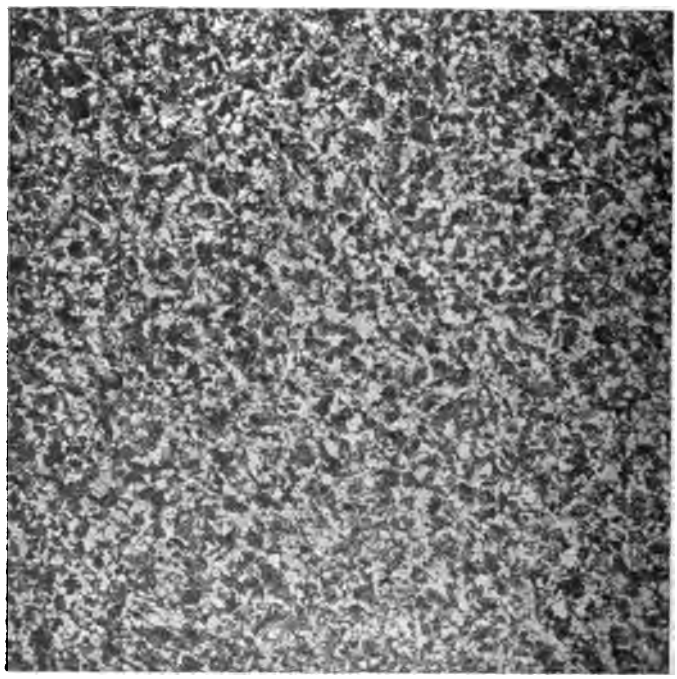


FIG. 7.—SPECIMEN A0A. × 100.

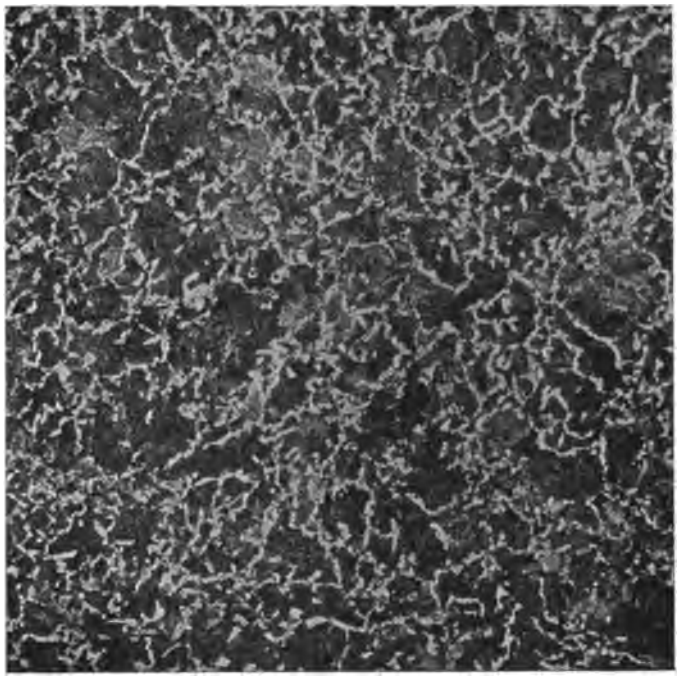


FIG. 8.—SPECIMEN A0.25. × 100.

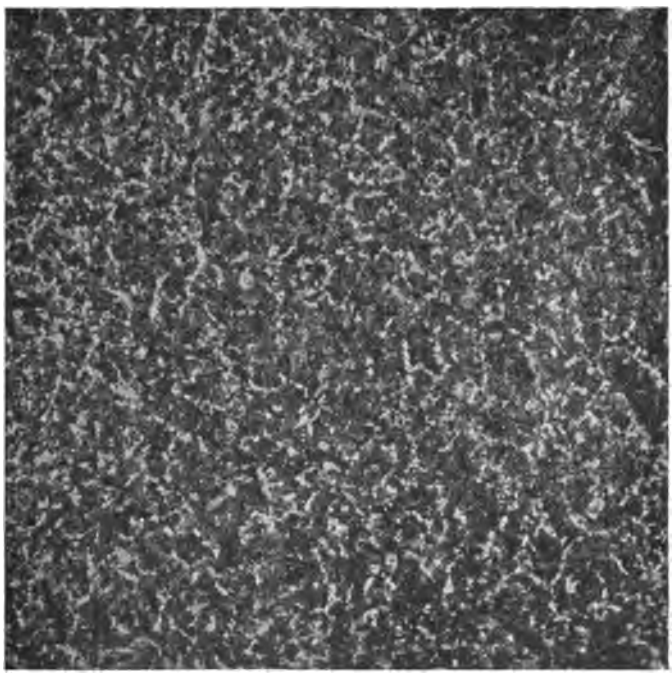


FIG. 9.—SPECIMEN A0.25A. × 100.

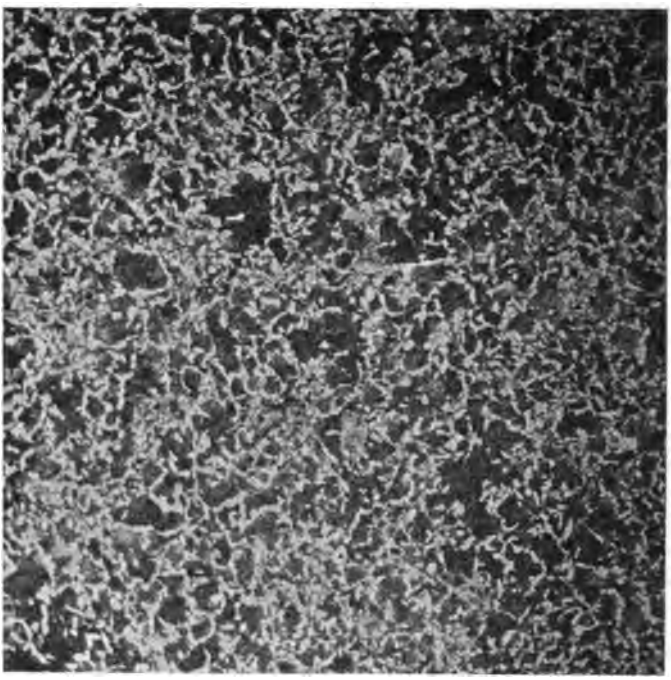


FIG. 10.—SPECIMEN A0.5. × 100.

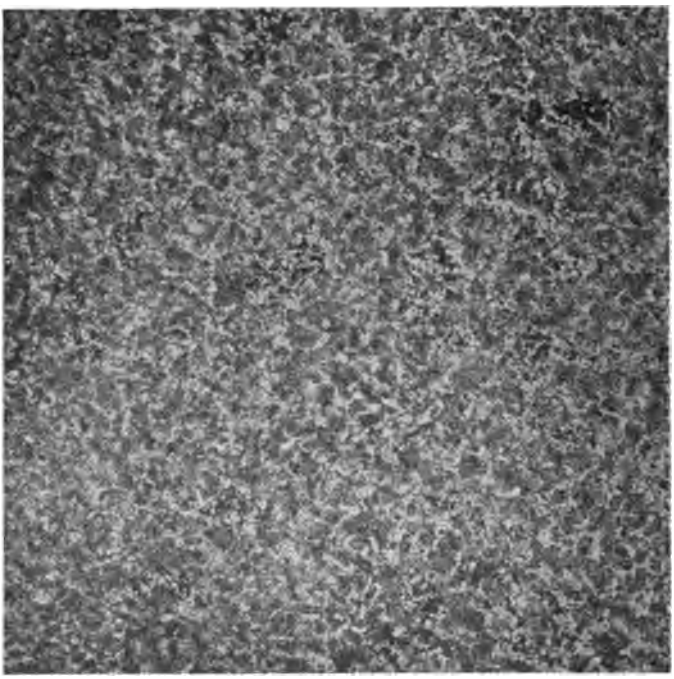


FIG. 11.—SPECIMEN A0.5A. × 100.

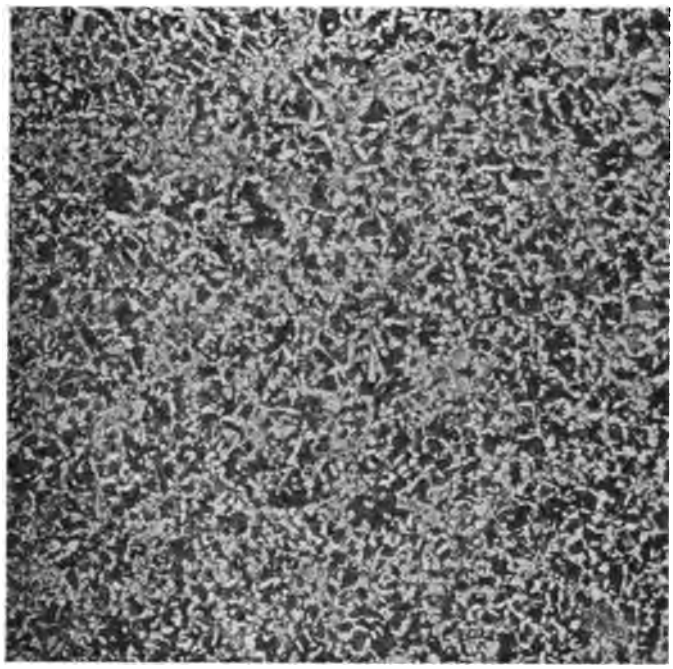


FIG. 12.—SPECIMEN A1. × 100.

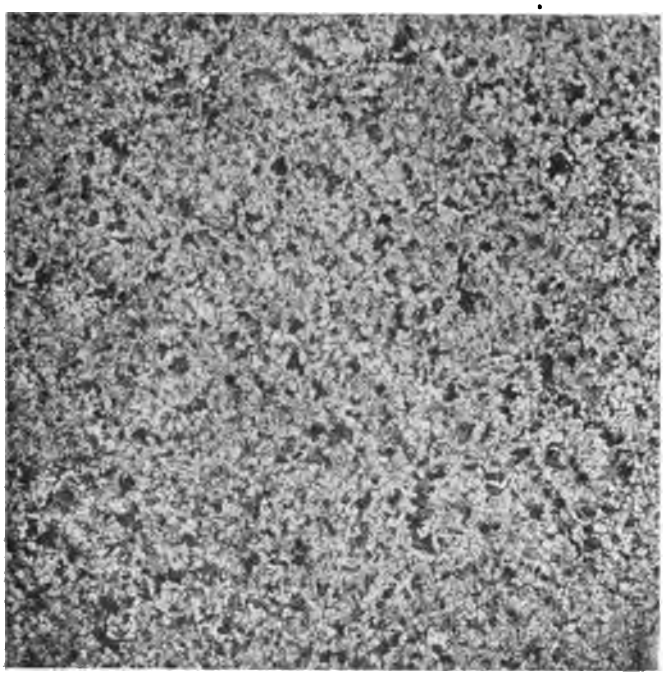


FIG 13.—SPECIMEN A1A. × 100.

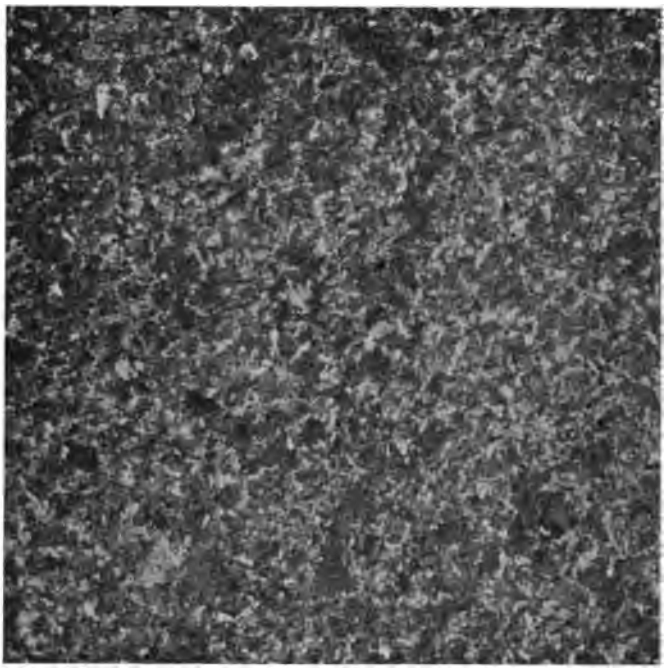


FIG. 14.—SPECIMEN A2. × 100.

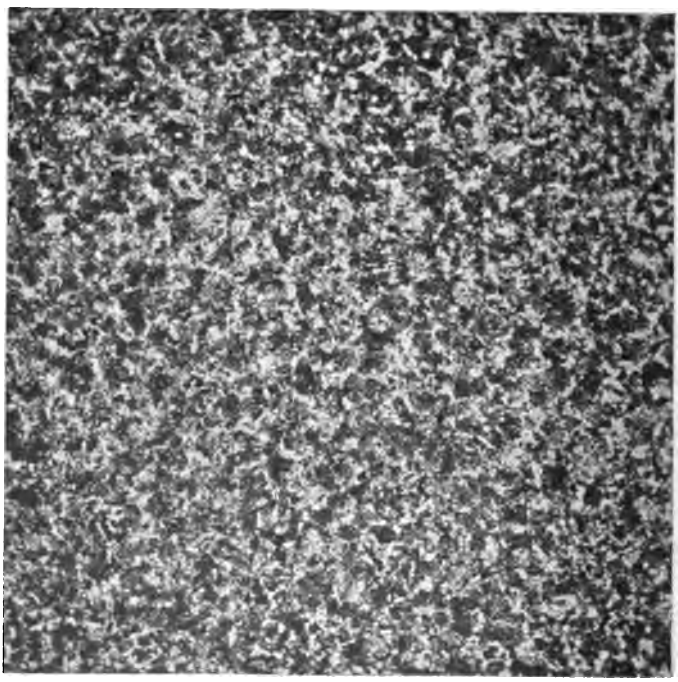


FIG. 15.—SPECIMEN A2A. × 100.

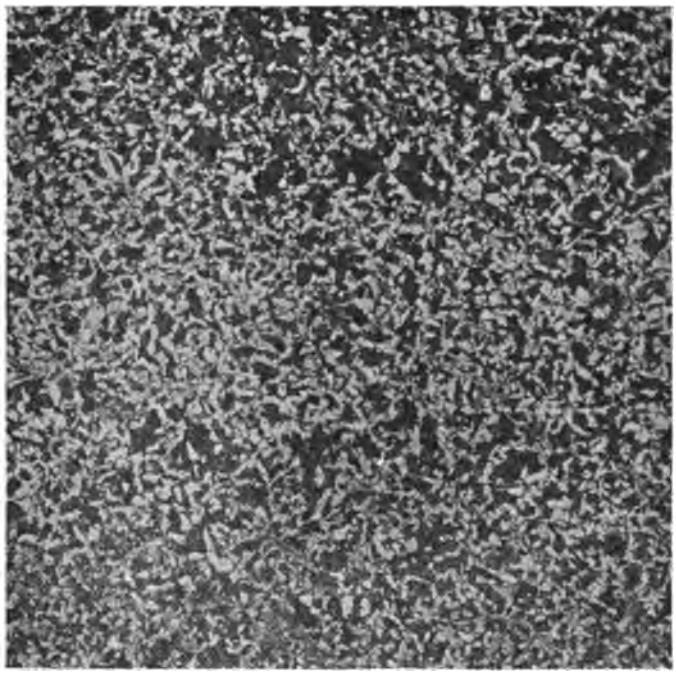


FIG. 16.—SPECIMEN A3. × 100.

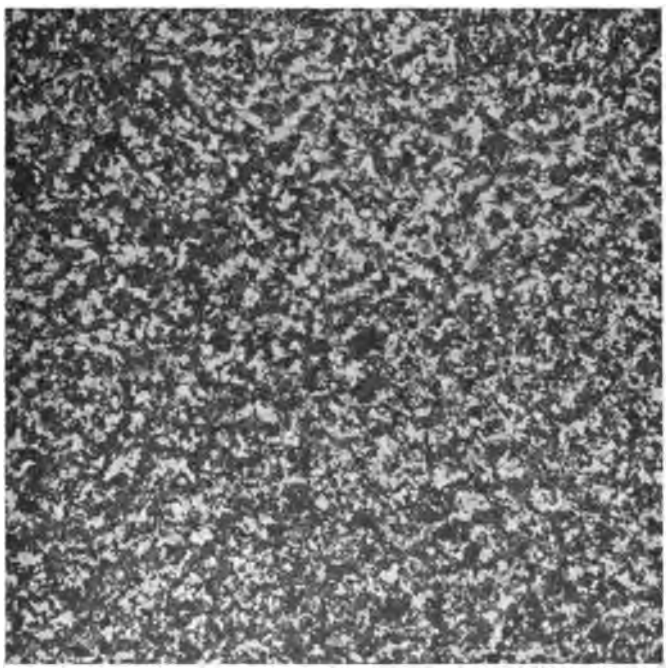


FIG. 17.—SPECIMEN A3A. × 100.

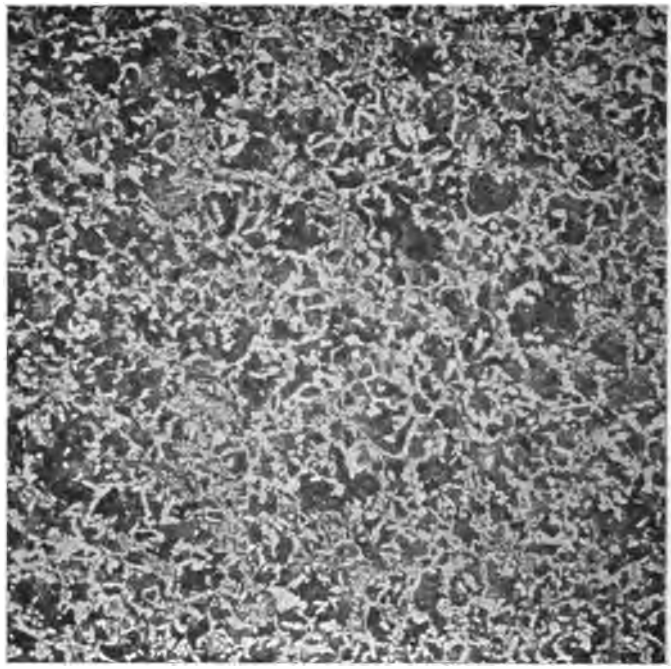


FIG. 18.—SPECIMEN A4. × 100.

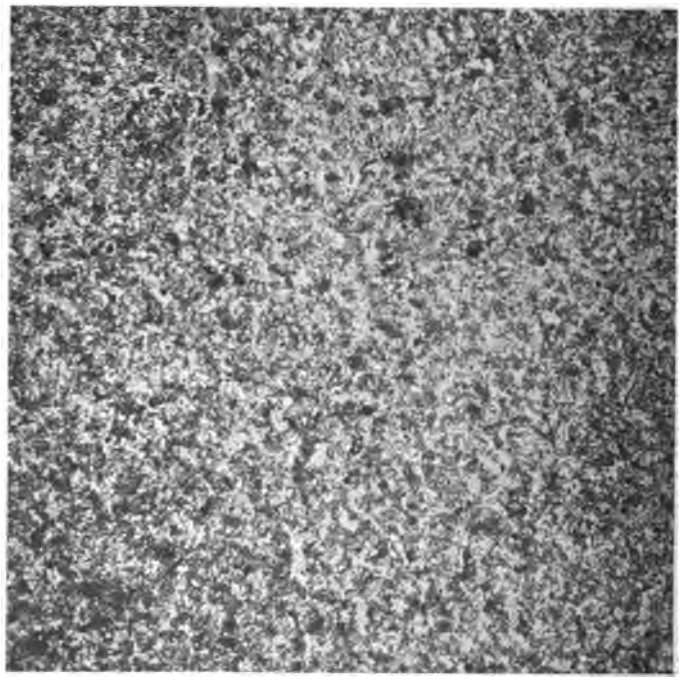


FIG. 19.—SPECIMEN A4A. × 100.

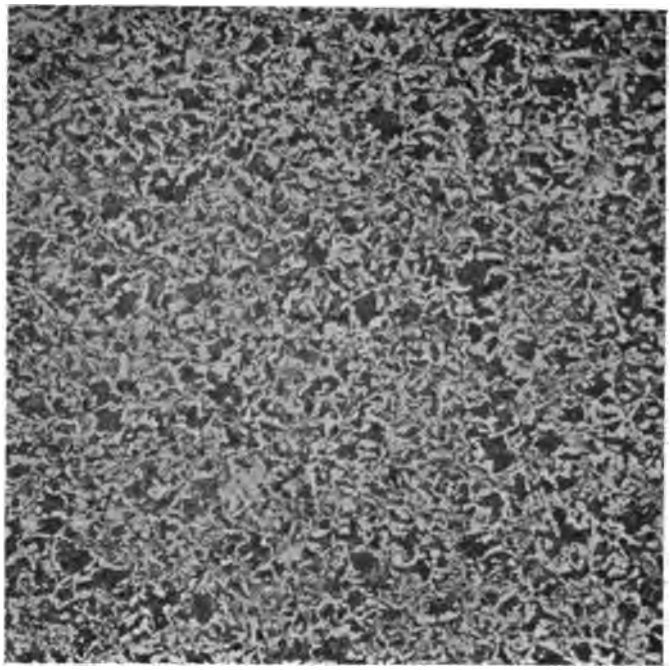


FIG. 20.—SPECIMEN A5. × 100.

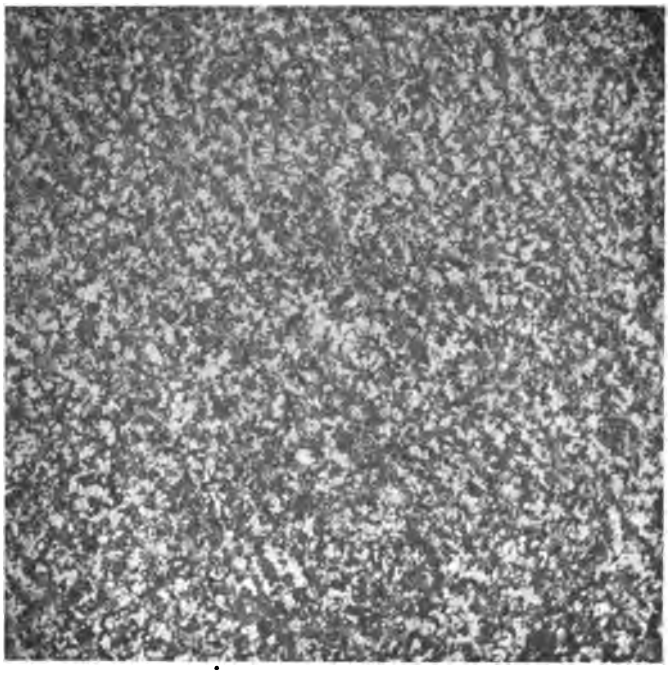


FIG. 21.—SPECIMEN A5A. × 100.

evenly distributed. In the annealed specimens the influence of copper is not so marked as in the unannealed specimens.

With rapid or moderately rapid cooling copper appears to favor the formation of finer-grained pearlite, or, to use the words of Stead,[23] "more diffused carbide." This tendency increases with increasing percentages of copper. This, together with the hardening effect upon the ferrite of the copper, explains the greater hardness of copper steels. In the annealed specimens the tendency to reduce the size of the pearlite grains is not so marked.

Up to a certain limit, copper forms with iron a homogeneous alloy which is hardly distinguishable under the microscope from pure iron. Above this limit, it appears as metallic copper enveloping the grains of ferrite.

Stead[24] says that the maximum amount of copper which forms a homogenous alloy with pure iron is 8.0 per cent. This alloy appears perfectly "free from copper-coloured constituent when examined under the microscoscope." The amount of copper which will harden the ferrite in steels will depend upon the amount of iron that remains in excess of what goes to form cementite with the carbon present, or, in other words, the amount of pure iron that is free to alloy with copper. Hence, all other conditions being equal, the greater the percentage of carbon in a steel, the less the amount of copper it is capable of taking up in solution. Accordingly, the amount of copper must be less in proportion as the carbon in the steel increases, in order that an alloy which is homogenous is formed. The maximum amount of copper is limited for steel by its carbon content, and if the copper exceeds this maximum amount metallic copper becomes apparent in the steel. Stead[25] also found that in such cases, "fracture generally follows the cupreous envelopes," which apparently afford planes of weakness. Thus excess of copper would be a source of weakness rather than of strength. Breuil[26] says that "the presence of copper increases the quantity of pearlite in the steels, and to some extent it causes the steels to be more highly carburised, and consequently harder. With copper up to 4.0 per cent. these steels contain no free copper, that element being in solution in the iron."

Copper, in fact, acts in a two-fold manner, as has been previously noted: 1. It tends to distribute the carbide more evenly, thus tending to produce a finer-grained structure. 2. It hardens the ferrite by forming an iron-copper alloy which is harder than pure iron.

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[23] *Journal of the Iron and Steel Institute*, vol. lx. (1901, II), p. 119.
[24] *Idem*, p. 112.
[25] *Idem*, p. 112.
[26] *Idem*, vol. lxxiv. (1907, II), p. 41.

With increasing percentages of copper, according to various authorities, more and more metallic copper makes its appearance, starting first with fine films surrounding the grains and as filaments traversing the crystals of ferrite. This becomes more and more obvious with increasing copper content until it takes the form of globules which are mechanically suspended in the solid metal.

According to Stead,[27] the formation of these globules is also in some measure dependent upon the rate of solidification, being highest when rapidly cooled and lowest when slowly cooled. This produces heterogeneity in the structure of the mass.

According to Burgess and Aston,[28] the structure of copper steels "is a fine pearlite, with a fibrous cementite which is liberated with increasing copper." This explains the increasing hardness. The hardness does not result through a lowering of the points of transition.

The same view is also held by Wedding.[29]

Critical Points of the Forged Bars.—The critical points were determined upon specimens, cut from the larger bars, by means of a Le Chatelier thermo-couple and a Siemens-Halske galvanometer of the suspension type. The time interval was taken for each tenth of a millivolt, by means of two stop watches. The curves for the eight specimens are shown in Fig. 22.

One long arrest is noticeable in each, corresponding to Ar_1, or the recalescence point. The point Ar_1 is lowered from 736° ± 5° C. in the steel containing no copper to 644° ± 5° C. in the steel containing 4.512 per cent. of copper. The points Ar_2 and Ar_3 are obscure in all the curves. This may be attributed to the percentage of carbon contained in the steel, as these three points are practically coincident in steels containing more than 0.30 per cent. of carbon.

Breuil [30] says that "copper lowers the recalescence point of steels, but does not lower it below 550° C." Müller [31] found that the arrest points of iron are lowered from 60° to 80° by the presence of copper. This explains why a steel containing copper is more readily hardened than one not containing that element, and in this respect the effect of copper resembles that of carbon, though to a less extent.

Tensile Tests.—Tensile tests were made upon: 1. Steel as forged, for which test pieces were turned from the forged bars. 2. Steel annealed. Eight pieces, in the rough before turning, were annealed

[27] *Journal of the Iron and Steel Institute*, vol. lx. (1901, II), p. 115.
[28] *Transactions of the American Electrochemical Society*, vol. xvi., p. 246 (1909).
[29] *Stahl und Eisen*, vol. xxvi., No. 23, p. 1444 (Dec. 1, 1906).
[30] *Journal of the Iron and Steel Institute*, vol. lxxiv. (1907, II), p. 13.
[31] *Stahl und Eisen*, vol. xxvi., No. 24, p. 1493 (Dec. 15, 1906).

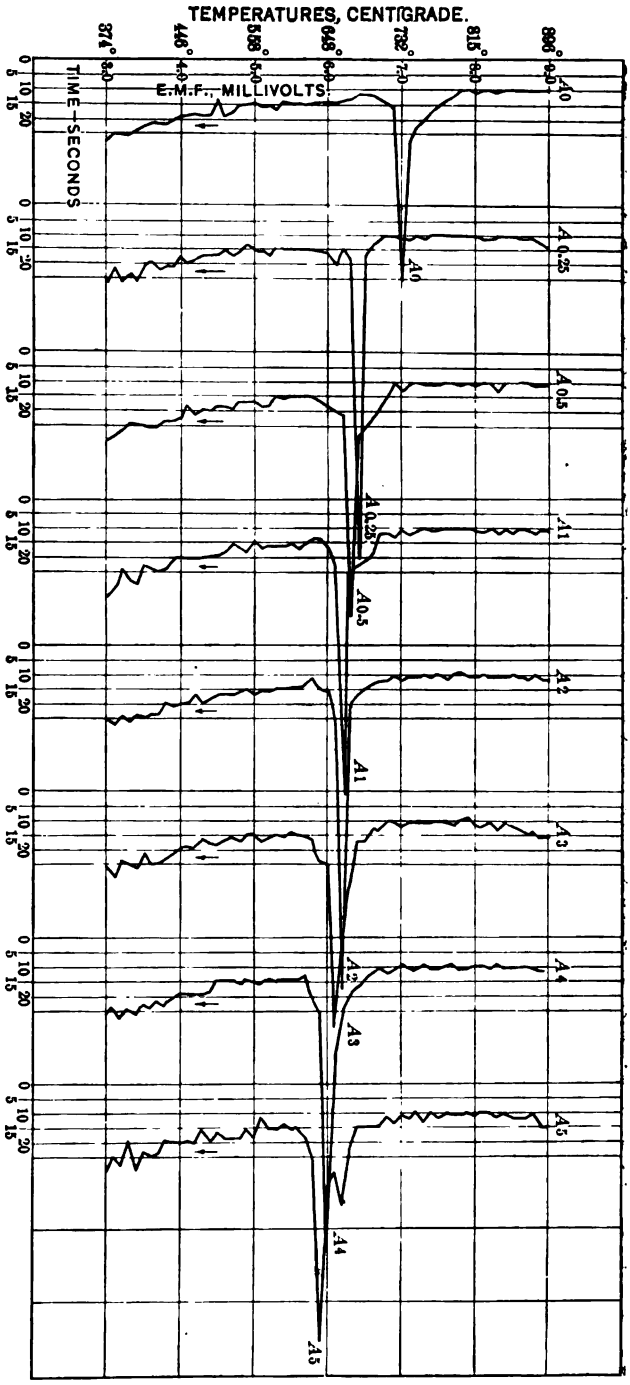

FIG. 22.—COOLING CURVES OF COPPER STEELS.

in a gas furnace in a sealed muffle so as to exclude air so far as possible. The temperature was raised very slowly to avoid local overheating. Finally, after two hours the maximum temperature of 820°, as measured by a Le Chatelier thermo-couple, was reached. The eight bars were all annealed at the same time, so that each bar was subjected to exactly the same conditions. The bars were then allowed to cool in the furnace over night. The test pieces for the second series of tests were turned from this annealed steel. The test pieces were of the shape and had the dimensions shown in Fig. 23. The diameter was measured accurately three or four times at different points with a micrometer gauge and the mean of these results was taken. An electrically driven 100,000-lb. Tinius Olsen testing machine was used for pulling the bars. Photographic views of the test pieces after fracture are shown in Figs. 24 and 25.

We have taken the elastic limit as being the point where the test piece begins to show a permanent set, as indicated by the drop of the beam, without any increase of stress. Ultimate strength has been

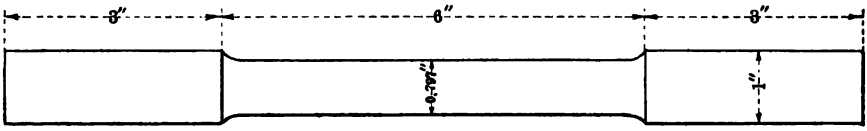

FIG. 23.—SHAPE AND DIMENSIONS OF TEST PIECES.

taken as being the maximum stress which the test piece sustains without breaking, and the ultimate breaking strength as the actual pressure exerted upon the test piece at the moment of rupture. Elongation was measured on 5 in. Reduction in area was measured at the point of fracture.

Table III gives the results for the elastic limit, ultimate strength, breaking strength, elongation, and reduction in area for both the annealed and the unannealed specimens.

The elastic limit in both the annealed and the unannealed specimens increased steadily with increasing percentages of copper. Reference to the curves, Fig. 26, shows that in the case of the annealed steels, from A0 to A0.25, the increase in elastic limit is rather rapid and then it becomes more gradual and more regular. From A1 to A5 the curve becomes practically a straight line. The increase in the elastic limit is greater in the forged specimens than in those annealed. Although the carbon content of A4 is somewhat lower than that of A0 and that of A5 is about the same, yet the elastic limit in the case of A4 is increased more than 100 per cent. and in A5 a still greater amount.

FIG. 24.—UNANNEALED TEST PIECES AFTER FRACTURE.

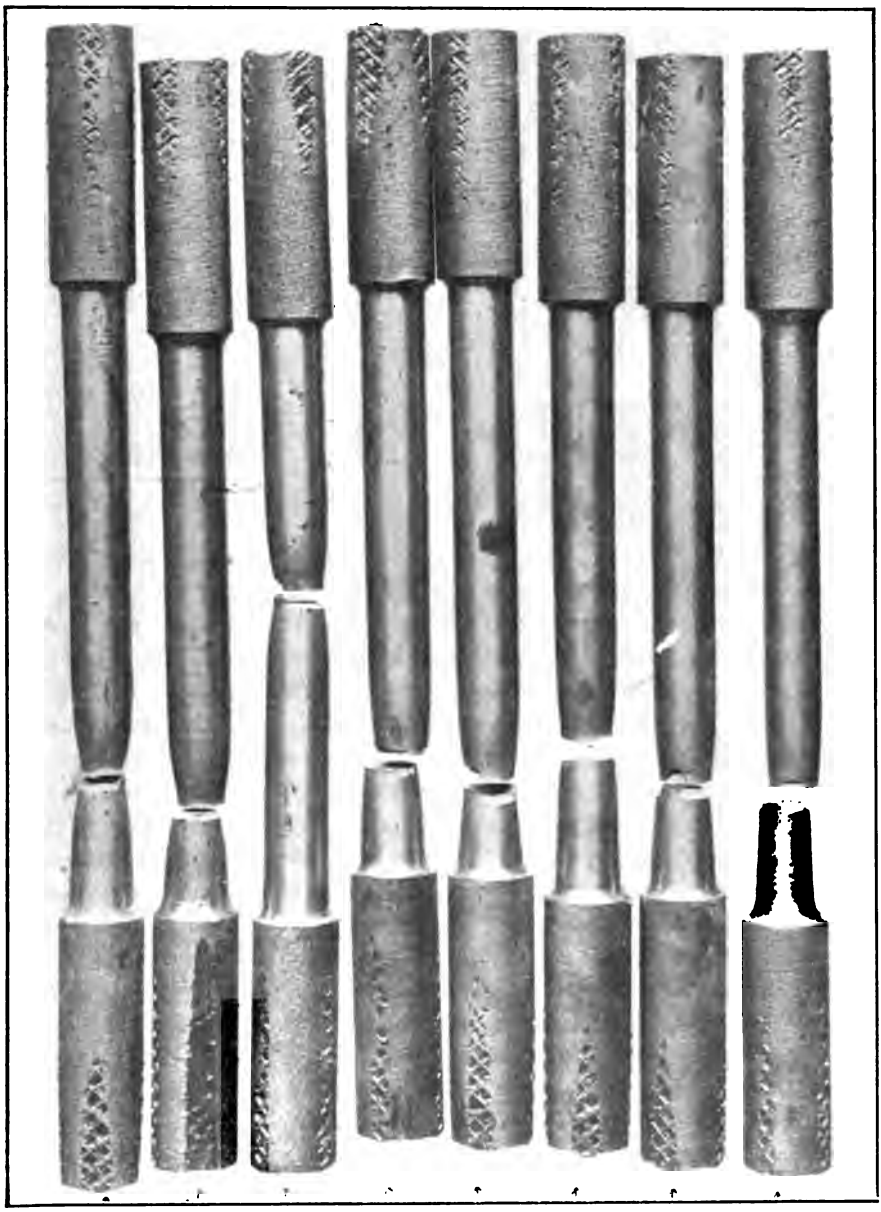

FIG. 25.—ANNEALED TEST PIECES AFTER FRACTURE.

TABLE III.—*Results of Physical Tests of Steel as Forged and After Annealing.*

Unannealed.

Mark.	Copper. Per Cent.	Elastic Limit. Pounds per Square Inch.	Ultimate Strength. Pounds per Square Inch.	Elongation on 8 In. Per Cent.	Reduction of Area. Per Cent.	Breaking Strength. Pounds per Square Inch.	Remarks.
A0	0.0	51,600	86,700	20	33.2		
A0.25	0.165	53,300	99,500	10	8.1		
A0.5	0.493	60,300	99,000	17.5	21.7		
A1	0.846	60,300	97,400	20	29.3		
A2	1.857	84,000	120,660	10	4.0		
A3	2.773	94,600	124,300	10	12.3		
A4	3.574	106,500	111,300	2.5	0.804	Same as Ultimate Strength.	
A5	4.512	114,400	134,000	2.5	3.54		Showed flaw in center.

Annealed.

Mark.	Copper. Per Cent.	Elastic Limit. Pounds per Square Inch.	Ultimate Strength. Pounds per Square Inch.	Elongation on 8 In. Per Cent.	Reduction of Area. Per Cent.	Breaking Strength. Pounds per Square Inch.	Remarks.
A0	0.0	39,000	75,200	25	54.2	62,000	
A0.25	0.165	48,300	93,500	16.25	48.25	80,600	
A0.5	0.493	49,000	94,000	20	31.4	87,900	
A1	0.846	55,000	87,150	15	37.6	79,400	Showed flaw after breaking.
A2	1.857	61,700	96,600	13.7	37.9	88,350	
A3	2.773	66,000	89,999	16.35	38.4	84,300	
A4	3.574	68,000	99,980	16.25	37.0	90,760	
A5	4.512	74,200	110,000	17.5	43.8	85,300	

The ultimate strength curve, Fig. 27, rises with the increase in the percentage of copper, and up to A2 the general trend of the curves for both the annealed and the unannealed specimens is the same; at A1 there is a marked depression, due to a flaw in the center of the test piece. The break at A3 on the annealed curve is rather unexpected and appears to be abnormal, but it may be due to the fact that this specimen has the lowest carbon content of the whole series. A0.25 gives a better ultimate strength in the unannealed condition than A0.5, but the latter gives a higher value than the former although the carbon content of A0.25 is greater than that of A0.5 by nearly 0.1 per cent. A1 is appreciably lower in carbon than its predecessors and hence the depression on both the curves at this point. The behavior of A5 unannealed is remarkable. It gives very satisfactory tests despite the fact that the test piece had a flaw at the center.

In general, our results show quite close agreement with those of other recent investigators. Within the limits investigated the ultimate strength appears to increase almost in direct proportion to the amount of copper present. The ultimate strength of A0 unannealed was 86,700 lb. per square inch, while A5 gave 134,000 lb. per square inch—an increase of more than 50 per cent. In the annealed specimens the minimum ultimate strength is 75,200 lb. per square inch in A0, rising to 110,000 lb. per square inch in A5. This shows an increase of 50 per cent.

The elongation, Fig. 28, falls abruptly from A0 to A0.25 and then increases in A0.5. A1, untreated, has a better elongation than A0.5, but this is not strictly comparable on account of the unavoidable difference in the temperatures at which the specimens were forged.

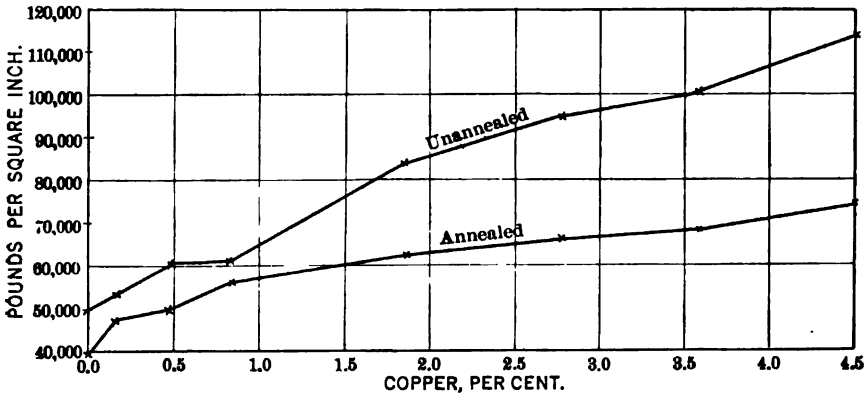

FIG. 26.—ELASTIC LIMIT CURVE.

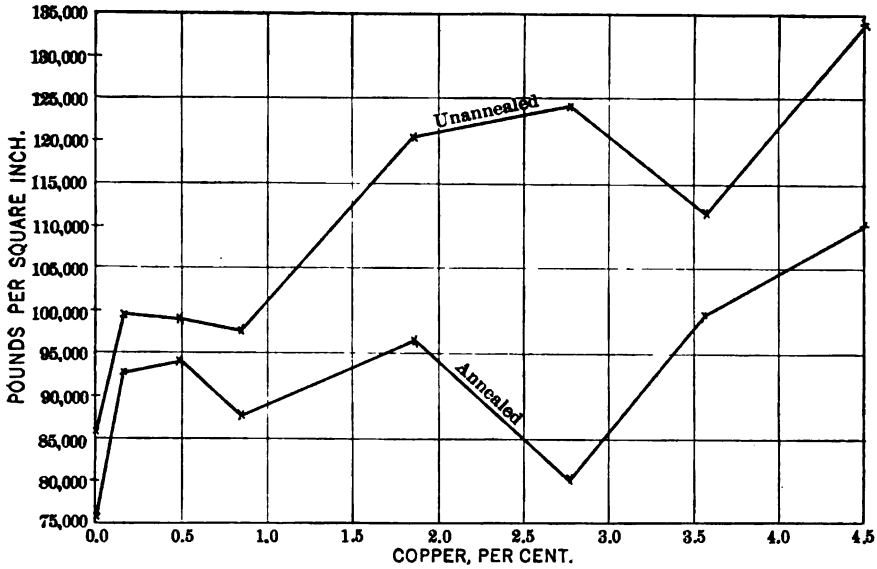

FIG. 27.—ULTIMATE STRENGTH CURVE.

The minimum elongation in the annealed series takes place at A2, after which it increases quite steadily. With the unannealed specimens the minimum elongation occurs at A4 and A5 and the general trend of the curve shows a gradual decrease of elongation, which indicates

that the steel becomes more brittle as the proportion of copper increases. In the annealed specimens the minimum elongation, 13.7 per cent., was with A2. A5, containing 4.512 per cent. of copper, shows a much better elongation, 17.5 per cent.

Examination of the results for the reduction of area of the unannealed specimens does not lead to any definite conclusion. With the annealed specimens the reduction of area decreases up to A0.5, and above that point increases and finally reaches 43.8 per cent. in A5.

With the unannealed specimens the actual breaking strength in all cases was the same as the ultimate strength. The actual breaking strength in the case of the annealed specimens shows a regular in-

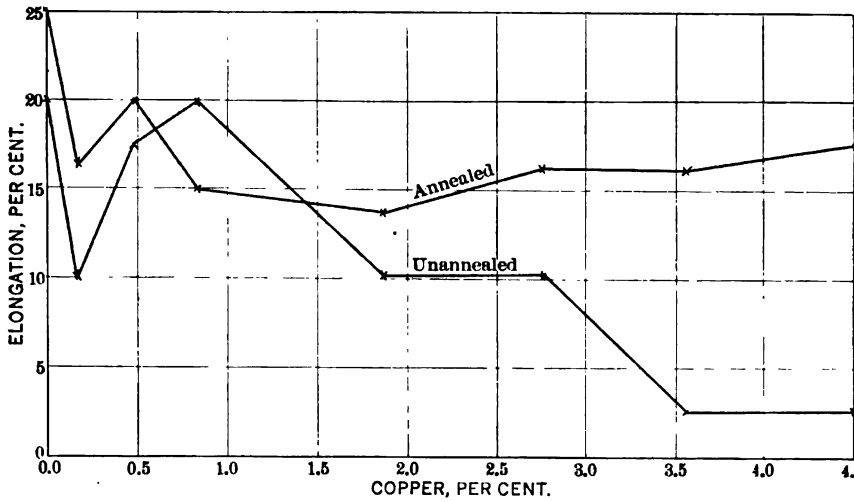

FIG. 28.—ELONGATION CURVE.

crease with increase of copper, except with A1, which, on account of a flaw in the center, gave a lower value than is to be expected. A3 gave a lower result than A2, probably on account of the lower percentage of carbon.

A5 shows the highest elastic limit and ultimate strength, both annealed and unannealed. The elongation and reduction of area in the annealed condition are satisfactory, although they are not so good in the unannealed condition. A4, annealed, ranks second in the series in elastic limit, ultimate strength, and breaking strength. It also shows a good elongation and reduction of area, but, as might be expected, it has a lower ultimate strength in the unannealed condition than A2 and A3. It shows the minimum elongation and reduction

of area. Breuil's[32] 4 per cent. copper specimen corresponds rather closely to A5 of our series.

For the purpose of comparison with copper steel, a number of results of tensile tests upon the alloy steels already in very general use are of interest.

Howe[33] gives the following results upon one of the hardest brands of Brooklyn chrome steel (chromium, 0.38; combined carbon, 0.90; manganese, 1.89; silicon, 0.03; tungsten, 0.98 per cent.):

Tensile Strength. Pounds per Square Inch.	Elongation. Per Cent.	Reduction of Area. Per Cent.
134,000	2	0

Döhlen[34] (chromium, 0.50; combined carbon, 0.91 per cent.):

Tensile Strength. Pounds per Square Inch.	Elongation. Per Cent.	Reduction of Area. Per Cent.
122,300	.....	15.7

Blair and Smith[35] (chromium, 0.38; combined carbon, 0.84; graphite, 0.01; manganese, 0.22; silicon, 0.09 per cent.):

Tensile Strength. Pounds per Square Inch.	Elongation. Per Cent.	Reduction of Area. Per Cent.
110,572	.....	.....

This specimen of chrome steel has about the same tensile strength as A5 annealed.

Manganese steel, forged.—Hadfield[36] (carbon, 1.60; silicon, 0.26; manganese, 19.10; phosphorus, 0.09 ±; sulphur, 0.06 ± per cent.):

Natural State.		Water Toughened.
Tensile Strength. Pounds per Square Inch.	Elongation. Per Cent.	Tensile Strength. Pounds per Square Inch.
116,480	1	132,160

Nineteen per cent. manganese steel with 1.6 per cent. of carbon, quenched in water, has less tensile strength than A5 copper steel, forged.

Nickel steels.—Riley[37]:

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[32] *Journal of the Iron and Steel Institute*, vol. lxxiv. (1907, II), p. 17.

[33] *Metallurgy of Steel*, p. 76 (1890).

[34] Ledebur: *Handbuch*, p. 261; also Howe: *Metallurgy of Steel*, p. 76 (1890).

[35] *Report of U. S. Board to Test Iron*, vol. ii., p. 590; also Howe: *Metallurgy of Steel*, p. 76 (1890).

[36] *Journal of the Iron and Steel Institute*, vol. xxxiii. (1888, II), Excerpt *Proceedings of the Institution of Civil Engineers*, vol. xciii. (1888), p. 40 *et seq.*; also Howe: *Metallurgy of Steel*, p. 361 (1890).

[37] *Engineering*, vol. xlvii., p. 574 (May 17, 1889); also Howe: *Metallurgy of Steel*, p. 370 (1890).

Nickel. Per Cent.	Carbon. Per Cent.	Manganese. Per Cent.	Tensile Strength, Rolled. Pounds per Square Inch.	Elastic Limit, Rolled. Pounds per Square Inch.	Elongation on 4 in. Per Cent.	Contraction, Rolled. Per Cent.
1.0	0.42	0.58	129,024	71,904	11	24
5.0	5.0	0.34	116,480	69,664	15.6	14

Tungsten steel.—Howe[38] (Mushet's Special: tungsten, 7.81; carbon, 1.99; silicon, 0.09; manganese, 0.19 per cent.):

Tensile Strength. Pounds per Square Inch.	Elongation. Per Cent.
146,400	0

Hardness.—Below are given the tests for hardness as indicated by the Shore scleroscope upon the unannealed specimens:

Steel.	Hardness.	Steel.	Hardness.
A0	28	A2	37
A0.25	31	A3	37
A0.5	29	A4	40
A1	28	A5	43

For the purpose of comparison the hardnesses of other well-known steels as indicated by this instrument are given.

	Annealed.	Hardened.
Mild steel, 0.15 per cent. carbon	22	30–45
Tool steel, 1.0 per cent. carbon	30–35	40–50
Vanadium steel	35–45	.....
Chrome-nickel steel	47	60–95
High-speed steel	.....	70–105

A rough qualitative test of the hardness of our copper steels was obtained by observing their behavior while being sawed and turned. In general, the unannealed specimens seemed to increase in hardness in proportion to the percentage of copper present. The hardening effect of the copper was not very marked in the annealed specimens, all of which seemed to work about the same in the lathe.

The hardening effect of copper has been observed by all investigators, both early and recent. The hardening effect of copper upon steel, as has already been pointed out, is due to two distinct causes: 1. It retards the formation of pearlite and hence there is more diffused cementite, thereby making the constituents of the steel finer. 2. It hardens the ferrite itself by forming with it an alloy which is harder than ferrite alone.

Stead[39] prepared an alloy with 8 per cent. of copper and 92 per

[38] *Metallurgical Review*, vol. ii., p. 441; also Howe: *Metallurgy of Steel*, p. 81 (1890).

[39] *Journal of the Iron and Steel Institute*, vol. lx. (1901, II), p. 113.

cent. of iron which was said to be as hard as forged steel containing 1.5 per cent. of carbon.

Roush[40] carried out hardness determination on two series of alloys, one with from 0.25 to 45 per cent. of nickel and the other with from 0.1 to 8.5 per cent. of copper. They were tested both as annealed and as forged. The addition of nickel gave a maximum hardness when present between 10 and 20 per cent., while the addition of up to 8 per cent. of copper to iron was said to give a gradual but irregular increase in hardness.

The term "hardness" has been used rather loosely without much scientific significance. It must be borne in mind that the hardness we are dealing with at present is different from the hardness of the mineralogist. Turner[41] has suggested that it be called "tensile hardness," since it closely follows the tensile tests.

It will be noted that the results which we give for hardness correspond fairly well with our tensile tests given in Table III. The Brinell tests for hardness given by Breuil[42] correspond with his tensile tests upon the same specimens. This agreement has been observed by several other investigators.

Electrical, Magnetic, and Other Properties of Copper Steel.—We were unable to investigate the electrical and magnetic properties of the series of copper steels which we made; but in connection with our other data the results of others along this line will be of interest.

Burgess and Aston,[43] experimenting upon the conductivity of alloys of iron and copper, found that there is no appreciable change in the conductivity of the alloy up to a copper content of 7.0 per cent. The resistance is slightly greater than that of pure iron. With alloys having more than 90 per cent. of copper the resistence is less than half that of pure iron. The presence of small amounts of iron in copper greatly lowers its conductivity. Conductivity is increased by annealing.

Fric,[44] who investigated the electrical properties of some of the steels made by Breuil, found that copper increased the electrical resistance but "there is a maximum resistance beyond which the resistance ceases to increase."

Stead and Wigham[45] give the following table for the relative resistance of cupreous and non-cupreous steels used for wire making:

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[40] *Journal of the Iron and Steel Institute*, vol. lxxxii. (1910, II), p. 516.

[41] *Idem*, vol. lx. (1901, II), p. 139.

[42] *Idem*, vol. lxxiv. (1907, II), p. 35.

[43] *Transactions of the American Electrochemical Society*, vol. xx., p. 205 (1911); also *Metallurgical and Chemical Engineering*, vol. viii., No. 2, p. 79 (Feb., 1910).

[44] *Journal of the Iron and Steel Institute*, vol. lxxiv. (1907, II), p. 70.

[45] *Idem*, vol. lx. (1901, II), p. 135.

	Resistance per Mile. Ohms.	Relative Resistance. Normal.	Cupreous.
A	1399.2	100	107.0
B, Cupreous	1499.5		
C	1269.3	100	96.0
D, Cupreous	1318.9		
E	1271.7	100	99.8
F, Cupreous	1267.5		
G	1320.0	100	95.8
J, Cupreous	1264.7		
N	1437.9	100	110.0
X, Cupreous	1580.5		

In two cases copper effected an increase and in three cases a decrease in the electrical resistance. However, they were not very sure of their results. The samples investigated were No. 25 gauge copper steel wire and the conductivity was measured at 70° F.

Dillner [46] found that the magnetic properties of iron are not affected by 0.64 per cent. of copper, which decreases the conductivity of the iron as of other metals.

Brown,[47] determinations of the densities and specific heats of some alloys of iron, found that copper decreases the specific volume (reciprocal of density) about 0.0005 cc. for every 1 per cent. of added copper, but the specific volume of the alloy is not changed to any appreciable extent by further additions of copper up to about 4 per cent. Specific heat of iron with high percentages of carbon is not changed by chromium, cobalt, and copper when added separately.

Conclusions.

The following conclusions are given as a result of our work upon copper steel.

1. In making copper steel by the crucible process it is necessary to "kill" the metal before adding the copper, and just before pouring it is advisable to add a small amount of some deoxidizing agent.

2. In forging the ingots, containing from 0.437 to 0.605 per cent. of carbon, traces of "red-shortness" began to be shown with a copper content of 4.512 per cent. Up to this point the steel forged about like machine steel.

3. Up to 0.846 per cent. of copper our steels gave satisfactory welds. At 1.857 per cent. the weld was much weaker and above this point the steel could not be welded.

4. There appears to be a marked tendency for copper to eliminate sulphur.

[46] *Stahl und Eisen*, vol. xxvi., No. 24, p. 1493 (Dec. 15, 1906).

[47] *Scientific Transactions of the Royal Dublin Society*, p. 68, Aug. 23, 1907.

5. In the series of steels studied the segregation of copper toward the bottom of the ingot begins to be slightly noticeable with 0.846 per cent. of copper. With 2.773 per cent. of copper the segregation is more noticeable and with 4.512 per cent. of copper it is very marked.

6. The presence of small percentages of copper up to 0.493 per cent. has a most marked effect in preventing the corrosion of steel by dilute sulphuric acid. As the percentage of copper increases, the loss by corrosion increases until with 2.773 per cent. of copper the loss is greater than when the steel does not contain copper.

7. As the percentage of copper increases the structure becomes finer, due to the more even distribution of fibrous cementite. This is more apparent in the forged than in the annealed specimens.

8. The point Ar_1 was lowered from 730° C. in the steel containing no copper to 635° C. in the steel containing 4.512 per cent. of copper.

9. The elastic limit in both the annealed and the unannealed specimens increases steadily until with 4.512 per cent. of copper it was increased more than 100 per cent. over that of steel containing no copper.

Within the limits investigated the ultimate strength appears to increase almost in direct proportion to the amount of copper present.

With the unannealed specimens the actual breaking strength in all cases was the same as the ultimate strength. The actual breaking strength in the case of the annealed specimens shows an increase with the increase of copper.

In the annealed specimens the elongation is somewhat irregular, but in general decreases up to 1.857 per cent. of copper and then increases and reaches a maximum at 4.512 per cent. of copper.

The general tendency in the annealed specimens is to decrease the elongation up to 1.85 per cent. of copper and then up to 4.512 per cent. of copper to gradually increase it. In the unannealed specimens the general tendency is to decrease the elongation throughout the series.

10. The hardness of the steel increases with increasing copper content and corresponds fairly well with the tensile tests.

Bibliography and Symposium of Opinion.

Arranged Chronologically.

1. Louis Savot 1627) [*Jour. I. and S. Inst.*, 1889, I, p. 130] stated in his book that copper made iron brittle, and mentioned the difficulties experienced by "smiths" in working iron containing copper. He referred to a still older authority, Budelius, who held that copper rendered iron incapable of being welded.

2. Jars (1774) [*Voyages Métallurgiques*, Lyons, 1774, vol. i., p. 4; also Ball and Wingham, *Jour. I. and S. Inst.*, 1889, I, p. 123] comments, "It is generally thought that copper

is a pest for iron," but he adds that he had been told by Cramer that the quality of iron was improved by small quantities of copper, and that even 1 per cent. of copper had no deleterious effect on the welding properties of the metal.

3. Rinmann (?) [Percy's *Iron and Steel*, 1864, p. 147] strongly heated, in a blast furnace, a crucible containing a mixture of 5 parts of iron and 1 part of copper. It was hard and tough and could only be broken with difficulty. He says "it cannot be denied that the presence of copper in bar-iron causes incurable red-shortness."
4. Faraday and Stodart (1820) [*Quart. Jour. of Science, Lit. and the Arts*, 9, p 329, 1820; also *Philos. Trans.*, 1822, p. 266] melted steel with 2 per cent. of copper, but the quality of the metal did not appear to have improved at all.
5. Mushet (1835) [*Phil. Mag.*, 6, 1835, p. 81] found that a 50 per cent. alloy of copper with malleable iron possessed great strength, becoming harder with an increasing percentage of iron. Steel melted with 5 per cent of copper was considerably hardened but could not be forged. He concluded as a result of his experiments that copper unites with iron more freely as the percentage of carbon decreases.
6. Stengel (1837) [*Karsten's Archiv.*, 10, 1837, p. 714; also Percy, *Iron and Steel*, p. 151] states that a much smaller percentage of sulphur than of copper will make iron red-short and that 0.1 per cent. of sulphur is more injurious to the mechanical properties of iron than 0.75 per cent. or more of copper.
7. Karsten (?) [*Eisenhüttenkunde*, 1, p. 498; also Percy, *Iron and Steel*, p. 148] stated that iron would take up but a very small amount of copper, which made it red-short.
8. Longmaid (1861) [Patent No. 1863, A. D. 1861] was said to have a patent for an alloy consisting of 2.5 to 10 lb. of copper to 1 ton of iron, which was supposed to possess unusual hardness.
9. Eggertz (1862) [*Jahres Bericht*, Wagner, 1862, p. 9; also Percy, *Iron and Steel*, pp. 149, 153; also Howe, *Met. of Steel*, p. 83] found that wrought iron containing 0.5 per cent. of copper showed only traces of red-shortness.
10. Percy (1864) [Percy's *Iron and Steel*, 1st Ed., p. 149] who experimented with alloys of iron and copper, states that the two metals are miscible in all proportions.
11. Willis (1880) [*Jour. I. and S. Inst.*, 1880, I, p. 93] found that 0.1 per cent. of copper did not produce any appreciable effect on the quality of steel.
12. Wasum (1882) [*Stahl u. Eisen*, 1887, p. 193; *Jour. of the Chem. Soc.*, 1883, p. 404; *Jour. I. and S. Inst.*, 1882, I, p. 369; also *E. and M. Jour.*, 1884, vol. xxxviii, p. 5] made experiments in which he investigated the effect of copper alone, sulphur alone, and the two together upon steel. He concluded that, up to 0.862 per cent., copper did not develop a trace of red-shortness. Copper and sulphur combined did not appear to produce red-shortness unless the percentage of sulphur is sufficient itself to cause it. He regarded from 0.15 to 0.16 per cent. as the limit for sulphur.
13. Choubley (1884) [*Bulletin de la Société de l' Industrie Minérale*, xiii., 205; *Jour. I. and S. Inst.*, 1884, I, p. 248; also *E. and M. Jour.*, 1884, vol. xxxviii., p. 5] confirms the observation of Wasum on the rolling qualities of copper steel, and found that even 1 per cent. of copper in the absence of sulphur did not produce red-shortness.
14. Holtzer (1889) [Howe, *Met. of Steel*, p. 368; also *E. and M. Jour.*, 1890, vol. 1, p. 426] exhibited some copper steels at the Paris Exhibition of 1889 containing from 3 to 4 per cent. of copper. They were remarkable for extremely high elastic limit, especially of the hardened bars, which went up to 142,228 lb. per sq. inch.
15. Brustlein (1889) [Howe, *Met. of Steel*, p. 368; also *E. and M. Jour.*, 1890, vol. 1, p. 426] says that steels with more than 1 per cent. of copper were decidedly red-short and had no future, that copper did not make a thorough mixture with steel, and that it favored the formation of blow-holes.
16. Ball and Wingham (1889) [*Jour. I. and S. Inst.*, 1889, I, p. 123; also *Jour. Franklin Inst.*, July, 1890, p. 35] observed in the course of their experiments that the presence of carbon seemed to favor the more intimate mixture of copper with iron. Some of their specimens with small percentages of carbon and copper worked well both cold and hot. They found that copper makes iron and steel extremely hard, and that

"within certain limits copper does not prejudicially affect the mechanical properties of steel."

17. Riley (1890) [*Jour. I. and S. Inst.*, 1890, I, p. 123] observes that copper is not really alloyed with iron, but exists disseminated throughout the metal. If aluminum is used in making the alloy, a perfect mixture is obtained.
18. Schneider (1890) [*E. and M. Jour.*, 1890, vol. l., p. 426] obtained a patent upon a method of alloying copper with steel. His alloys were remarkable for great strength, tenacity, and malleability. They were claimed to be specially useful in the manufacture of ordnance, armor plate, gun tubes, projectiles, etc. These alloys contained between 5 and 20 per cent. of copper.
19. Bauerman (1890) [*Treatise on Met. of Iron*, p. 50, ed. 1890] says that copper and iron can be melted together in all proportions, but doubts the formation of a homogeneous alloy
20. Scranton (1891) [Howe, *Met. of Steel*, p. 83, ed. 1891; also Garrison, *Jour. Franklin Inst.*, Aug., 1891, p. 125], of the Scranton Steel Works, is said to be habitually manufacturing Bessemer T rails with 0.50 to 0.66 per cent. of copper which are not red-short.
21. Howe (1891) [*Met. of Steel*, p. 82, 1891] declares that iron and copper unite in all proportions, and the alloys of iron with small amounts of copper are said to be homogeneous. He also states that the effect of copper resembles that of sulphur to render steel red-short and incapable of being welded.
22. Garrison (1891) [*Jour. Franklin Inst.*, Aug., 1891, p. 121] hopes for a very extensive use of copper steel in the arts and manufactures in view of its high elastic limit combined with a considerable elongation. He says that 0.5 per cent. of copper will alloy with steel without difficulty, but it is doubtful if 10 per cent. would make a homogeneous alloy.
23. Hogg (1893) [*Jour. of Soc. of Chem. Industry*, vol. xii, p. 236; also *Jour. I. and S. Inst.*, 1893, I, p. 388] found that copper does not tend to segregate in steel.
24. Arnold (1894) [*Jour. I. and S. Inst.*, 1894, I, p. 107] had no difficulty in hammering an ingot with 0.1 per cent. of carbon and 1.8 per cent. of copper. He states that copper has a greater influence than manganese in raising the elastic limit.
25. Colby (1899) [*Jour. I. and S. Inst.*, 1900, 1, p. 412; also *Iron Age*, Nov. 9, 1899, pp. 10–11] states that small percentages of copper have no deleterious effect upon steel. He made propeller shafts for U. S. battleships and gun tubes, by forging steel containing copper, which fulfilled the specifications of the government. The tensile strength was not less than 75,000 lb. per square inch, combined with an elastic limit of not less than 36,000 lb. per square inch and an elongation of 20 per cent. in length of 2 in. It stood bending fairly well and could be successfully welded. The copper content of his steel was 0.565 per cent.
26. Williams (1900) [*Iron Age*, Nov. 29, 1900; also *E. and M. Jour.*, vol. lxx., 1900, p. 667] experimented upon the atmospheric corrosion of copper steel and came to the conclusion that the loss due to weathering decreases with the increase of the copper content.
27. Lipin (1900) [*Stahl u. Eisen*, vol. xx., pp 536–541 and 583–590; also *Jour. I. and S. Inst.*, 1900, II, p. 551] observed that copper increases the fluidity of steel and makes it more and more crystalline at the fracture. He found steel to be red-short when the copper reached 4.7 per cent. He is of the opinion that tool steel may contain copper up to 1 per cent. and and suggests the quenching of cupreous tool steels in oil instead of water in the process of hardening. He concludes that the presence of copper need not cause apprehension, although there may not be any advantage in its presence. He also found that as the percentage of carbon increased, the proportion of copper must be reduced, otherwise the metal cracked during working.
28. Ruhfus (1900) [*Stahl u. Eisen*, vol. xx., p. 691; also *Jour. I. and S. Inst.*, 1900, II, p. 554] gives from 0.4 to 0.5 per cent. of copper as the limit beyond which red-shortness becomes evident.

29. Stead and Evans (1901) [*Jour. I. and S. Inst.*, 1901, I, p. 89] concluded as the result of their investigations that "Copper has no more right to have the character of making steel red-short than carbon," that "between 0.5 and 1.3 per cent. copper had no deleterious effect on either the hot or cold property of steel," and that "a very large amount (2.0 per cent.) makes the steel more liable to be over-heated" In small quantities it slightly raises the tenacity and the elastic limit.
30. Stead and Wigham (1901) [*Jour. I. and S. Inst.*, 1901, II, p. 122] experimenting on the effect of copper on steel for wire making, found that 1.28 per cent. of copper has apparently no effect when the copper content is low. They think that the copper-iron portion of the steel is distributed unequally and may have more deleterious effect on the mechanical properties of the steel in proportion as the carbon content is higher. They are of the opinion that copper in higher-carbon steel for wire making should be avoided. They also found that copper helps to retard the corrosion in steels.
31. Stead's (1901) [*Jour. I. and S. Inst.*, 1901, II, p, 104] exhaustive research on copper and iron alloys proves that the amount of carbon present in the iron determines the amount of copper that can be alloyed with it; that some of the copper is held mechanically suspended in the alloy in the form of globules, and this depends on the rate of solidification of the alloy. He concludes that "Copper in foundry iron need not be feared, as its only effect appears to be that of raising its tenacity."
32. Wigham (1906) [*Jour. I. and S. Inst.*, 1906, I, p. 222]. In America and Germany steels which contain appreciable amounts of copper have been used for different purposes.
33. Wigham (1906) [*Jour. I. and S. Inst.*, 1906, I, p. 222] believes that copper is very difficult to alloy with steel. In steel it is not of practical value to use more than 0.6 per cent., and that copper up to 0.25 per cent. is no disadvantage in the making of the best classes of steel wire.
34. Pfeiffer (1906) [*Metallurgie*, vol. iii., pp. 281–287; also *Jour. I. and S. Inst.*, 1906, IV, p. 908] thinks that copper remains suspended in fine particles throughout the mass of metal and that copper is liable to be injurious when it exists in sufficient quantities to cause non-uniformity in the structure of the metal.
35. Dillner (1906) [*Stahl u. Eisen*, Dec., 1906; *Bihang till Jernkontorets Annaler*, 1906, pp. 357–378; also *Kungl. Tekniska Högskolans Material-profningsanstalt*, 1896–1906, Stockholm, Lindstáhl] observed that 0.62 per cent. of copper is entirely harmless in a soft steel, although it increases the brittleness of a hard steel. Steel containing 1 per cent. of carbon and more than 3 per cent. of copper is not capable of being rolled.
36. Müller (1906) [Wedding, *Stahl u. Eisen*, Dec., 1906] established the fact that iron containing 0.14 per cent of carbon was capable of alloying in all proportions with copper. He also found that within certain limits copper acts beneficially; it diminishes the malleability, also the tendency to red-shortness. It raises the fusibility and the fluidity and increases the hardness and tensile strength. He also found that the absorption of carbon takes place more energetically with increasing percentage of copper. Iron originally rich in carbon is not in condition to receive more than 4.75 per cent. of copper.
37. Campbell (1907) [*The Manuf. and Prop. of Iron and Steel*, 1907, p. 358] says that most of the Bessemer and open-hearth steels mentioned in his book contained from 0.3 to 0.5 per cent. of copper. "This will be sufficient proof that the best steel may contain up to 1.0 per cent. of copper without being seriously affected, but if at the same time sulphur is high, say 0.08 to 0.1 per cent., the cumulative effect is too great for molecular cohesion at high temperatures and it cracks in rolling." He found no difference in the ultimate strength of steels with high or low copper content. "The high copper gives a slightly higher elastic ratio which is a benefit and a better elongation and reduction of area.
38. Breuil (1907) [*Jour. I. and S. Inst.*, 1907, II, p. 1] mentions that Vanderheym, Engineer of the Paris, Lyons, and Mediterranean Railway Co., had "observed the favorable influence exerted by the presence of copper on certain steels, notably those employed for the axles of railway vehicles."

39. Breuil (1907) [*Jour. I. and S. Inst.*, 1907, II, pp. 45–46; also *Comptes Rendus*, vol. cxlii., pp. 1421–1424] concludes his long and exhaustive research by the following statement: "copper steel remains commercially serviceable up to about 4 per cent. of copper." The property of satisfactory rolling is sacrificed with percentages of copper exceeding 4 per cent. Copper steels have greater tensile strength with increasing amounts of copper and this is most noticeable in the case of mild steels. Copper steels equal, from the point of view of tensile strength, nickel steels, and would be less costly than the latter. Copper steels are not more brittle than nickel steels. "What is particularly advantageous is that while hardening the steel the presence of copper does not render it brittle. It confers upon it a very fair degree of elasticity while leaving the elongation good, thus conducing to the production of a most valuable metal."
40. Sahmen (1908) [*Zeitschrift für anorganische Chemie*, vol. lvii., pp. 1–33; also *Jour. I. and S. Inst.*, 1908, I, p. 409] holds that copper and iron are miscible in the molten condition and that there are two series of mixed crystals, from 0.0 to 3.5 per cent. and from 97.3 to 100 per cent. by weight of copper respectively.
41. Burgess and Aston (1909) [*Trans. Am. Electro-Chem. Soc.*, 1909, pp. 241–256; also *Iron Age*, vol. lxxxiv., pp. 1476–1479] conclude as the result of their investigation of the alloys of electrolytic iron and copper that hardness increases with increase of copper up to a limit of from 5 to 7 per cent. There is also increase of tensile strength with the increase of copper. The alloys up to 2 per cent. of copper forged well, at low heats, from 7 to 80 per cent. alloys—non-forgeable. Between 80 and 100 per cent. they forged at a "fair red heat." With less than 5 per cent. of copper the alloys welded easily. Burgess was inclined to believe that the copper-iron alloys were going to be rather corrodible metals.
42. Clamer (1910) [*Jour. I. and S. Inst.*, 1910, II, p. 515; *Amer. Soc. for Testing Materials*, June, 1910] found that the nickel in nickel steel may be replaced by copper without materially altering the physical properties of the metal.
43. Sargent (1912) [*Electro-Chem. and Met. Jour.*, 1910, p. 68] says that red-shortness is to be expected from additions of copper to steel in the ordinary steel making processes on account of the formation of oxides of copper.
44. Editoral (1912) [*E. and M. Jour.*, 1912, vol. xciv., p. 677]. Remarkable durability is mentioned of some rails on the New Haven railroad which upon analysis were found to contain about 0.5 per cent. of copper. Rails with 0.5 per cent. of copper have also been used on the Pennsylvania road for a long time.
45. Buck (1913) [*Jour. of Industrial and Engineering Chemistry*, June, 1913, p. 447], as the result of an elaborate investigation shows that steel containing 0.15, 0.24 and 0.34 per cent. of copper withstands atmospheric corrosion about twice as long and corrosion by dilute sulphuric acid 50 to 100 times as well as ordinary steel.
46. Burgess and Aston (1913) [*Jour. of Ind. and Eng. Chem.*, June, 1913, p. 458] investigated the corrodibility of alloys of electrolytic iron and copper. They report a high resistance to attack by acid and a low atmospheric corrosion for the whole series investigated (copper 0.089 to 7.05 per cent.).
47. Anonymous (1913) [*E. and M. Jour.*, vol. xcv., p. 1211, June 14, 1913]. "Chicago, Milwaukee & St. Paul Ry. officials say that complete reports on the 5,000 tons of steel rails, which the company ordered last year with an alloy of 0.6 per cent. copper, which were laid in various sections of the system, principally in the Northwest, show that not one broken rail has been found in the lot. This is considered a remarkable showing. The company has ordered 10,000 more tons of the same copper-steel rails, and if the result is similar, will adopt this rail as its standard."

Mr. Ray's portion of this work has been submitted in partial fulfillment of the requirements for the degree of Master of Arts at Stanford University.

Discussion.

Allerton S. Cushman,* Washington, D. C.:—I am very much interested in the subject matter of this paper. I have had occasion to speak to this question before. There has recently been so much written and said and claimed about the effect produced by the addition of small quantities of copper to steel that there is certainly an impression abroad that a new and wonderful cure-all has been discovered. Are we facing a method of alchemy by which, at the expenditure of very little trouble and only a few cents' worth of copper per ton, the well-known characteristics of steel with respect to some features are so marvelously changed as in effect to produce a new metal? I am a great believer in the future of the iron and steel alloy field. I do not think that with all the advances we have made in metallurgy we have begun to scratch the possibilities of usefulness to mankind in the exploration of the alloy field, and I include in that alloy field even what might be designated as the dilute alloy field; that is to say, where a very small quantity of a metal alloyed with iron and steel confers upon it some new or extraordinary qualities. With respect to many of the physical characteristics of the copper-iron or copper-steel alloys I am not particularly competent to speak. With respect, however, to the investigation of corrosion problems and with respect to claims made with regard to resistance to corrosion, I am competent to speak insofar as I have specialized to some extent in that field and have given up a great deal of my time for a great many years to a study of it. Some years ago, being particularly interested in the study of corrosion problems, I received the very generous permission of a large manufacturing concern to use their large open-hearth furnaces any way I saw fit for exploring this particular field. There is not a metallurgist in this room who will not realize the generosity of a manufacturing company who would permit a man to pursue a theory and give him as his material to work with 60 tons of molten metal. At all events, I threw copper into the open-hearth furnace. I explored this alloy field, not in a little crucible in which I had to take out portions and have them poured out into test pieces, but 60 tons of each one of the alloys were manufactured, covering pretty thoroughly the range reported on by this paper. Each one of these heats was carefully watched throughout its heat treatment in the rolling mill until it was rolled down to the finished product, and following this a very elaborate series of weather-exposure tests was made. In addition, every possible labor-

* Non-member.

atory test that has been proposed, including a number of original tests, was used in order to study the possible effect of copper in producing an extra corrosion resistivity in the metal. I had originally hoped that copper was going to prove to be a solution, or at least a partial solution, of the difficulty involved in manufacturing a metal of maximum resistance to corrosion at a reasonable cost. As time went on I was obliged to conclude as the result of many tests that copper was not beneficial to iron in aiding it to resist corrosion, but, on the contrary, was in many cases deleterious. In order to be conservative one has to admit that although copper may not be of any benefit to a comparatively pure iron base, it may, in some extraordinary way, either by preventing segregation of other impurities or for some other reason, be beneficial to steel. I am not prepared to prove that in some cases copper steel may not show an added resistance to corrosion, but I cannot say that the tests which I have under way seem to be tending toward that conclusion, and I greatly fear that the introduction of a certain small copper content as a standard of specification is going to admit improperly made steels, provided their copper content seems right. I am fortunate enough to have at my disposal a saw that in a reasonably short space of time will cut in two pieces longitudinally an iron or steel ingot 18 by 20 in., weighing about 5,000 lb. With this instrument at my command I have been interested in splitting open a number of ingots in order to study the best methods of getting dense, solid metal. Insofar as segregation influences the resistance to corrosion of the finished products, I think we must all admit that there may be two kinds of segregation: the one chemical and the other physical. For instance, in an ordinary steel, sulphide of manganese or a double carbide of manganese and iron may crystallize out of the molten magma during the cooling operation, and produce chemical segregation. Again, a molten metal on cooling may inclose gases and be full of blow-holes. If a material of this nature is rolled down to a finished sheet, it will suffer from this physical segregation or open condition of the ingot and be more or less laminated. Such a laminated material cannot be expected to be as slow rusting as one that is rolled from a dense, solid ingot. My observation so far, in the use of this apparatus, has been that the copper metal appears very much worse from the standpoint of physical segregation than the commercially pure iron. It is apparent that the authors of this paper, who were working with small runs of the crucible steels, encountered the same difficulty, and in their summary they recommended the advisability, where copper steels are about to be made, of the use of special deoxidizers in order

to overcome the tendency to make open ingots. They state: "In making copper steel by the crucible process it is necessary to 'kill' the metal before adding the copper, and just before pouring it is advisable to add a small amount of some deoxidizing agent." Of course, on the large scale of steel operation in the open-hearth plant, deoxidizing agents are invariably used and the metal is properly quieted.

I think it is now generally admitted that the rate at which a metal dissolves in acid is not a true measure of its corrosion resistance when it is put out under service. I have always thought that I was more liberal toward the acid test than a number of my colleagues, inasmuch as I have always claimed that it was a useful test if properly interpreted, even though it is not a true measure of atmospheric corrosion resistance. It will be noted (see table, p. 531, and Fig. 5) that at first as the copper content rises in their experimental heats the solubility in acid goes down, but, having reached a minimum, it starts up again, and when it goes up it proceeds far beyond where it started from when there was no copper in the metal at all. I can think of no reason why adding successive increments of copper to successive heats should have this effect, unless electrolytic action begins to make itself more and more felt as the increase of copper and the tendency to segregation increases. I therefore interpret this curve as showing that copper dissolved in steel tends to segregate, and if this is true, this tendency has to be overcome by the manufacturers of copper steel or else electrolysis, with its well-known tendency to produce rapid and localized corrosion, will take place.

The question I am discussing is whether the use of copper as a preservative for steel is a move in the right direction. If we should grant that a copper steel properly made will last longer than a steel without copper in it, what guarantee is there in the purchase or use of copper steel that it is properly made and that neither chemical nor physical segregation will produce deleterious results? Speaking for myself, I can only say that if I had no other evidence but this theoretical consideration, I should be opposed to the widespread use of copper steel until the actual evidence came in to prove that it was superior in rust resistance to carefully made steels which did not carry copper. I believe I may truly say that I have under my observation at the present time probably more extended atmospheric and other sorts of corrosion tests than any other investigator of this problem. I have been interested in it for a number of years, and I am anxious to get at the truth. If I can be convinced in the course of time that copper steel is more resistant to corrosion than any other

form of ferrous metal I shall not hesitate to make the admission, but the results so far obtained on tests which I am constantly inspecting have pointed in quite the opposite direction. For instance, some time ago I caused to be built a large manure stack which was made of alternate slats of corrugated ungalvanized material, all of equal gauge. The slats were not allowed to touch each other, but had open spaces between them, through which the seepage from the manure could escape. Some of these slats are now beginning to go, and I have no hesitation in stating here that in this particular case the copper steels and copper irons were the first to give way. I am quite aware of the fact that it is impossible to discuss so important a subject as this without developing grave differences of opinion between colleagues. Separate investigators do not report results which agree with each other. This seems to be inevitable in the study of all sorts of problems in this world. There can be, after all, only one solution, and that is to patiently wait until sufficient time has gone by for the points at issue to be thoroughly tested out; but I cannot agree that the tests made by any one investigator, whether by myself or by anybody else, as the result of the observation of a few months or of a year, can be taken as conclusively settling an important problem in which so many different kinds of people are vitally interested.

ROBERT W. HUNT, Chicago, Ill.:—A matter has come under my attention within the last two weeks which is of interest to me, particularly as I think I have been somewhat of an old fogy in thinking that the way to make good steel is to make good steel. There is a pipe line company in the far West which is about to lay several hundred miles of pipe. They have taken out of an existing line some samples of pipe and submitted them for analysis, so that they might have specifications drawn to give them, if possible, exactly the same material and results, as the pipe had given such excellent service and had been in the ground a little over 30 years. The pipe was made in Wheeling, W. Va. Analysis disclosed that, so far as its chemical composition was concerned, it was very ordinary Bessemer steel. But 30 years takes you back to about the time that Frank Hearn started making steel pipe. It was just about the time that the attention of the people who were then making steel in this country was particularly directed toward making soft steel, and they endeavored to get good results by great care in the blowing of that metal, having the ingots sound, discarding anything that indicated segregation or porosity, and working it carefully and thoroughly; and I think the life of that pipe is explained in that way. I am going to try to find out if

that is not some of Frank Hearn's original pipe. You may recall, Mr. Chairman, that he announced to the Institute the great results that he was accomplishing as against the then generally used iron pipe.

G. H. CLEVENGER, Palo Alto, Cal. (communication to the Secretary*):—If there is, as Mr. Cushman says, an impression abroad that the addition of a small amount of copper to carbon steel is going to prove a universal panacea for all the ills of the steel maker and user, it certainly is time that this erroneous idea was corrected. On the other hand, I cannot see why there should be a particular prejudice against the use of copper in carbon steel.

Surely Mr. Cushman could not have read our paper carefully or he would not have inferred that we poured our test pieces directly from the crucible. For his benefit I will again repeat that our steel was cast into ingots, these were forged into bars, and from these bars were cut the test pieces, which were turned to uniform size, sufficient metal in each case being removed to eliminate skin effect. Our results are therefore strictly comparable with crucible steel made in the regular way.

We have not advocated the acid test as being a measure of atmospheric corrosion to be expected. The only safe method of studying any corrosion problem, and there are a great variety of them, is to make the tests under precisely the same conditions which the metal must meet in service. Manure is no better for this purpose than acid or other artificial agents. If the problem is making steel which will withstand the corrosive action of manure, then the manure pile is the place to go to make the tests. Our corrosion tests with sulphuric acid were made with the object of showing only in a very general way (for we experimented with only one concentration of acid) the resistance of certain of the copper steels to this acid. We had in mind, more particularly, a problem which arises in many mines where the water contains sulphuric acid. Frequently the very mines which produce the copper have acid water to deal with, which rapidly destroys rails and other iron and steel objects which come in contact with it.

The question which is raised regarding the opportunity for the marketing of inferior steel on account of the presence of copper, and the difficulty of obtaining copper steel which has been properly made, amounts to nothing more than a reflection upon the integrity of the steel makers. There is no more reason for believing that copper steel

* Received Apr. 27, 1914.

cannot be properly made, and to specification, or that makers are going to practice fraud in its production, than there would be with pure iron or the many other kinds of steel now on the market. Let us not befog the issue with suppositions of this kind, but rather deal with the problem in the broad manner which its importance deserves.

When we have such a variance of opinion regarding corrosion, by men so well qualified to speak upon the subject as Messrs. Burgess, Cushman, Buck, and others, it would seem necessary that this point be settled by some impartial court of final appeal. In this connection I should like to suggest that the Bureau of Mines carry on a comprehensive series of corrosion tests under all possible conditions, not only with copper steel, but also with the other iron and steel products.

Of particular interest in showing the possible effect of the presence of small quantities of copper and nickel upon a certain type of corrosion, is the following case which came recently under my observation.

A rounded mass of steel weighing about 3 lb., which had been found in excavating a gravel bed near Niles, Cal., was sent to me for identification as a meteorite. Investigation showed that it was not a meteorite, but ordinary high-carbon steel of the following composition: [48]

	Per Cent.
Carbon	1.030
Manganese	0.200
Phosphorus	0.026
Sulphur	0.049
Silicon	0.210
Copper	0.179
Nickel	0.387

This mass of steel, at some time in the past had evidently been churned around a great deal, for it was smoothly rounded, and had the same general shape as the ordinary large gravel pebble. It was, however, apparent that it had remained stationary in the position in which it was found for a considerable period. Although this steel had remained in the damp gravel for an indeterminate length of time, there were no signs of rust or corrosion. The mass was cut through the center by means of a hack saw, and the surfaces thus exposed were polished. Fresh surfaces of this steel exposed to the rain and weather alongside of the ordinary reinforcing and merchant bar being produced at the Pacific Coast Steel Co.'s works at South San Francisco remained bright, while under the same conditions there formed upon the new steel a heavy coating of rust. Inspection of the analysis shows the only unusual thing regarding the composition of this steel

[48] Analysis made by Welton J. Crook.

to be the presence of a small percentage of nickel and copper. Adding the percentage of nickel to the percentage of copper, and calculating the percentage of nickel and copper present in the mixture, gives nickel 68.38, and copper, 31.62 per cent. The composition of Monel metal is given by Browne [49] as follows: nickel, 68 to 72; iron, 0.5 to 1.5; sulphur, 0:014; carbon, 0.073 to 0.15 per cent., and copper to balance.

It is evident that this grade of steel could be made by the addition of Monel metal to carbon steel of the composition indicated by the above analysis.

Browne [50] states that incorrodibility increases with the increase in the percentage of nickel up to 18.0 per cent., and that with ordinary nickel steels 3.0 to 3.5 per cent. nickel corrosion is slightly less than with carbon steels. No opinion is given for steels containing small proportions of nickel. He calls attention to the fact that, at least in nickel steels, increase in carbon content causes lessened corrosion.

It would be impossible to say which was the most potent element in preventing corrosion in this specimen of steel. But even granting that the high carbon content is not without its influence, the behavior of this steel must in a large part be due to the presence of either nickel or copper, or both in combination.

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[49] David H. Browne: *Electrochemical and Metallurgical Industry*, vol. vii, No. 3, p. 114 (Mar., 1909).

[50] Nickel steel; a Synopsis of Experiment and Opinion, *Trans.*, xxix., 603, 604 (1899).

Grain Growth in Silicon Steel.

BY W. E. RUDER, SCHENECTADY, N. Y.

(New York Meeting, October, 1913.)

IT has been pointed out by Stead[1] that grains of considerable coarseness may be developed in steels containing from 3 to 5 per cent. of silicon, and in a previous paper[2] the present author has shown that under the proper conditions of annealing single grains having an area of 50 sq. cm. may be obtained.

It is the purpose of this paper to discuss in more detail the conditions promoting the growth of these grains, in the hope that the experiments described may throw some additional light upon the general problem of grain growth in metals.

These silicon steels lend themselves particularly well to such experiments because of the ease with which the change in structure may be followed and because of the fact that the effect of the carbon present is largely obliterated by the high percentage of silicon. This forms a solid solution with the iron, which acts very much like a pure metal. This alloy has been found to have but one critical point, viz., Ar_2. This point, determined by Roberts-Austen for Hadfield's 4 per cent. Si alloy, was found to lie at 703° C. Vigouroux[3], in working with pure Fe-Si alloys, finds this point to rise from 750° C. for 0.5 per cent. Si to 860° for 6 per cent. alloy, with a flat region in the curve from 3 to 4 per cent. Si. No accurate determination of this point has been undertaken by the present author, but it has been roughly determined to lie between 730° and 750°.

In the preliminary experiments, some of which have been described in a previous paper, it was found that practically all of these steels, when annealed to 1,200° C. or higher, showed grains of comparatively large size. These grains varied in size, however, from about 4 sq. mm. to 50 sq. cm. (see Figs. 1 and 2) under the same conditions of anneal, and it was for the purpose of determining the cause of this difference in size that the experiments herein described were undertaken.

From a preliminary study of the chemical composition of the specimens

[1] *Metallographist*, vol. i, No. 4, pp. 289, 325 (Oct., 1898).
[2] *Journal of Industrial and Engineering Chemistry*, vol. v, No. 6, pp. 452 to 458 (June, 1913).
[3] *Comptes rendus*, vol. clvi, pp. 1374 to 1376 (1913).

FIG. 1.—PORTION OF A SINGLE GRAIN OF SILICON STEEL OBTAINED BY ANNEALING TO 1,300° C. IN VACUUM. ACTUAL SIZE.

FIG. 2.—SILICON STEEL ANNEALED AT 1,300° C. IN VACUUM AND ETCHED WITH CONCENTRATED HNO_3.

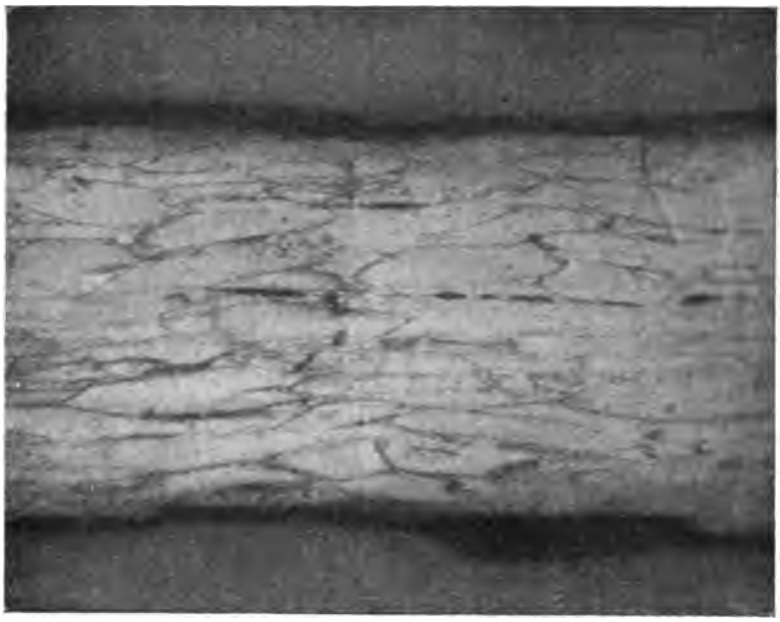


FIG. 3.—SILICON STEEL, UNANNEALED, ETCHED WITH DILUTE HNO_3.
90 diameters.

FIG. 4.—SILICON STEEL ANNEALED AT 800° C. AND ETCHED WITH HNO_3.
90 diameters.

it was found that the carbon (which varied from 0.01 to 0.06) had apparently no effect whatever upon the grain size. Those specimens containing about 3.25 per cent. Si had the largest grains, but these were of very irregular shape. Specimens containing 3.75 per cent. or more had grains of very regular shape, so called "equi-axed," and varying in size from 4 sq. mm. to 3 sq. cm. It so happened that the 3.25 per cent. specimen ran a little higher in manganese and phosphorus than the others, and it may be that this has some influence in forming the large grains, but this has not been determined. In fact, no effort has yet been made to determine the exact influence of the silicon content or of any of the impurities. The 3.25 per cent. alloy was used in all of the following experiments, so that the question of composition may be set aside for the time being.

The complete analysis is here given: C, 0.05; Mn, 0.15; P, 0.038; S, 0.026; Si, 3.24 per cent.

The Influence of Annealing.

For most of the work no special polishing was required—simply smoothing up with No. 0 emery, after removing the scale, and etching with HNO_3. A rapid etching was obtained by using concentrated HNO_3 and alternately dipping and washing the samples, allowing them to remain in the acid only a fraction of a second at a time.

All of the annealing experiments were carried out in tube furnaces, in a stream of pure dry hydrogen, to prevent oxidation, and the time given represents the time at temperature, not including heating and cooling.

As received from the mill, the material has the well-known fibrous structure shown in Fig. 3 (90 diameters).

Upon annealing to 800° C. in a closed pot, the real grains are formed, but are still of microscopic dimensions (see Fig. 4). In order to determine the temperature at which the larger grains begin to form, that is, such as can be readily discerned by the unaided eye, eight samples, varying in thickness from 0.005 to 0.025 in., were annealed in hydrogen for 3 hr. at 850° C., cooled over night, and then polished and etched with concentrated nitric acid. There was no change in structure. This was repeated at 920°, 960°, 1,000°, 1,050°, and 1,100° C. Samples more than 0.007 in. thick began to show slight grain growth at 1,050° C., while the 0.025-in. sample showed quite large sized grains (see Fig. 5). Above this temperature the large grains did not show any visible change, but the smaller grains grew until the entire remaining field was occupied by large equi-axed grains. These grains, once formed, could not be changed by any heat treatment short of actual fusion.

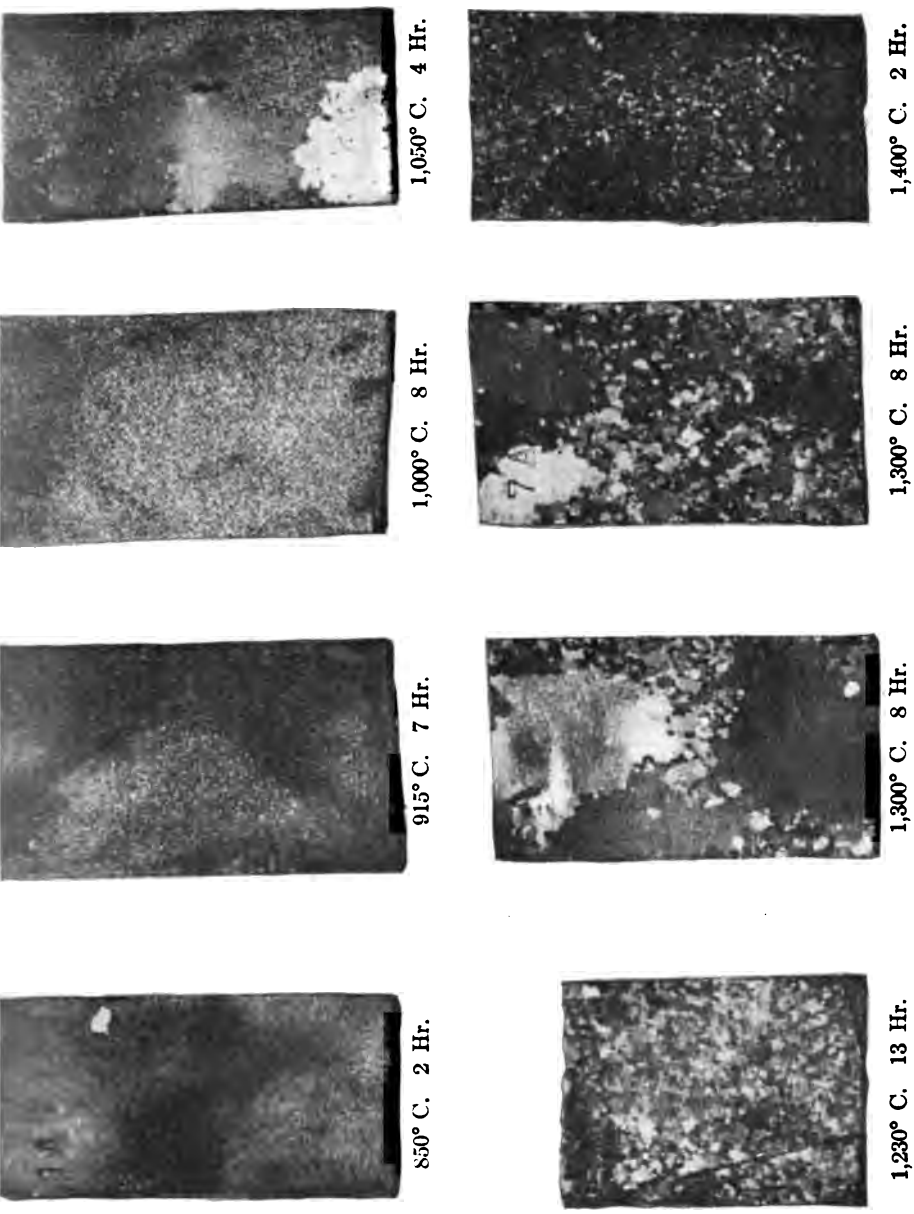


FIG. 5.—SILICON STEEL, ANNEALED, ETCHED WITH CONCENTRATED NITRIC ACID.

In order to determine the influence of time of anneal upon the grain growth, several 0.025-in. sheets were placed in a platinum-wound furnace and heated in hydrogen to 915° C. for 17 hr., 1,000° C. for 8 hr., and 1,230° C. for 15 hr. After each period one sample was removed and examined, while the others remained in the hot furnace. No change what-

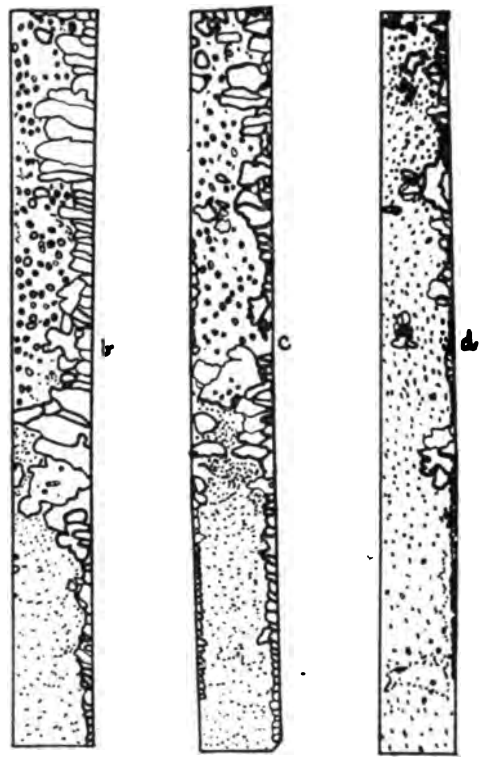


FIG. 6.—SHOWING THE GROWTH OF GRAINS ALONG THE DIRECTION OF STRAIN. ½ SIZE.

ever occurred until after the last period. Here the grains were quite large, but still of the same order of magnitude as those heated to an equal temperature for 2 hr.

At the lower limit of grain growth, *i.e.*, 1,050° C., it required about 15 min. to obtain any noticeable gain in size, but at 1,300° C. a few grains of from 3 to 5 mm. diameter formed in 3 sec. Upon further heating these sometimes grew to from 8 to 10 mm., but, as a rule, continued heating produced other grains, so that in 15 min. the whole sheet would be covered with grains of about 5 mm. average diameter. The grains grow slowly at 1,050° and reach their maximum size in about 3 hr., or less if the tem-

perature is higher. This maximum size is determined by the condition of strain existing in the sheet or bar, as will be explained later.

Another series of samples previously run showed quite large grains after annealing at 950° C. (see Fig. 6, reduced to five-eighths size). In these samples the grains begin at the edges and grow inward and are of the kind described by Dr. Percy as columnar. That these grains started to grow from one edge only of two of the strips and from both edges of the third, is explained by the fact that samples *b* and *d* were cut from either side of *c*, and the grains have grown only in those regions strained by cutting, and have their longest dimension in the direction of the strain.

FIG. 7.—SECTION OF SILICON STEEL INGOT SHOWING GROWTH OF GRAIN IN THE DIRECTION OF THE RADIAL STRAINS CAUSED BY COOLING.

Similar columnar or radial grains were found in the cast metal (see Fig. 7), where radial strains had been set up on cooling.

The Influence of Mechanical Working.

In previous work it had been noticed that no matter how high the temperature of anneal was carried, short of the actual melting point, the grains always formed with their longest dimension in the direction in which the original sheet had been rolled. This is quite remarkable in that the large grains do not grow under 1,050° C., and it is difficult to understand how any strain could persist after the piece had been annealed to 800° C. Repeated experiments, however, have demonstrated beyond question that the effect of rolling does persist, very much like the latent image on a photographic plate. This fact, coupled with the facts noted in the preceding paragraph, gave rise to a closer investigation of the effect of mechanical working.

The two samples shown in Fig. 8 were rolled along the edges *a*, so that the samples were reduced in thickness for only about one-third of the width. After annealing at 1,050° C. for an hour they developed the structure shown. The deformed areas have small grains, while those

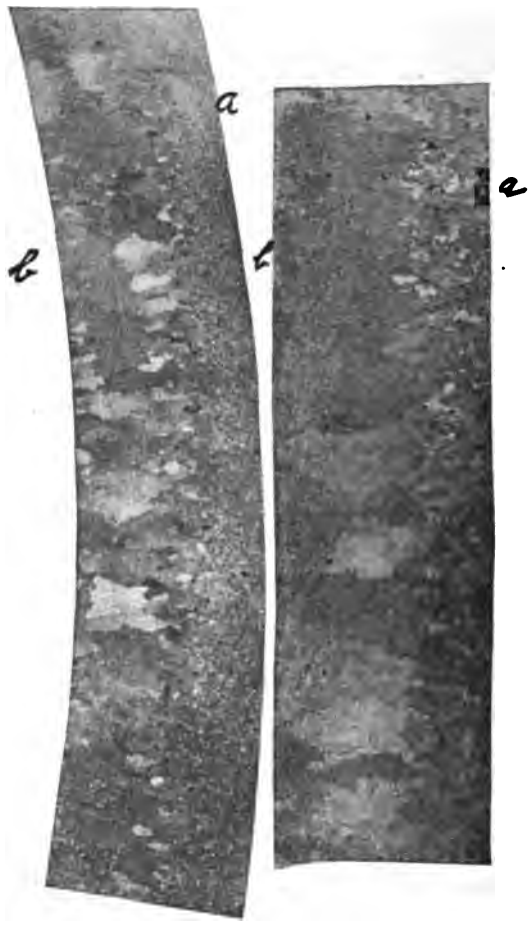


FIG. 8.—SHOWING THE EFFECT OF STRAIN UPON THE STRUCTURE OF SILICON STEEL. SAMPLES WERE ROLLED ALONG THE EDGES *a*. ACTUAL SIZE.

areas which were put under strain without any actual deformation developed grains of considerable coarseness, which have grown in the direction of strain. Samples hammered with a ball hammer showed the same effect, *i.e.*, a small fine-grained circular area surrounded by a fringe of large grains extending into the surrounding unaltered portion.

Several large-grained samples were then hammered lightly in different portions of the grains, and these worked areas, after annealing above 900° C., were found to be broken up into smaller grains. In this case,

Fig. 9

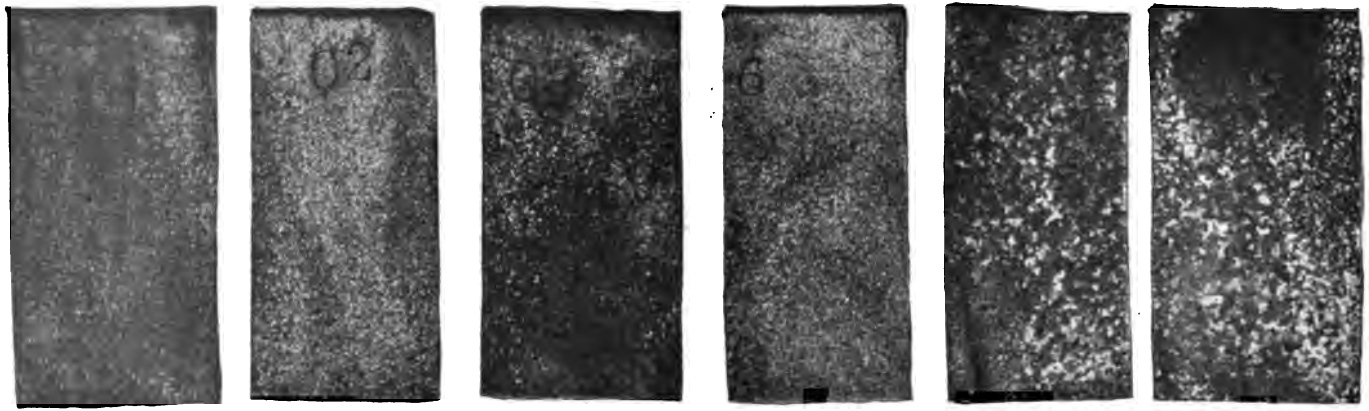
Fig. 10

FIGS. 9 AND 10.—LARGE-GRAINED SAMPLES IN WHICH SMALL GRAINS WERE FORMED BY HAMMERING ALONG THE BOUNDARIES OF THE ORIGINAL GRAINS, AND SUBSEQUENTLY ANNEALING. ACTUAL SIZE.

however, only the deformed areas were altered, while the original grain otherwise remained unchanged.

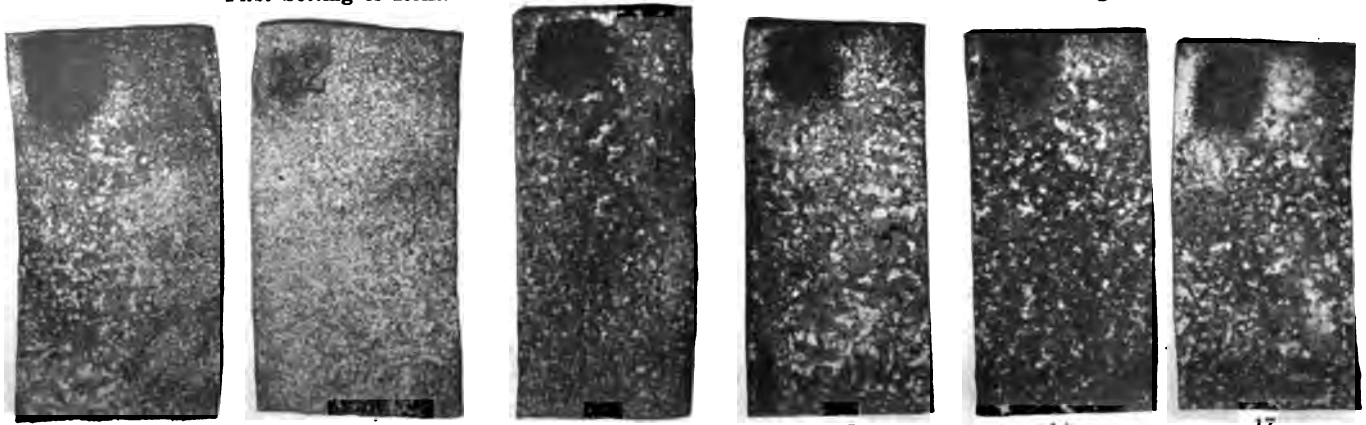
Figs. 9 and 10 show how these smaller grains may be formed at will wherever desired.

From these facts it is evident that there must be some critical strain at which the grains grow to the largest dimensions. Such a condition of strain in the metal would explain why sometimes in the same sheet



01 02 03 6 9 10

First Setting of Rolls. Second Setting.



11 12 13 15 16 17

Second Setting. Third Setting.

FIG. 11. SHOWING INCREASE OF GRAIN SIZE WITH DECREASE OF ROLLING PRESSURE.

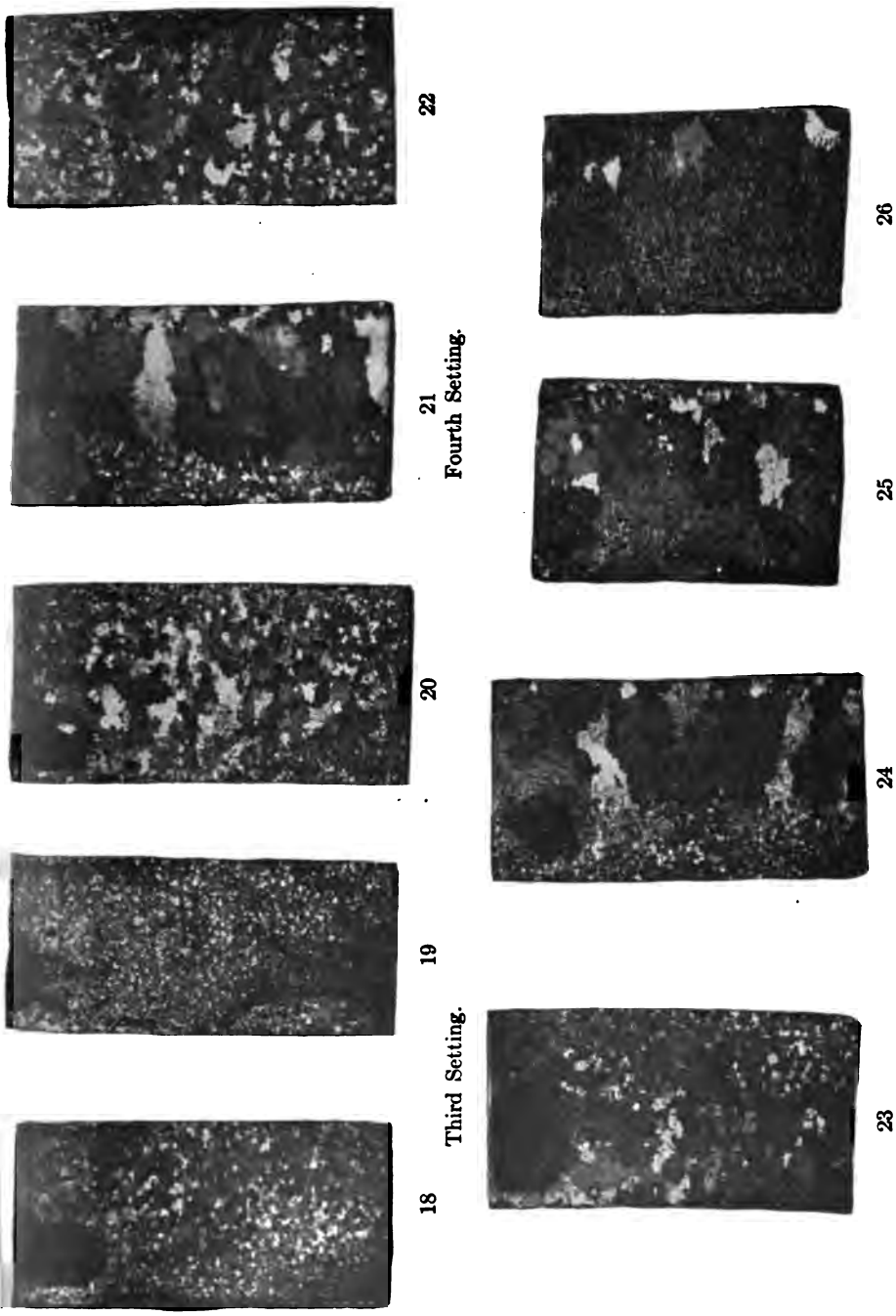


FIG. 12.—SHOWING INCREASE OF GRAIN SIZE WITH DECREASE OF ROLLING PRESSURE.

extremely large grains are found surrounded by very fine ones when conditions of heat treatment were precisely the same throughout the sheet.

In order to determine, as far as possible, how to obtain those conditions of strain most favorable to grain growth, a series of 26 samples were rolled under different conditions of temperature and rolling pressure. The first eight samples were rolled at from 600° to 1,000° C. with one setting of the rolls. The pressure was then decreased and another set run, and so on throughout the series. After this treatment, the samples were all annealed to 950° C. for 3 hr., etched and examined.

Figs. 11 and 12 show how the grain size increases with decreased rolling pressure. The pronounced effect of strain is a little more clearly illustrated in Fig. 13, where the middle area only has been subjected to the mechanical working. This was accomplished by placing a narrow strip of metal on the center so that only the area covered by this strip would be touched by the rolls. No. 1 was given a very light pass and remains practically unaltered; Nos. 2 and 6 were given heavy passes through the center section; Nos. 3 and 4 were rolled under moderate pressure and Nos. 7 and 8 under light pressure. The growth of grain under heavy pressure is seen to be outside the deformed area, while under very light working the deformation was not sufficient to produce any strain outside of the actual area under pressure, and so only those grains grew as in No. 7. An intermediate condition is that of No. 4, where the growth has extended outside of the area of deformation, but very little.

F. Robin[4] obtains practically the same results with steel and other metals, for temperatures below 900° C. Hammering, rolling, or compression tends to develop a coarsely granular structure *outside* of the deformed area. The granules in the deformed area mutually limit each other and are small, while just outside of the area their development is greater and extends into the unaltered section. As has been shown in the case of silicon steel, he finds that punching, shearing, or bending produces, on reannealing, large granules, which extend as far as from 7 to 12 mm. into the unaltered region.

That conditions of strain may be set up during annealing is shown in Fig. 14, which is one of six samples so placed in a furnace that the lower end was held at 1,340° C., while the upper end was at about 900° C. These were examined after 1, 1.5, 2, and 4 hr. The large grains grew in all samples in the region between the 900° area and the 1,300° area and began as a small grain on one edge of the sheet and grew across. Only one of the sheets in the final stage is shown in the illustration. This growth was evidently due to the strains set up by the difference in expan-

[4] *Comptes rendus*, vol. clv, pp. 716 to 719 (1912).

sion between the hot and cold areas and not to a flow of heat, as might at first be supposed.

The temperature at which the grain begins to grow to visible size in a sheet free from unequal strains has been shown to be 1,050° C. A slight strain, such as is caused by a light blow from a hammer, reduces the temperature necessary to about 725° C., as nearly as could be determined. At this temperature a slight growth was noted after 0.5 hr.

It is well known that ordinary mild steel or wrought iron coarsens rapidly between 600° and 750° C.,[5] and when heated above A_3 becomes fine again, and that quenching from any intermediate temperature gives a fine-grained structure. In silicon steels the conditions for coarsening are quite different and this difference is undoubtedly due to the effect of the silicon upon the carbon, thus causing the Si-Fe alloy to act more like a pure metal. If this assumption is correct we would expect no fining of the grain above A_3 in very pure iron, and such indeed has been found by Stead and Carpenter[6] to be the case. Silicon steel differs from the pure iron of Stead and Carpenter in that its grains are formed on *heating*, and quenching from the highest temperatures does not refine the structure. This would go to show that there is no difference between the structure of the gamma and the alpha iron in the presence of silicon.

The large grains, if subjected to mechanical working, require a higher temperature (1,020° C.) to produce a recrystallization than do the small grains under strain (750° C.). If unstrained, however, the small grains begin to grow at the same temperature at which the strained large grains are recrystallized. The large grains are then more stable under strain than are the small ones.

Summary.

It has been shown

1. That the grain of a 3.25 per cent. silicon steel may be considerably coarsened by annealing at temperatures above 1,050° C. If the sheets are subjected to any kind of unequal strain the very large grains begin to form at about 725° C.

2. That the grains form on heating at a rate depending upon the temperature; *i.e.*, the higher the temperature above 1,050° C. the quicker the grains form.

3. That the grain size is predetermined by the amount and kind of mechanical working to which the sheets have been subjected and not by the rate of heating or cooling.

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[5] Stead: *Journal of the Iron and Steel Institute*, vol. liii (1898, I), pp. 145 to 186.
[6] *Journal of the Iron and Steel Institute*, Sept., 1913.

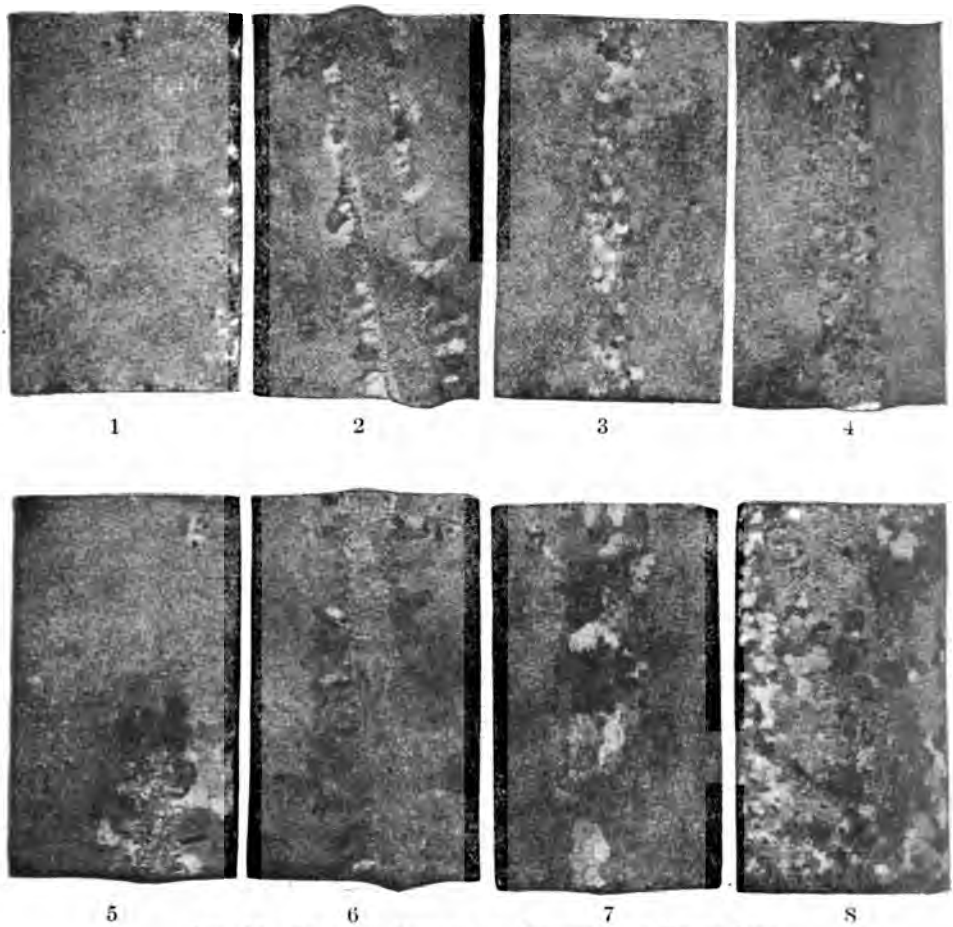

FIG. 13.—SHOWING EFFECT OF PRESSURE ON GRAIN SIZE.

FIG. 14.—SHOWING EFFECT ON GRAIN SIZE OF STRAIN SET UP DURING ANNEALING.

4. That light working or straining produces large grains, and heavy working produces fine grains.

5. That the large grains once formed connot be altered by any heat treatment short of actual fusion.

6. That only very slight growth is obtained in sheets less than 0.007 in. thick.

7. That the only way this coarse grain structure may be broken up is by mechanical working followed by a reanneal above 1,020° C.

8. That the effect of cold mechanical working persists after heating to incipient fusion.

DISCUSSION.

JOHN G. HOMAN,* Follansbee, W. Va.:—I would like to make a few remarks about Mr. Ruder's paper, in connection with the grain growth, particularly of silicon steel. Dr. Benedicks, I believe, has found that most of the elements located in the uneven series of the periodic system are those which produce grain growth or large grain field; while the others, the even series, are the refiners. Inasmuch as last year there was made in America something over 120,000 tons of steel used in the electrical industries, and inasmuch as 100,000 tons of that steel was silicon steel, the paper is of particular interest, because of the fact that there seems to be a definite connection between large grains and magnetic quality in steel, and I think that if Mr. Ruder's paper should point out to us a way in which we can increase the grain growth of silicon steel in our mills, it will be of enormous practical value. One particular thing, however, that seems to be summarized in the paper, is the fact that it requires something over 1,200°, or in the neighborhood of a thousand, I may say, to produce these phenomena, and that does not seem to be practical just at present in our systems of closed heating.

P. H. GRIFFIN, New York, N. Y.:—Does the gentleman mean the recrystallization of the metal?

JOHN G. HOMAN:—There certainly must be a recrystallization of the metal.

P. H. GRIFFIN:—Well, I should say that in cast iron, which is the only thing with which I have any experience, it is a very simple thing to alter the crystallizing of metal by subjecting it to the variation of heat and cold.

* Non-member.

JOHN G. HOMAN:—The attempt to produce the grain growth has been by different systems of heating and subjecting the mass to pressure or alteration of mass.

ALLERTON S. CUSHMAN,* Washington, D. C.:—I would like to know whether Mr. Ruder would be willing to state whether any investigations were made with respect to the magnetic and electric constants of the specimens after their grain growth had been modified by these experiments.

W. E. RUDER:—In answer to the question, I would say that the total watt loss is apparently not affected by size of grain; that is, on an average. Occasionally we find that the watt losses may be higher or lower, but we cannot ascribe that directly to the size of grain. On the other hand the hysteresis loss of the large grain is considerably lower than of the fine-grained structure; but the eddy current loss is almost double. The material has decidedly different magnetic qualities.

ALLERTON S. CUSHMAN:—The point I wanted to get at was whether in the case of very extraordinary sized grains a maximum efficiency was developed.

W. E. RUDER:—No. The experiments which I made upon hysteresis were confined to one series. You can realize the difficulty of getting a definite test on hysteresis and getting materials sufficiently uniform so that one could say the grains were all of the same size. The large-grain material would vary from a quarter of a square inch up to a square inch in size, so no definite conclusion can be arrived at there; but by taking an average lot of small, medium, and large grains the hysteresis follows the grain size very sharply. I might say that the eddy-current loss, too, is dependent upon the grain size up to the point where the grain size equals the thickness of the sheet; and beyond that there is apparently no difference.

ALLERTON S. CUSHMAN:—The idea I was trying to express has been in my mind for some time. I think it is not at all improbable that the same consideration comes into play with respect to magnetic and electric qualities as has been discussed here in an earlier paper with respect to the relation of strength of material with the growth of the grain, and the cementitious material, or material that lies in between grains. I asked the question in pursuing the idea whether there is not some ideal size of grain that gives the very best results.

* Non-member.

W. E. RUDER:—That I have not been able to determine very exactly. I cannot get the grains to grow large and small and get them where I want them. They do not always grow a quarter of an inch or half an inch as I would like them. As a matter of fact, the magnetic qualities of a sheet are very nicely brought out by placing some of this large-grain material in a magnetic field and getting an image of the flux by scattering on iron filings. In that case the crystals are very definitely outlined by the iron filings. This investigation was carried out in connection with a previous paper in which I had discussed the intergranular cement in silicon steel. There it showed either an intensified magnetism at that point or a gap.

HENRY M. HOWE, New York, N. Y.:—In Figs. 15 and 16 I condense some of the late remarkable discoveries as to this extraordinary grain-coarsening, as distinguished from the normal slow coarsening which occurs on long and high heating. The abscissæ represent grain size, the ordinates temperature, continuous lines rising temperature, and broken lines falling temperature, somewhat after Brinell. Plastically deformed impure iron, Fig. 15 (wrought iron and low-carbon steel cold rolled), coarsens with rising temperature, from say 450° to 850°, to refine abruptly on passing Ac3, without further coarsening on further heating. Pure iron, Fig. 16, does not coarsen thus in heating but does coarsen abruptly on cooling past Ar3.[7]

Strikingly unlike as these conditions look at first, on closer examination the coarsening seems in essence the same, the congenital coarsening of non-gamma iron crystals. I say non-gamma in order to avoid the contentious question as to the existence of beta iron.

To explain, Sauveur has shown[8] that the coarsening of Fig. 15 occurs only after a certain critical degree of plastic deformation. According to our present ideas, the essence of that deformation is the amorphizing of part of the alpha iron. The amorphous iron recrystallizes on reheating, or to be more specific dis-amorphizes, passing not from one crystalline form to another but from the amorphous to the crystalline state, which is quite a different thing. It is apparently during the birth of this crystalline iron that this enormous coarsening

[7] Stead and Carpenter: Crystallising Properties of Electro-deposited Iron, *Journal of the Iron and Steel Institute*, Sept., 1913. I found that even electrolytic iron coarsened somewhat if the temperature rose high enough, *e. g.*, to 1,300°.

[8] Note on the Crystalline Growth of Ferrite, *International Association for Testing Materials*, VI Congress, 1912, II, 6.

The doubling of the curve above A3 is only to indicate that this part of the curve represents both heating up and cooling down, and not to suggest that the grain size changes, when rising temperature gives place to falling. Let the reader make abstraction of this doubling, and mentally superpose the unbroken on the broken line.

occurs, hence I call it the congenital coarsening of non-gamma iron crystals.

The refining of this impure iron on rising above Ac3 is not surprising, meaning either that the crystals of gamma iron do not inherit the size of the non-gamma iron whence they spring, or that the crystals of the non-gamma iron born on again cooling past Ar3 do not. But as Goerens [9] has shown that the effects of overstrain are removed chiefly between 500° and 520°, and as therefore this should be the temperature of dis-amorphizing, more precise experiments are needed

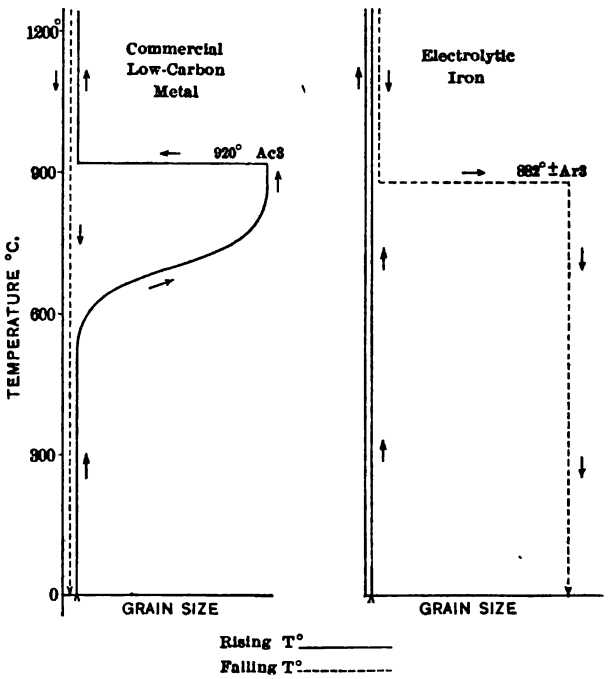

Fig. 15. Fig. 16.

FIGS. 15 AND 16.—BEHAVIOR OF VERY LOW-CARBON METAL.

The commercial coarsens on dis-amorphizing and refines at Ac3, the electrolytic coarsens at Ar3.

to show whether this coarsening will occur there. It was actually observed by Professor Sauveur at 650°, though Stead [10] reported that it occurs between 500° and 770°.

Turning to the pure electrolytic iron, Fig. 16, its coarsening occurs very abruptly in the change at Ar3 from gamma to non-gamma iron, so that this too is congenital coarsening of non-gamma iron.

[9] *Iron and Steel Institute, Carnegie Memoirs*, vol. iii, p. 383, Fig. 42 (1911).

[10] The Crystalline Structure of Iron and Steel; *Journal of the Iron and Steel Institute*, vol. liii (No. I, 1893), pp. 145 to 189. Brittleness Produced in Soft Steel by Annealing; *idem*, vol. liv (No. II, 1898), pp. 137 to 154.

Determination of the Position of Ae3 in Carbon-Iron Alloys.*

BY H. M. HOWE AND A. G. LEVY, NEW YORK, N. Y.

(New York Meeting, October, 1913.)

§ 1. INTRODUCTION.—This paper gives the results of our micrographic determinations of the position of Ae3 in a series of 14 hypo-eutectoid steels of varying carbon content, one of them very rich in phosphorus, and of SEe for one hyper-eutectoid steel. We have proceeded chiefly by noting the temperature at which the reabsorption of the pro-eutectoid element is found to be complete after a 30-min. holding, but in part also by noting the temperature at which its precipitation begins. Observations on these same steels by Messrs. Burgess, Crowe, and Rawdon, of the United States Bureau of Standards, are given in a paper[1] issued simultaneously with this, while in a third paper[2] one of us compares these data with those of earlier observers. These three papers are treated as one whole by numbering their sections, tables, and illustrations continuously, and by setting the folding plates of micrographs at the end of the last paper.

THE COMPOSITION OF THE STEELS EXPERIMENTED ON is given in Table I.

§ 2. *Notation and Definitions.*—Following Osmond's development of Tschernoff's notation, in which "Ac3" is the upper limit of the hypo-eutectoid part of the transformation range as observed in heating up, and "Ar3" is that same limit as observed in cooling down, and following the usual custom of calling the upper limit of the hyper-eutectoid part of that range "SE," we may go farther and call this latter limit "SEc" when observed in heating up and "SEr" when observed in cooling down.

Further, we may call the equilibrium position of these lines, *i. e.*, the position which they would have in pure iron-carbon alloys if all

* A contribution from the Metallurgical Laboratories of Columbia University. MS. received May 14, 1913.

[1] Thermal and Microscopical Examination of Professor Howe's Standard Commercial Steels, by G. K. Burgess, J. J. Crowe, and H. S. Rawdon, U. S. Bureau of Standards, p. 605, this volume.

[2] Discussion of the Existing Data as to the Position of Ae3, H. M. Howe, p. 611, this volume.

TABLE I.—*Composition of the Steels Used in these Investigations.*

No. or Mark.	Chemical Composition. C.	Si.	Mn.	P.	S.	Source.	Analyzed by	Form for Specimens.	Effect of Prolonging Heating from 30 to 60 Minutes. Lessened the Ferrite?	Made Ferrite Disappear?
						I. HYPO-EUTECTOID AND EUTECTOID STEELS. *A.* Low Manganese.				
VI.	0.027	.....	0.26	0.005	0.024	W. B. Kunhardt	B.[b] and M.	Rolled bar ⅝ in. diam.	.....	.....
A.	0.105	0.013	0.24	0.015	0.028	Pittsburg Testing Laboratory	P. T. L.	Plate 1 in. by $\frac{5}{16}$ in.	?	No.
IV.[a]	0.214	0.039	0.05	0.013	0.010	W. B. Kunhardt	B. and M.	Rolled bar ⅝ in. diam.	.....	.....
IV. A.	0.227	0.039	0.05	0.013	0.010	W. B. Kunhardt.	B. and M.	Rolled bar ⅝ in. & ½ in. diam.	.....	.....
IIII.	0.235	0.039	0.05	0.013	0.010	W. B. Kunhardt.	B. and M.	Rolled bar ⅝ in. diam.	Yes.	No.
H.	0.244	0.050	Nil	Nil	0.007	C. F. Burgess	B.	Rolled bar ⅝ in. diam.	.....	.....
61	0.382	0.027	0.22	.....	0.004	W. Campbell	B. and Morse[c]	Rolled bar ¾ in. diam.	.....	.....
I.	0.40	0.103	0.16	0.014	0.012	W. B. Kunhardt.	B.	Rolled bar ⅝ in. diam.	.....	.....
E.	0.563	0.18	0.15	0.013	0.013	J. A. Mathews	B. and M.	Rolled bar ½ in. diam.	.....	.....
V.	0.59	0.144	0.17	0.018	0.013	C. S. Marshall and F. H. Daniels.	B. and M.	Rolled bar ⅝ in. diam.	No	No.
G.	0.73	0.141	0.07	0.012	0.019	W. Campbell	B. and M.	Rolled bar $\frac{5}{16}$ in. sq.	.....	.....
VII.	0.92	0.14	0.123	0.009	0.011	J. A. Mathews	B. and M.	Rolled bar ⅝ in. diam.	.....	.....
						B. High Manganese.				
125	0.110	0.004	0.60	0.036	0.064	W. Campbell	B.	Rolled bar ½ in. sq.	.....	.....
III. A.	0.140	0.040	1.16	0.020	0.018	W. B. Kunhardt.	B. and M.	Rolled bar ⅝ in. diam.	.....	.....
B.	0.32	0.122	0.41	0.004	0.022	Pittsburg Testing Laboratory	P. T. L.	Forging ½ in. diam.	.....	.....
II.	0.46	0.094	1.215	0.017	0.015	W. B. Kunhardt.	B.	Rolled bar ⅝ in. diam.	.....	.....
						C. High Phosphorus.				
P.	0.342	0.10	0.11	0.417	0.029	W. B. Kunhardt.	B. and M.	Ingot 6 by 6 in. & Rolled bar ¾ in. dia.	.....	.....
						II. HYPER-EUTECTOID STEELS.				
VIII.	1.143	0.124	0.24	0.016	0.009	J. A. Mathews	B. and M.	Rolled bar ⅝ in. diam.	Yes	No.
IX.	1.45	0.14	0.16	0.009	0.006	J. A. Mathews	B. and M.	Rolled bar ⅝ in. diam.	.....	.....

FOOT-NOTES TO TABLE I.—[a] IV., IV. A, and IIII., are different bars of the same lot of steel, the carbon content differing slightly as shown. IV. A, was used only for the precipitation experiments represented on Plate 4, the pieces used at 910°, 880°, 850°, and 830° being about ⅝ in. long by ⅝ in. diam., and that used at 840°, ½ in. long and ⅝ in. diam. Steels VI., VII., and IX., were not examined microscopically. [b] B. in this column indicates Booth, Garrett & Blair; M. the makers of the steel; and P. T. L., the Pittsburg Testing Laboratory. [c] *Trans.*, xxix., 731 (1899).

The immediate reason for determining A3 was to provide a basis for our experiments on the influence of heat treatment on the tensile properties and endurance of nine steels of widely varying carbon content. In order to carry out these mechanical tests a very large quantity of each steel was needed, a quantity too great to be had in steels of extraordinary purity. Instead it was necessary to get such purity as can be had commercially. It was this consideration that determined the composition of the steels actually used. Perfectly pure steels, consisting of carbon and iron only, would have been preferred, as giving a base line of wider general interest; but as a basis for experiments in heat treating our specific steels, their own A3 was more appropriate than the A3 of pure iron-carbon steels.

The main series of steels on which these heat-treatment experiments are made consists of seven, of which five are covered by the Ae3 determinations of the present paper. In order to learn the influence of manganese and of phosphorus we have in addition experimented on two steels rich in manganese and one rich in phosphorus. Moreover, in order to fill in the gaps in Ae3 we have tested eight other steels of which only a little could be had. One of these, H, was made by Prof. C. F. Burgess from pure electrolytic iron. It is noteworthy that the results reached with this pure steel agree closely with those reached with the less pure steels which are nearest to it in carbon content.

lag were eliminated, "Ae3" and "SEe," in which "e" represents equilibrium.[3]

§ 3. *Our Procedure.—Reabsorption.* In order to detect the temperature at which the reabsorption of the ferrite apparently becomes complete, we first assembled the ferrite into coarse masses, the disappearance of which could be traced the more readily, by overheating to from 1,200° to 1,350° and cooling thence in the furnace. The structure at this time is shown in Fig. *A* of Rows 4, 5, 6, 7, 8, 9, and 17, Plates 2, 3, and 6.

We next reheated a series of such coarsened specimens of each steel to various temperatures below and above the supposed position of Ae3, so as to cause various degrees of reabsorption; held the temperature stationary for from 30 to 60 min. to give time for reabsorption to complete itself to the degree due; quenched in water to fix the *status quo;* ground the specimens very deeply or sawed them open so as to disclose the undecarburized interior; etched them so as to disclose their ferrite; and examined and photographed them microscopically, with the results shown in Figs. 1 and 3, and the micrographs of Plates 2, 3, and 6. In many cases the specimens after quenching were next reheated to 300° or 400° so as to troostitize their martensite, because the unreabsorbed ferrite can be recognized more confidently when in contrast with the black troostite than when in contrast with martensite. From our general knowledge of the subject we feel safe in assuming that no ferrite released during this troostitizing reheating can be mistaken for that remaining unreabsorbed before quenching.

Proceeding thus by stages 10° apart, we selected as our end of Ae3 a temperature between the highest at which recognizable ferrite remained and the lowest at which none remained; taking into consideration, in the exact selection of temperature, the quantity of ferrite unreabsorbed at the former temperature.

In addition we traced the course of reabsorption in steadily rising temperature as described in § 20 and shown in Row 13, Plate 5, but without record of the temperatures which the various stages of reabsorption represent, and of course without light on the temperature of A3.

§ 4. *Precipitation.*—Conversely, for three of our steels we determined Ar3, the temperature at which the precipitation of ferrite begins in cooling down, by heating a series of specimens to 1,000°, and cooling them severally to one or another of a series of tempera-

[3] See H. M. Howe, Ae1, The Equilibrium Temperature for A1 in Carbon Steel, p. 740, this volume.

tures in the neighborhood of Ae3, and quenching and examining them in the way already indicated for Ac3, with the results indicated in Fig. 2 and by the micrographs of Rows 10, 11, and 12, Plate 4.

§ 5. *The Course of Reabsorption Shown Micrographically.*—Row 4 of Plate 2 shows the general course of reabsorption in a previously coarsened 0.235 per cent. carbon steel, under these present conditions, of holding for 30 or 60 min. at the quenching temperature and then quenching. When the temperature has risen from Ac1 to 825° (Fig. *D*) the internal ferrite of the various grains has been so far reabsorbed as to make the initial network structure stand out again prominently, though the several sides of the polygons seem to have balled up as if by surface tension. At 830°, Fig. *E*, the further reabsorption has reduced the residual ferrite to a series of isolated and apparently unrelated specks. At 840°, Fig. *F*, most of these have disappeared, and those which remain have shrunk. At 850° the last has vanished, and the appearance is that shown in Fig. *F* of Row 5.

Under the microscope the bright, slightly yellowish white, metallic specks of ferrite contrast much more strongly with the surrounding martensite than in these photographs, *e. g.*, Figs. *E* and *F* of Row 4. We do not think that one is likely to be mistaken for the other, after a very little practice.

In Row 6 the progress of reabsorption in another steel relatively poor in manganese follows the same general course as in Row 4, though of course for given temperature the reabsorption in this higher-carbon steel goes further than in the lower-carbon one of Row 1. The progress from 780° to 800° of Row 6 is less than is due.

The influence of manganese in lowering A3, first noted by Osmond, seems to stand out well in certain micrographs but to be masked elsewhere. Thus for given temperature the smaller carbon content of Row 5 should lead to a greater quantity of unreabsorbed ferrite than is found in Row 6, as is shown by the numbers printed immediately under the several micrographs. That the expected excess is not found we naturally refer to the greater manganese content of Row 5, noting in Fig. 3 also that the relatively high manganese content of this specimen seems to have depressed its Ac3 materially. Yet Fig. *B* Row 5 seems to have at least as much excess of ferrite over Row 6 as the difference in carbon content implies. But we have found these appearances very deceptive, especially on such small magnification.

Micrographs showing the progress of reabsorption in the other steels are held in the archives of the Metallurgical Department of Columbia University. They do not differ enough from those here shown to warrant reproducing them.

§ 6. *The Course of Precipitation Shown Micrographically*, Plate 4.—Here the temperature of the beginning of precipitation descends as the carbon content rises from 840° for 0.227 carbon to 800° for 0.40 carbon, and 765° for 0.59 carbon.

Comparing those steels for which both precipitation and reabsorption are shown, the lag seems to be very slight. Or, at least, in view of the rapidity with which the quantity of ferrite due increases as the temperature falls, as shown in Fig. 12, for given temperature the difference between the quantity of unreabsorbed ferrite left in heating up and the quantity precipitated in cooling down is within the probable limits of error of our temperatures. This Steel IV A of 0.227 per cent. of carbon has precipitated about as much ferrite at 840° as it holds unreabsorbed at 840°; and Steel I., of 0.40 carbon, has precipitated about as much ferrite at 800° as it holds unreabsorbed at 800°.

The reason why the lag is so small is evidently that before quenching the temperature was held stationary for so long a time, 30 min., giving an incomparably better opportunity for an approach to equilibrium than can be expected in the relatively rapid heating or cooling of the thermal method. It is because of the slightness of this lag that we take our results to give a reasonably close approximation to the true position of Ae3.

The last of the residual ferrite in the reabsorption tests occurs in a few relatively large grains, but the first of the ferrite to precipitate in the precipitation tests occurs in many and very minute ones, probably because at the moderate temperature to which these were heated before cooling for precipitation, a very large number of crystallization centers persisted, at each of which the precipitation of ferrite started independently in the cooling. Our first trials, heating to from 850° to 900° before cooling for precipitation, gave us results hard to interpret. We then adopted 1,000° as a convenient temperature from which to start the cooling, but a higher temperature, by destroying more of the crystalline centers,[4] might have yielded ferrite in fewer and larger spots.

§ 7. *Graphical Representation.—Steels poor in manganese.* Fig. 1 shows the results of our reabsorption tests for steels poor in manganese, summed up by the line *HLM*, which is reproduced in the other graphical diagrams for comparison. The circular black spots indicate temperatures at which we found unreabsorbed ferrite, the white-centered circles those at which we found none. Note that the very pure electrolytic steel of 0.244 carbon agrees with the slightly less pure

[4] Cf. Benedicks, *Proceedings of the Institution of Mechanical Engineers*, May, 1910. p. 703.

steel of about 0.22 carbon. The thermal results of Messrs. Burgess and Crowe with these same steels are shown by means of St. Andrew's crosses, " ×," and plus marks, " +."

In view of the probability that the eutectoid point is at about 0.90 carbon and 725°, and that the break at *L* probably occurs at about 760°, representing Ae2, the line *HLM* represents our results closely. Thus in 6 out of our 9 hypo-eutectoid steels, this line lies within the 10° step in which we found that the reabsorption of ferrite became complete; in two of the remaining steels it lies 2° or 3° outside this step; and in the last one it lies 7° below this step.

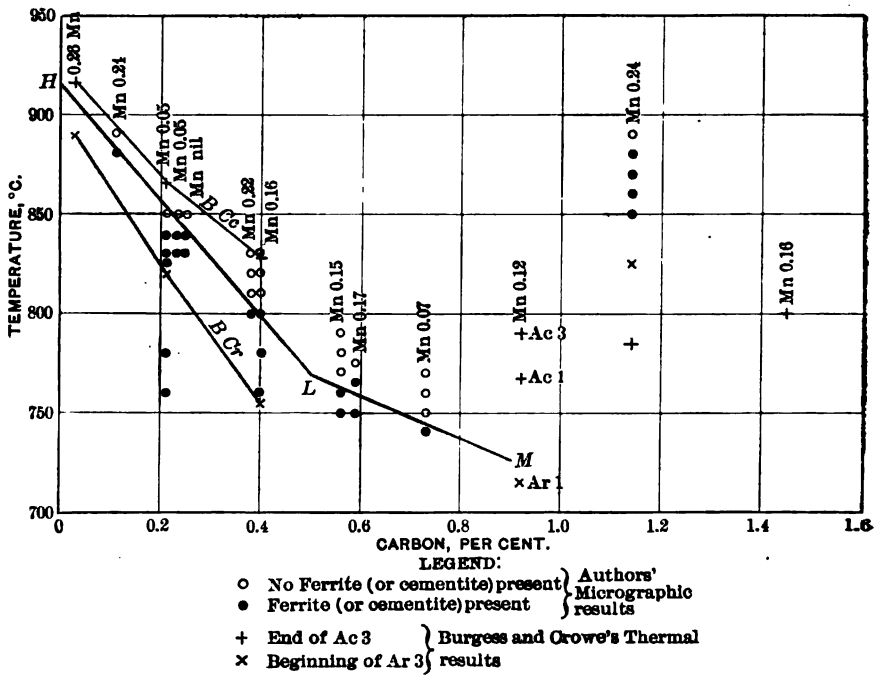

FIG. 1.—THE AUTHORS' MICROGRAPHIC REABSORPTION DETERMINATIONS OF Ac3 IN LOW-MANGANESE STEELS, TOGETHER WITH THE THERMAL DETERMINATIONS OF A3 BY BURGESS AND CROWE ON THE SAME STEELS.

The agreement is closer than is to be expected under our conditions, in view of the many sources of error, (1) decarburization, (2) persistence of solidificational segregation, (3) persistence of transformational segregation, (4) precipitation of " quenching " ferrite,[5] (5) failure to detect the last traces of unreabsorbed ferrite, and (6) temperature errors from various sources. The first four of these act cumulatively to raise the observed temperature of A3, and the sixth may act in the same direction. (See § 31, p. 624.)

[5] Ferrite which precipitates during the quenching, as distinguished from that left unreabsorbed or precipitated at the holding temperature before quenching. (See p. 601.)

§ 8. *A Shortage of Recognizable Ferrite* is apparent on comparing the quantity seen in each of the micrographs of Rows 4, 5, and 6, with that written immediately under each figure, and due on the theory that *HLM*[a] is really Ae3. Heyn[6] noted a like but smaller shortage of ferrite in his specimens quenched within the transformation range.

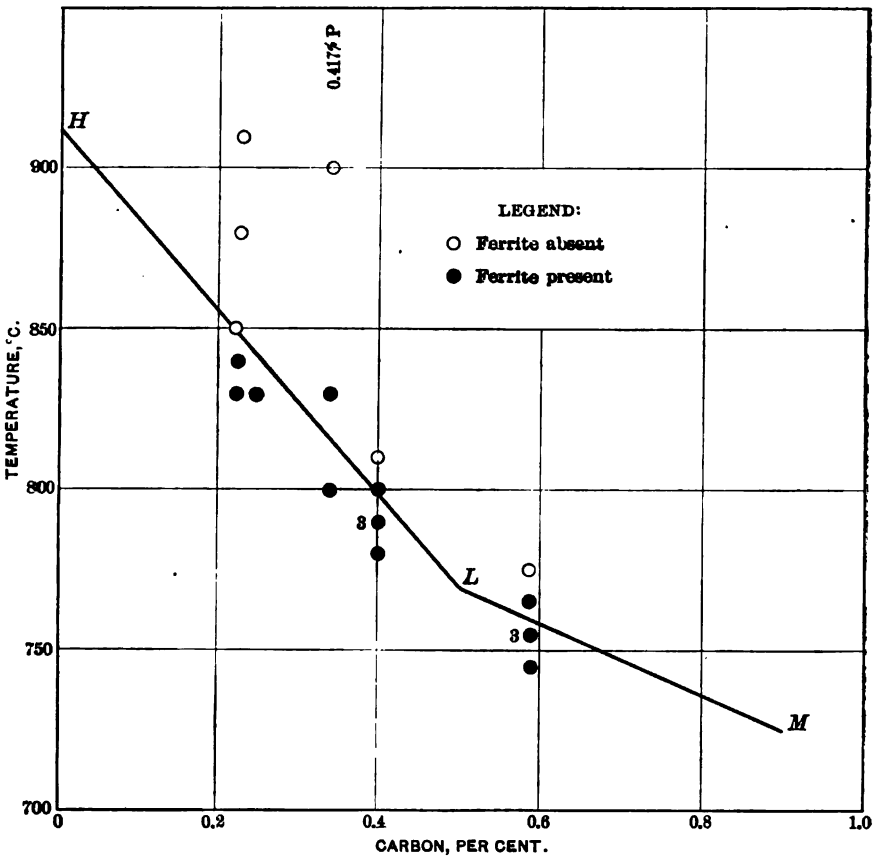

FIG. 2.—THE AUTHORS' MICROGRAPHIC PRECIPITATION DETERMINATIONS OF AR3 IN LOW-MANGANESE STEELS.

Note.—The line *HLM* is reproduced from Fig. 1 for comparison.

§ 9. *Precipitation.*—Though, as shown in Fig. 2, the temperature of Ar3 as found for the three steels experimented on by precipitation and that of Ac3 as found by reabsorption, fall within the same 10° step, yet the results by precipitation were less concordant among themselves than those by reabsorption.

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[a] Or more accurately *AAA*. See § 24 and Fig. 14.

[6] *Verhandlungen des Vereins zur Beförderung des Gewerbfleisses*, p. 378 (1904).

§ 10. *Comparison of Our Micrographic Results with the Thermal Data of Burgess and Crowe.*—Because Ae3 is the upper limit of the transformation range in equilibrium, the thermal data which give the most nearly direct evidence as to its position are the beginning of Ar3 and the end of Ac3, for each of these shows under its own peculiar conditions the actual upper limit of that range, Ar3 for falling and Ac3 for rising temperature. The maxima of Ar3 and Ac3 represent simply the temperatures at which the prior lag of the transformation is wiped out most rapidly, causing temporary maxima of retardation. As to the beginning ofAc3 and the end of Ar3, strictly speaking these coincide with Ac1 and Ar1 respectively; but as habitually reported they represent simply the temperatures at which the breaking up of lag becomes sufficiently accelerated to cause a noticeable retardation of the rise or fall in temperature. Hence for the purpose of checking up the micrographic results, the beginning of Ar3 and the end of Ac3 alone among the thermal data need attention.

These, plotted in Fig. 1, show great lag, with a gap of from 38° to 74° between the beginning of Ar3 and the end of Ac3. The unusual width of this gap may represent unusual sensitiveness in the determinations of the end of Ac3. Our line *HLM* lies from about one-third to about one-fourth way down from the upper edge of this gap. Hence if it is a close approximation to Ae3, this implies that the lag in the thermal observation in cooling is greater than in heating up, as indeed it should be, for well-known reasons.

§ 11. *Steels Rich in Manganese.*—Fig. 3 shows our micrographic results with four steels rich in manganese, and the thermal results of Burgess and Crowe with two of these steels, using the symbols already explained for Fig. 1, from which the lines representing the micrographic position of Ae3, the beginning of thermal Ar3, and the end of thermal Ac3 for our low-manganese steels are reproduced.

Though manganese had but little effect on the position of Ac3 as we found it micrographically, it lowered materially the thermal position as found by Burgess and Crowe, the end of Ac3 by about 30° in both cases, and the beginning of Ar3 by about 60° in one case (manganese 1.16 per cent.), and in the other case (manganese 1.21) by a smaller quantity, which cannot be measured even approximately without rather rash extrapolation.

§ 12. *Acknowledgments.*—This investigation was made in the Metallurgical Laboratories of Columbia University, in part under a grant from the Carnegie Institution of Washington. Our thanks are due to Messrs. W. B. Kunhardt of the Carpenter Steel Co., J. A. Mathews of the Halcomb Steel Co., C. S. Marshall and F. H. Daniels of the

American Steel & Wire Co., Profs. W. Campbell of Columbia University and C. F. Burgess of the University of Wisconsin, and the Pittsburg Testing Laboratory for the specimens of steel; to Messrs. Booth, Garrett & Blair and the Pittsburg Testing Laboratory for the chemical analyses; and to Prof. William Campbell of Columbia University for counsel.

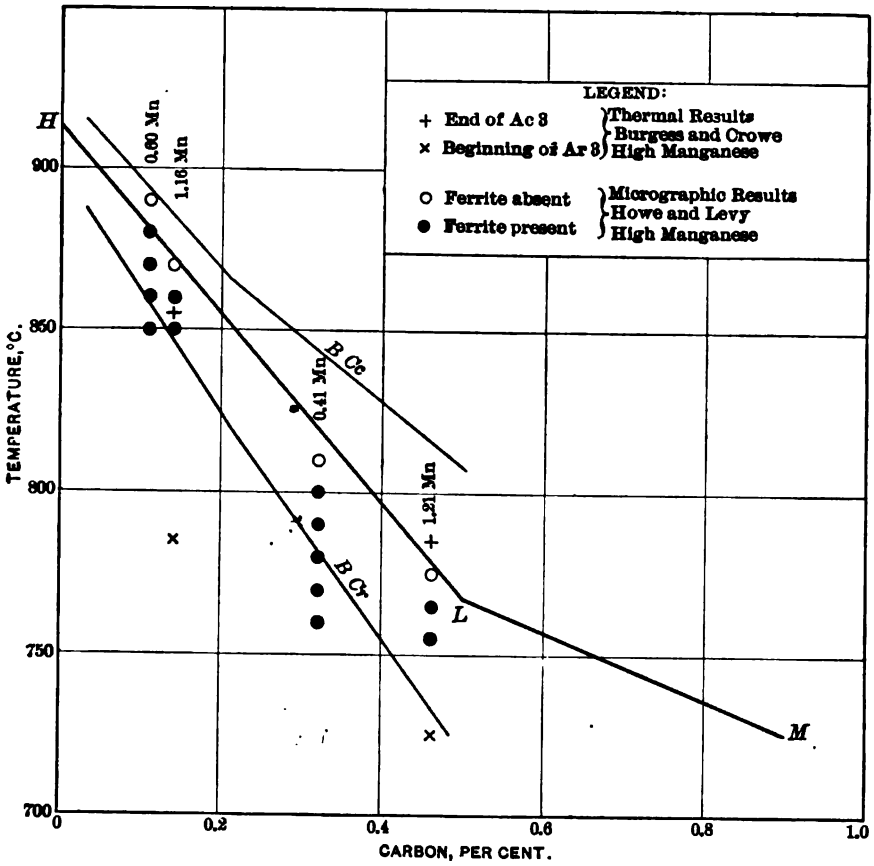


FIG. 3.—HIGH-MANGANESE STEELS. THERMAL A3 OF BURGESS AND CROWE AND MICROGRAPHIC A3 OF THE AUTHORS.

Note.—The lines *BCc*, *BCr*, and *HLM* are reproduced from Fig. 1 for comparison.

To Messrs. Kunhardt and Burgess our obligations are unusually great, for they purposely made special products for us. Mr. Kunhardt's task of rolling this very phosphoric steel into a 12-ft. bar $\frac{5}{8}$ in. round must have been a serious one.

SUMMARY.

1. We determine micrographic Ac3 and Ar3 in a series of 14 steels, of which 4 are rich in manganese, by quenching from a series

of temperatures in and above the transformation range, and noting that at which the reabsorption of ferrite is complete in heating up and that at which the precipitation of ferrite begins in cooling down. Line *HLM*, Fig. 1, p. 592, sums up our results.

2. Like Heyn, we find in specimens quenched within the transformation range a shortage of recognizable ferrite below that which the position of our line *HLM* calls for. (§ 8, p. 593.)

3. The results obtained by reabsorption and by precipitation agree, but the precipitation evidence is less harmonious than the reabsorption. (§ 9, p. 593.)

4. The micrographic results agree with the thermal ones reached by Burgess and Crowe. (§ 10, p. 594.)

5. When the ferrite is reabsorbed during steadily rising temperature, its network preserves its continuity better than when reabsorption occurs at constant temperature. (§ 20, p. 601.)

6. Phosphorus raises A3, at least in unforged castings (§ 21, p. 603).

Appendix 1.

§ 13. *The Initial Coarsening*, for facilitating the detection of the end of the reabsorption of ferrite, was done by heating the specimens, usually $\frac{5}{8}$ in. in diameter and from 2 to 4 in. long, to about 1,200° C. in molten barium chloride in a graphite crucible, heated in a gas furnace, within which the crucible and the specimens were cooled slowly.

From these coarsened specimens wads about $\frac{5}{8}$ in. in diameter and 0.5 in. long were cut for the reabsorption treatment. This preliminary coarsening treatment was not applied to the specimens treated by precipitation, except in the case of the phosphoric steel.

Reabsorption.—The various coarsened specimens were then reheated to various temperatures, some believed to be below and some above Ae3, held there for 30 min., and immediately drawn and quenched in cold water.

The reabsorption and precipitation heatings were carried out in the apparatus shown in Fig. 4.

The specimen *A* lay in a bath, *B*, of molten chlorides of sodium and potassium, or, for the higher temperatures, of chlorides of barium and potassium, in a graphite crucible, *C*, $1\frac{5}{8}$ in. high and 1.5 in. in diameter at the top, which stood in the middle of the length of a horizontal steel pipe, *D*, 2.5 in. inside diameter and 30 in. long, within an alundum tube, *E*, on which a spiral of nichrome electric resistance wire, *F*, was wound. This outer tube was encased heavily in insulating material, *G*. The thermo-junction *J* lay immediately above the crucible, and rested on it.

In order to carry still further the protection against surface decarburization which the bath of fused chlorides offered, an abundance of charcoal was set within the inner tube *D*.

In order to flatten the thermal gradient by lessening end cooling, clay plugs, *H*, *H*, were set within the inner tube about half way between the ends and the middle, and both ends were closed with caps. The thermal gradient of the furnace at 960° is shown by the following determinations to be very flat.

To the left.		Middle of length.	To the right.	
2 in.	1 in.		1 in.	2 in.
958°	960°	962°	959°	955°

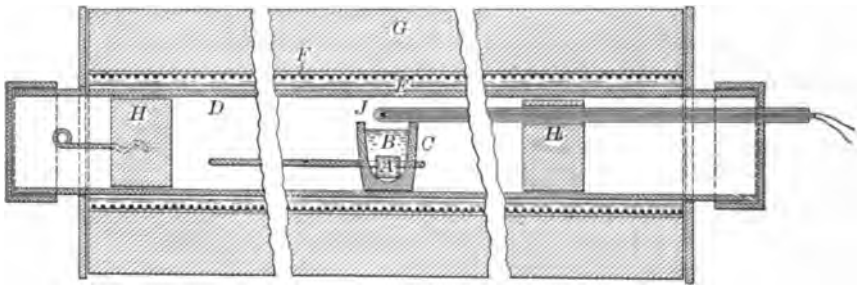


FIG. 4.—TUBULAR ELECTRIC RESISTANCE FURNACE USED FOR MOST OF THE REABSORPTION AND PRECIPITATION EXPERIMENTS.

A, Specimen.
B, Bath of molten chlorides.
C, Graphite crucible, 1⅛ in. high, 1.5 in. diam.
D, Steel pipe, 2.5 in. diam.
E, Alundum tube.
F, Nichrome resistance wire.
G, Insulating material.
HH, Clay plugs.
J, Fused quartz tube containing the thermo-junction.

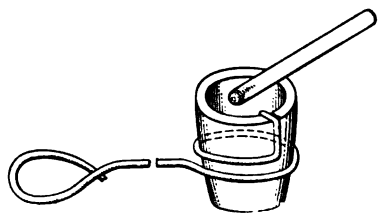


FIG. 5.—DETAILS OF CRUCIBLE AND QUENCHING WIRE.

In order that the crucible could be withdrawn quickly and inverted so as to throw the specimen into the quenching water, it was held in a wire coil as shown in Fig. 5.

Having first brought the middle of the furnace and the crucible with its molten chloride bath to a little below the temperature aimed at, the crucible was withdrawn to receive the specimen to be treated, and immediately replaced exactly beneath the tip of the thermo-

couple. In the reabsorption experiments the crucible and specimen were then heated to the desired temperature, held there for 30 min., drawn, and emptied quickly into a pail of cold water close by. In nearly every case the temperature of the thermo-junction rose, usually by from 2° to 5°, on drawing and quenching. Though this rise may represent a slight lagging of the temperature of the specimen behind that of the thermo-junction, we believe that it represents chiefly the accelerated combustion of the charcoal present within the tube caused by the inrush of air, for on opening the furnace in like manner without withdrawing the specimen a like rise of temperature occurred.

In a few cases we used a vertical cylindrical muffle, 5 in. in diameter inside and 28 in. high, heated by the resistance of a wire spiral wound about it, setting the specimen in a chloride bath in a large crucible 4.25 in. in diameter and 5 in. high which stood in this muffle. Here the thermo-junction lay beside the specimen. But this arrangement was soon abandoned because it gave slightly less concordant results. The advantage of having the thermo-junction within the salt bath beside the specimen seems to have been outweighed by the steeper thermal gradient within a vertical than within a horizontal furnace. In a vertical furnace the air cooled by contact with the cooler top is in a position to fall like a douche on the surface of the bath within the crucible. Moreover, if the thermo-junction lies within the chloride bath with the specimen in a vertical furnace, it is harder to make sure that, at the very moment of drawing and quenching, the specimen is truly at the temperature last recorded, than if the thermo-junction lies immediately above the bath in a horizontal furnace.

The galvanometer and the thermo-junction were calibrated by the Bureau of Standards at Washington, and repeatedly re-calibrated by the melting points of antimony and copper.

§ 14. *Does a 30-Min. Holding Suffice?*—In the case of steels A, IIII, V, VIII, and II, after examining the specimens so as to learn the highest temperature after quenching from which the pro-eutectoid element could still be detected, another specimen of that steel was next held at this same temperature for 60 instead of 30 min., in order to learn whether prolonging the exposure to this temperature would cause this element to disappear, or, in other words, to learn whether the persistence of the element at this temperature at the end of 30 min. was due to insufficient opportunity for diffusion, or whether it really meant that the temperature was below Ae3.

In three cases this prolonging of the holding had no effect. And though in two cases it seemed to lessen very slightly the quantity of

residual ferrite, the temperature-equivalent of this diminution was so slight that any merit of this prolongation in this respect seemed to us balanced by the greater danger of inaccuracy through uncontrolled temperature oscillations.

Precipitation Experiments.—These were carried out in the general way described for the reabsorption experiments. The bath of chlorides having been melted, the specimen was immersed in it, crucible and specimen were set within the horizontal nichrome furnace shown in Fig. 4, raised to 1,000°, held there for 10 min., cooled within the furnace to the quenching temperature, held there for 30 min., and then drawn and quenched in cold water, as already described.

Repetition Heatings.—In eight cases the reabsorption tests were confirmed by repetition, heating additional specimens independently either to the highest temperature at which unreabsorbed ferrite had been detected, or to the lowest at which none had been, or to both, and quenching as before. The results were concordant in every case.

Repetitions of the precipitation experiments, though, as might have been expected from the greater lag in cooling, they were less concordant than those of the reabsorption tests, yet in no case contradicted the initial results. They were of value in interpreting them.

§ 15. *Surface Decarburization* took place, probably during the initial coarsening. It is shown by the greater quantity of ferrite at the outside than in the interior of the specimens. Compare the outside of specimen IV, 93, Fig. *H*, Row 14, Plate 5, with its interior, Fig. *G*, Row 14, Plate 5. It is because of this tendency to decarburization that the specimens were made so thick. After the heat treatment and before etching, these wads were ground down $\frac{1}{8}$ in. or sawed open in order to give access to their undecarburized interior.

But though in the various steels, when the quenching temperature had risen just high enough to leave no visible unreabsorbed ferrite in the interior of the specimen, some ferrite remained in the superficial layer, proving that there was superficial decarburization, yet none was found there in the specimen quenched from 10° higher. This disappearance of the skin ferrite at a temperature so slightly above the disappearance of the internal ferrite argues for the consistency of our results, though of course it throws no light on the accuracy of the method itself.

16. *Etching.*—The purpose of the etching, to detect the pro-eutectoid ferrite, or in the case of steel No. VIII the pro-eutectoid cementite, could be served best by darkening the martensite or troostite which formed the rest of the field, leaving the pro-eutectoid element

light. To that end the etching was done with nitric acid, using a solution of 2 per cent. of acid of 1.4 sp. gr. in water, and an immersion of from 5 to 40 sec. or till the martensite or troostite turned dark yellow to black. This treatment affected the pro-eutectoid element but slightly. In order to clean the specimen and enable the nitric acid to act evenly, a preliminary etching of about 30 sec. was given, with a solution of 5 per cent. of picric acid in alcohol. This preliminary etching or cleaning was, on the whole, better done with this solution than with the others tried, such as alcohol alone, and nitric or hydrochloric acid much diluted with alcohol. On leaving this etching bath the specimen was washed with running water, and without drying at once immersed in the aqueous nitric acid.

Instead of nitric acid, alcoholic hydrochloric acid was tried; but with this the etching is much slower, needing from 5 to 15 min., and the contrast between the pro-eutectoid element and the martensite, etc., is on the whole less clear.

§ 17. *Troostitizing.*—In many cases the pro-eutectoid element sought was made the more prominent by converting the rest of the field wholly into troostite by a reheating to 300° or 400° before etching. As may be seen in Figs. *B*, *C*, and *D* of Row 4, Plate 2, and Fig. *A* of Rows 10, 11, and 12, Plate 4, this troostite darkens so deeply on etching that the bright ferrite stands out in very strong contrast. Were we to repeat this work we should thus troostitize all the specimens.

Thus the etching was directed strictly to the point immediately before us, creating a very strong contrast between the unreabsorbed ferrite and all the remainder of the field, and sacrificing deliberately all indications of the structure of this remainder.

§ 18. *Troostitic Areas.*—Very often the interior of a specimen quenched within the transformation range etched much darker than the outside before troostitizing, as shown in Fig. *F*, Row 14, Plate 5. Here and in other cases the darkest region lay part way between the skin and the axis, and was neither circular nor concentric with the specimen itself. These facts explain themselves as the resultant of two causes. In that the interior cools more slowly than the outside, it tends to transform further during the quenching than the outside. Were this the sole cause, then the darkest etching should be at the axis, because it is unlikely that any part has transformed beyond the osmondite stage of darkest etching. That this darkest etching is not at the axis indicates that the varying pressure within the specimen has had a part in determining its position, on the principle that pressure retards the transformation. That it is not con-

centric with the specimen indicates that the conditions of quenching have led to an unbalanced escape of heat from the surface of the specimen, as indeed must occur if, in entering the bath, its axis is not strictly vertical, or indeed if there is any eccentricity in the path of the escaping bubbles of steam.

This darker etching of the interior was noticed by Heyn.[7]

§ 19. *Quenching Ferrite.*—In certain cases a very fine network occurred within the quenched specimens, as in Fig. *E*, Row 14, Plate 5, representing Steel I (carbon 0.40), heated to 810°, or to the top of the 10° step in which the reabsorption of ferrite becomes complete, held 60 min., and quenched. The other cases in which this network was noticed were as follows: Steel A, carbon 0.105 per cent., quenched from 890° and 880°; Steel IV, carbon 0.214 per cent., quenched from 840°; and Steel I, carbon 0.40 per cent., quenched from 810°; all after 30 min. holding. This fine network is very distinct from the nearly equiaxed spots of ferrite which gradually decrease as the quenching temperature rises towards *HLM*, and increase as it is lowered from *HLM*. This may be "quenching ferrite," *i. e.*, that precipitated during the quenching, though the time available is rather short for such a coalescence.

Appendix 2.

§ 20. *The Course of Reabsorption with Steadily Rising Temperature.*—The micrographs of Row 13, Plate 5, represent a series of points in the length of one and the same piece of steel, I, of 0.40 per cent. of carbon, after a differential treatment which completed the reabsorption of pro-eutectoid ferrite in one end of the piece without allowing it to begin in the other end. In order to bring this about the piece was overheated to 1,350° and cooled slowly so as to assemble the ferrite together into coarse compact masses easy to recognize. Fig. *A* shows the structure of the coarsened bar at this stage. Successive stages in the reabsorption of the ferrite thus massed were established by heating the bar differentially so as to create a flat thermal gradient along its length, running through the transformation range. This was done by immersing one end in a bath of molten potassium sulphate and allowing the other to project into the air, under a suitable cover. These stages were then fixed by quenching the piece in cold water, and they were then revealed by etching the bar and photographing a series of points along its length, with the results shown in Row 13.

[7] *Verhandlungen des Vereins zur Beförderung des Gewerbfleisses*, p. 380 and Fig. 152 on Plate 17 (1904).

Fig. *B* shows, in its left-hand part, a polygonal grain which had not passed above Ac1, and therefore is in the condition of ferrite plus pearlite, of which the former appears as the light network and spines, the latter as the dark ground mass. The light-colored polygons at the right of this dark one had passed above Ac1, and therefore at the moment of quenching consisted of the unreabsorbed ferrite, shown still in white spines and network, and the austenite into which the pearlite had changed, here appearing as a martensite ground mass which, though slightly less light than the ferrite, is yet far lighter than the pearlite in the left-hand grain.

The successive figures *C* to *E* show stages in the further reabsorption of the ferrite. First the interior or cleavage ferrite disappears, then the network becomes discontinuous, and in Fig. *E* disappears. In some parts the network which the ferrite had formed can be traced beyond where the ferrite itself vanishes, but this darkening seems to be only a shadow effect, because under proper illumination the bottom of the grooves which form the grain boundaries appears of the same pearly white as the interior of the grains.

The progressive increase from Fig. *F* to Fig. *H* of the light martensite and decrease of the dark troostite represent the increasing lag of the transformation from the stage of martensite to that of troostite, an increase which the rise of the quenching temperature naturally induces.[8] The white spottings at the right-hand part of Fig. *E* of Row 13 indicate a possible cause of error. These occur above where the absorption of the ferrite network has become complete, and hence presumably well above Ae3. If such spots should be noticed in cooling down, they might well be taken for an early precipitation of ferrite, and thus the line A3 might be set not only too high but perhaps very much too high.

A careful study of the white constituent which forms the ground mass of the polygons in Figs. *C*, *D*, and *E* leaves no doubt that it is martensite.

One asks why this cooler and hence less lagging part of the specimen should be caught in the stage of martensite though the hotter part presented by Fig. *F* has transformed beyond into that of troostite, common report being that of the two it is quenching from above A3 that yields martensite and quenching from the lower part of the transformation range that yields troostite. Two reasons why martensite should be found in the cooler but not in the hotter parts suggest themselves: 1st, at the moment of immersion the austenite in the

[8] See H. M. Howe, Why Does Lag Increase with the Temperature from which Cooling Starts? *Trans.*, xlv, 516 (1913).

parts just above A1 was a higher carbon austenite than that in the hotter parts, and it is most familiar that an increase of carbon content increases lag. 2d, in quenching it was the hot end of the piece that entered the water first, and hence in its rapid descent through the water it was less surrounded by steam than the cooler parts above it. A repetition of this experiment gave like results.

The cleavage ferrite is often much more abundant near the boundaries of a given polygon than in the central part of it, suggesting that this cleavage ferrite has been trapped here in the course of its migration to the grain boundaries.

The shorter opportunity for balling up by surface tension explains why the ferrite network remains continuous to a further stage in its reabsorption here in rising temperature, than when it is reabsorbed at stationary temperature as in Row 6, Plate 2.

Appendix 3.

§ 21. *Phosphorus Raises A3.*—One of us had inferred that phosphorus raises the temperature of A3, from the behavior of phosphoric steel, and more particularly from Stead's important discovery that though, as long as the solidificational segregation of phosphorus persists the slag inclusions always occur within pro-eutectoid ferrite islands, they do not if that segregation is effaced by diffusion.[9]

In view of later observations it is more accurate to say that, after the phosphorus has been diffused, each slag inclosure has at most a narrow reef of ferrite about it, instead of being inclosed in a large ferrite island. (J. E. Stead, Private communication, Mar. 4, 1913.)

This inference is confirmed by the micrographs of Plate 6. Here the presence of 0.417 per cent. of phosphorus has raised A3 from about 820°, its inferred position for this carbon content, 0.34 per cent., in the absence of phosphorus, to above 950° if the initial ingot structure is not removed, and to above 870° if it is removed. The specimens here represented were treated for reabsorption of ferrite (Rows 15 and 16), and for precipitation (Row 17), substantially like the steels represented on Plates 2, 3, and 4, and as set forth in Appendix 1, except that the specimens cut from the ingot were not coarsened before the reabsorption experiments, and that before the precipitation tests the rolled bar was coarsened.

Some of the light particles in *F*, Row 15, representing the unforged ingot, are evidently ferrite which has persisted unreabsorbed even at 950°.

[9] Howe, discussion of J. E. Stead's paper, *Proceedings of the International Association for Testing Materials*, VI Congress, III, to appear.

When the initial ingot structure, *A* of Row 15, is refined by heating and rolling to that shown in *A* of Rows 16 and 17, the elevation of A3 caused by phosphorus, though less, is yet very marked. Thus even at 870°, or 50° above the normal position of A3 for this carbon content, great masses of unreabsorbed ferrite may remain, as shown in *E* and *F* of Row 16. After the temperature has been raised high enough to complete the reabsorption of ferrite and is again lowered, precipitation is very marked at 830° (*D*, Row 17). The lag is very great, the quantity of ferrite precipitated in a 30-min. stay at 830° being incomparably less than that unreabsorbed in a like stay at 870° (*F*, Row 16).

The black-centered islets in *D* and *E* of Row 15, the median inclosures in the network of *F*, Row 16, the rectangular ingot structure in *A*, *B*, and *C*, Row 15, and the dark bands about the ferrite network of *C* and *F*, Row 16, are of interest, as are also the suggestions of ingot structures in the dark and light bandings of *D*, Row 15, and even in the fine-grained *A*, Rows 16 and 17, and the coexistence in parts of Fig. *A*, Row 15, of four pretty distinct concentric parts, the dark phosphoric nuclei, the light gray bands round them, the darker gray beyond, and outside of all the dark pearlitic areas.

Though the rolled bar when re-coarsened (*B*, Rows 16 and 17) looks at first much like an unphosphoric steel of this carbon content thus coarsened (*A*, Row 5, Plate 2), yet it has some peculiarities. Note the dark parting between some of the polygon boundaries and the internal ferrite, recalling *C* and *F* of Row 16. It may be more than a fancy that the network is more continuous, smoother and more rounded than in unphosphoric steel, and the Widmanstättian structure, though recognizable, yet less marked than would be expected in view of the very high temperature of coarsening, a temperature so high that part of the specimen melted. Evidence that the network in this coarsened piece is really unusually smooth and continuous is given by the certainly unusual continuity and smoothness of the network after much reabsorption, as in *C* and *F*, Row 16.

The summary of this paper will be found on pp. 595 and 596.

Thermal and Microscopical Examination of Professor Howe's Standard Commercial Steels.*

BY G. K. BURGESS, J. J. CROWE, AND H. S. RAWDON, U. S. BUREAU OF STANDARDS, WASHINGTON, D. C.

(New York Meeting, October, 1913.)

I. Cooling and Heating Curves.

BY G. K. BURGESS AND J. J. CROWE.

§ 22. The results published in Professor Howe's paper[10] of our determinations on the Ac3 and Ar3 points for a series of commercial carbon steels[11] containing manganese in varying proportion, represent the average temperatures found by taking several (4 or 5) heating and cooling curves.

The accompanying curves, Figs. 6 and 7, are, for each steel, the first heating and cooling curves as obtained by the inverse-rate method of Osmond, using a cylindrical chronograph sensitive to 0.1 sec. for recording time. Simultaneous observations were also taken by the differential method with a platinum neutral and plotted by Rosenhain's derived differential method and gave substantially the same results, the curves having sensibly the same contours in both cases.[12]

The observations were taken *in vacuo* on cylinders $\frac{5}{8}$ by 1 in. in length bored to center with a $\frac{1}{8}$-in. hole to receive porcelain insulated thermo-couple, and inclosed in an out- and in-glazed Berlin porcelain tube heated electrically by platinum foil wound on the tube. The vacuum was usually better than 0.1 mm. The heating and cooling of the furnace was automatically controlled by a salt-water rheostat with shaped electrodes, giving a uniform rate of heating and cooling.

* MS. received May 14, 1913.

[10] H. M. Howe, Life History of Network and Ferrite Grains in Carbon Steel. *Proceedings of the American Society for Testing Materials*, vol. xi., pp. 262 to 386 (1911). See pp. 330 to 332.

[11] The specimens sent to the Bureau of Standards were untreated sections cut from the rods of the various steels, in the condition in which they were received from the makers. H. and L.

[12] For a comparison of cooling curve methods see G. K. Burgess, *Bulletin No. 5, U. S. Bureau of Standards*, pp. 199 to 225 (1908).

Alternating current was used from a motor-generator driven by a storage battery, insuring the greatest steadiness in the heating current. Contrary to the usual practice, the furnace had almost no insulating covering, so that it responded promptly to impressed quantities of heat. This arrangement also facilitates running the furnace rapidly from high to low temperatures.

TABLE II.—*Determination by Burgess and Crowe of the Transformation Points of the Steels of Howe and Levy, by Means of Heating and Cooling Curves.*[c]

No.		Low-Manganese Steels. VI.	IV.	I.	VII.	VIII.	IX.	High-Manganese Steels. III.	II.
Ac1	1[a]	743±10	754±5	754±5	761±5	746±10	751±15	738±10	730±5
	2	755±5	762±5	757±5	768±5	760±10	759±20	745±10	736±5
	3	762±5	777±10	770±10		774±10	772±20	761±10	764±10
Ac2	1	.....	.....	.....	.....	.....	.....	.....	.....
	2	784±10	786±10	.....	.....	.....	.....	776±10	?
	3	.....	.....	.....	.....	.....	.....	.....	.....
Ac3	1	?	?	?	?	?	?	?	?
	2	901±5	852±20	?	778±5[b]	780±15[b]	772 ?[b]	836±10	?
	3	916±5	865±15	828±10	790±5	786±15	800 ?	854±20	784±10
Ar3	1	889±10	820±10	754±5	.....	.....	.....	786±10	724 ?
	2	870±5	796±15	712±5	?	770±15[b]	?	763±15	?
	3	.....	.....	?	.....	.....	.....	.....	?
Ar2	1	778±5	?	.....	.....	.....	.....	?	.....
	2	765±5	.....	.....	.....	.....	.....	.....	.....
	3	.....	.....	.....	.....	.....	.....	.....	.....
Ar1	1	694?	682±15	695±2	707±15	708±10	712±5	630±3	648±3
	2	671±5	678±15	688±2	700±15	704±10	708±5	620±5	646±5
	3	668±20	666±20	666±5	662±20	666±10	678±5	600±10	611±10
C	.....	0.03	0.21	0.40	0.92	1.14	1.45	0.14	0.46
Si	.....	.....	0.039	0.103	0.123	0.124	0.14	0.040	0.095
Mn	.....	0.26	0.05	0.16	0.14	0.24	0.16	1.16	1.21
P	.....	0.005	0.013	0.014	0.009	0.016	0.009	0.020	0.017
S	.....	0.024	0.010	0.012	0.011	0.009	0.006	0.018	0.015

[a]—1 = Beginning, 2 = maximum, 3 = end.

[b] These very faintly marked points may be A2 points.

[c] These determinations differ in only a few points from those which we gave Professor Howe for his paper The Life History of Network and Ferrite Grains in Carbon Steel, *Proceedings of the American Society for Testing Materials*, vol. xi., p. 262 (1911).

In the figures, the ordinates are e.m.f.'s of the platinum-rhodium thermo-couple used, and also the corresponding temperatures centigrade of the sample; the abscissæ for each specimen are times in seconds for the specimen to cool 2° C. Each dot represents an actual observation. A vertical line indicates a uniform rate of cooling

or heating. The furnace was checked out with a platinum blank, also shown. For each specimen the times of heating and cooling for the plotted temperature range are indicated. The direction of the arrows shows whether it is a cooling or heating curve.

These curves appear to be fairly representative of what may be expected for commercial carbon steels containing manganese. The impurities appear to mask somewhat the sharpness of the A3 points and, when present, of the A2 points. The effect of increasing the manganese, as has been shown elsewhere, is to lower the A1 points and further separate Ar1 and Ac1.

The average thermal behavior, together with their analyses as given by Professor Howe, is shown in the accompanying table for each of these steels.

II. Microscopical Examination for Decarburization Upon Heating.

BY H. S. RAWDON.

§ 23. *Sample No. 1.*—Carbon 0.40 per cent.

Thermal treatment: Heated to 978° C., cooled in furnace. Repeated, heated to 1,005° C.

Sections examined:

(*a*) End of cylinder used for cooling curve.

(*b*) Section 5 mm. below end.

(*c*) Section from original unheated rod.

Results: Estimated carbon by amount of precipitated pearlite, assuming pearlite contains 0.85 per cent. of carbon.

(*a*) C, — 0.20 to 0.30 per cent. Average C, 0.23 per cent.

(*b*) C, — 0.22 to 0.48 per cent. Average C, 0.30 per cent.

(*c*) C, — 0.34 to 0.38 per cent. Average C, 0.36 per cent.

Average C, 0.27 per cent. for (*a*), (*b*), and (*c*).

A *very thin* skin around the longitudinal hole in the cylinder for the thermo-couple was entirely decarburized.[13] No *systematic* variation in carbon content could be noted.

Sample No. 2.—Carbon 0.46 per cent.

Thermal treatment: Heated to 972° C., cooled in furnace. Repeated, heated to 974° C.

Sections examined:

(*a*) End of cylinder used for cooling curve.

(*b*) 4 mm. below (*a*).

(*c*) 4 mm. below (*b*).

(*d*) From original unheated rod.

[13] This decarburization must have occurred in the heatings *in vacuo* at the Bureau of Standards, as the specimens sent there had undergone no heat treatment.—H. & L.

Results:

(*a*) C, 0.18 to 0.50 per cent. Average C, 0.32 per cent.

(*b*) C, 0.75 per cent.

(*c*) C, practically same as (*b*).

(*d*) C, practically same as (*b*).

(In (*c*) and (*d*) pearlite area was not measured, but sections were compared with (*b*)).

Under high magnification the pearlite was not of the lamellar type but suggested sorbite. Carbon seems much higher than chemical analysis calls for; may be due to the action of high manganese.[14]

Sample No. 3.—Carbon 0.16 per cent.

Thermal treatment: Heated to 978° C., cooled in furnace.

Sections examined:

(*a*) End of cylinder used for cooling-curve determination.

(*b*) End of cylinder used for cooling-curve determination after deep grinding.

Results:

(*a*) C, 0.17 per cent.

(*b*) C, 0.13 to 0.19 per cent. Average C, 0.16 per cent.

Agrees with the chemical analysis (C, 0.16 per cent.).

Sample No. 4.—Carbon 0.21 per cent.

Thermal treatment: Heated to 978° C., cooled in furnace. Repeated, heated to 976° C.

Sections examined:

(*a*) End of cylinder used for cooling-curve determination.

(*b*) Other end of cylinder used for cooling-curve determination after deep grinding.

(*c*) Original rod before heating.

Results:

(*a*) C, 0.10 to 0.16 per cent. Average C, 0.13 per cent.

(*b*) C, 1.13 to 0.22 per cent. Average C, 0.17 per cent. Average C, 0.15 per cent. for (*a*) and (*b*).

Around central longitudinal hole and at edge of section amount of pearlite is somewhat less for slight depth.

Sample No. 6.—Carbon 0.03 per cent.

Thermal treatment: Heated to 1,024° C., cooled in furnace. Repeated, heated to 1,024° C.

[14] Howe's paper, in which the early experiments on these steels were described, showed that manganese has the effect which Mr. Rawdon here infers. *Proceedings of the American Society for Testing Materials*, vol. xi., pp. 365, 367, Proposition 15 (1911).

Sections examined:

(*a*) Upper end of cylinder used for cooling curve.
(*b*) Other end of cylinder used for cooling curve.
(*c*) Section 5 mm. from end of cylinder.
(*d*) Section from original unheated bar.

Results:

(*a*) End of cylinder nearly free from pearlite.
(*b*) Same as (*a*).
(*c*) Nearly a complete ring 2 mm. deep contained no pearlite. Microphotograph 6 *A* is a view of part of this area.
(*d*) Shows some pearlite; free ring as (*c*). Microphotograph 6 *B*, Plate I., is a view of part of this area.

Otherwise the interiors of (*c*) and (*d*) are normal and showed about 0.08 per cent. of C. (did not use micrometer eye-piece).

Sample No. 7.—Carbon 0.92 per cent.

Thermal treatment: Heated to 972° C. for cooling-curve determination, cooled in furnace. Repeated, heated to 972° C.

Sections examined:

(*a*) End of cylinder used for cooling-curve determination.
(*b*) Other end of cylinder after deep grinding.

Results:

(*a*) Showed as a compact mass of pearlite, no ferrite.
(*b*) Same as (*a*).

Etched with sodium picrate, showed faint traces of free cementite.

Sample No. 8.—Carbon 1.16 per cent.

Thermal treatment: Heated to 974° C., cooled in furnace. Repeated, heated to 994° C. Repeated, heated to 986° C.

Sections examined:

(*a*) Upper end of cylinder used for cooling-curve.
(*b*) Other end of cylinder used for deep grinding.
(*c*) Original unheated bar.

Results:

(*a*) The surface pits very badly on polishing; almost impossible to prevent it. After picric acid etching, the section showed as a mass of pearlite with much free cementite forming the outlines of the grains. After sodium picrate etching the outlines of the cementite show plainly with the many polishing pits. The amount of cementite appears to be slightly less than the carbon content demands. See Microphotograph 8 *A*.

(*b*) Similar in every repect to (*a*).

(*c*) Similar to (*a*) except the free cementite is not all collected in the outlines of the grains. See Microphotograph 8 *B*. Polishing pits are not produced to any extent.

Sample No. 9.—Carbon 1.45 per cent.

Thermal treatment: Heated to 1,056° C., cooled in furnace. Repeated, heated to 1,055° C., cooled in furnace.

Sections examined:

(*a*) Upper end of cylinder used for cooling curve.

(*b*) Other end of cylinder after deep grinding.

(*c*) Section 8 mm. from end of cylinder.

(*d*) Section of unheated bar.

Results:

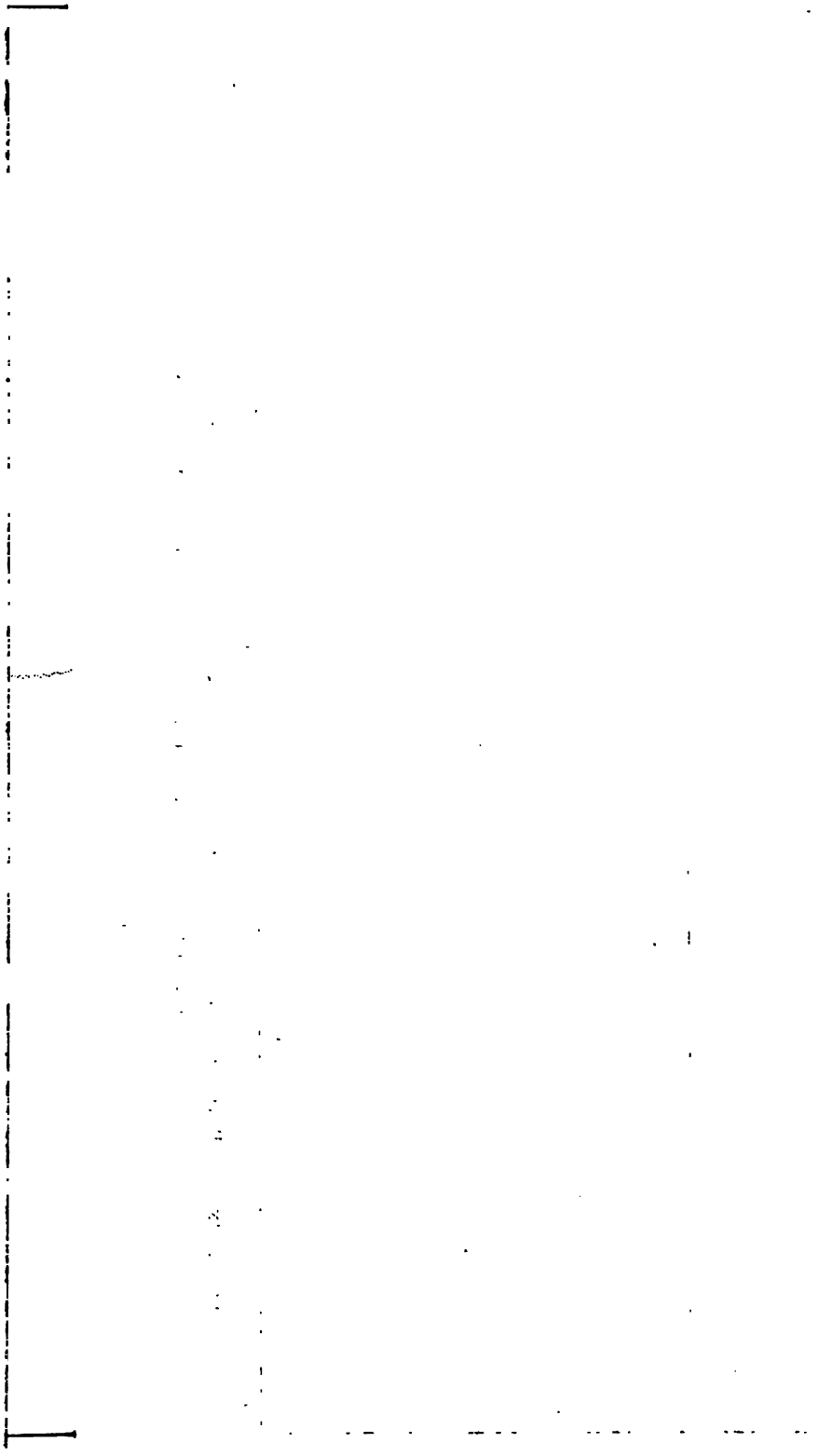
(*a*) The surface pits very badly upon polishing. Some free cementite is seen after picric acid etching together with ground mass of pearlite. After sodium picrate the free cementite shows more plainly together with the many polishing pits. Section is quite varied; some spots show about the correct amount of cementite occurring as outlines for the grains; in other spots the free cementite is entirely wanting; still others show it as isolated masses; on the whole, the amount appears considerably less than should be expected. Microphotograph 9*A* shows this diminution of cementite, though it is not typical of the whole section.

(*b*) Similar to (*a*) except more cementite occurs as outlines for the grains.

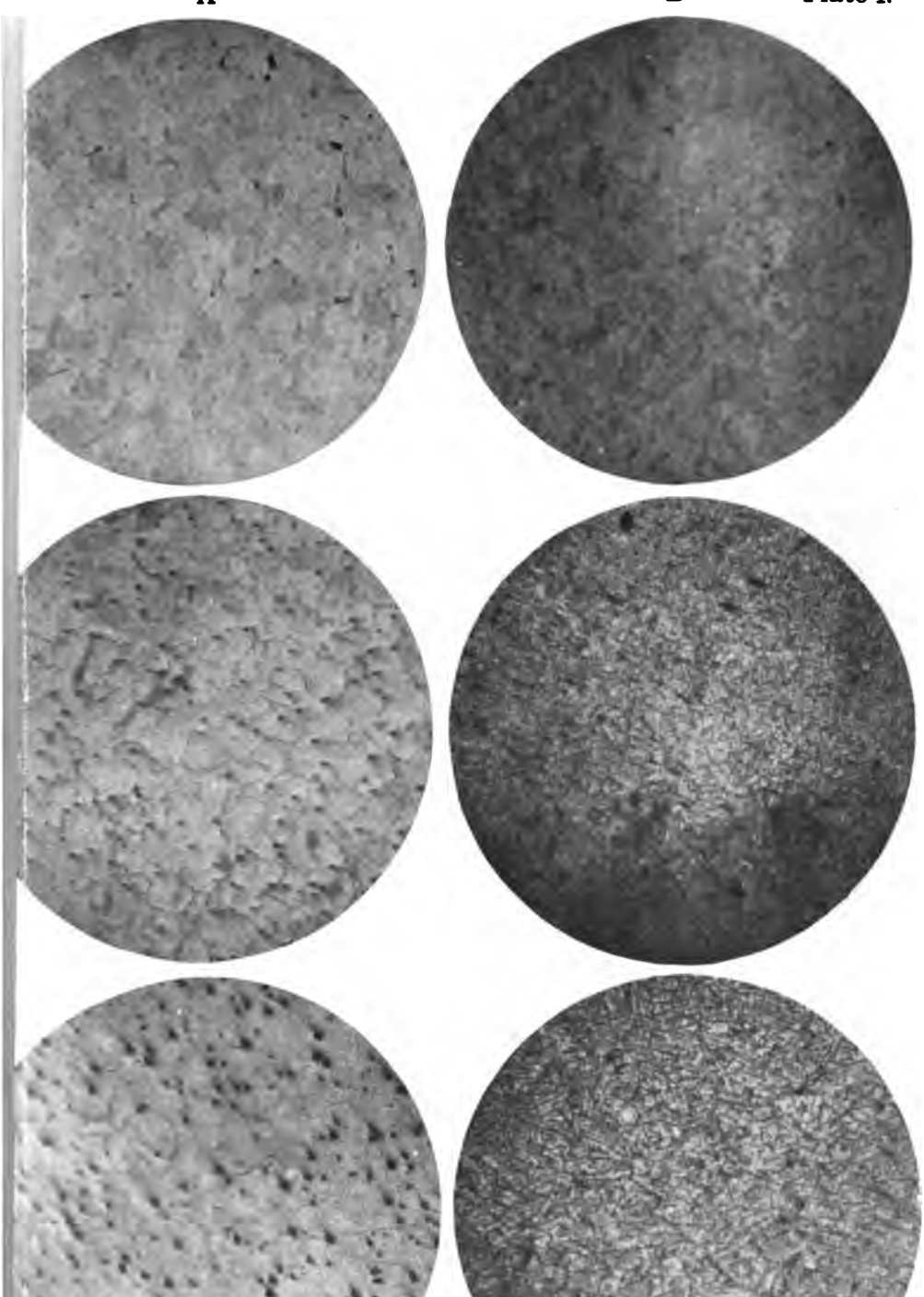
(*c*) The section is nearly uniform, though the amount of cementite which occurs as outlines for the grains appears slightly less than would be expected.

(*d*) The section, after sodium picrate etching, showed the cementite largely distributed throughout the interior of the grains. Polishing pits were almost entirely absent. Microphotograph 9 *B*.

On the whole, the amount of cementite appears somewhat less than the carbon content calls for; this is especially true of the end of the cylinder.



A B Plate 1.



A B

Discussion of the Existing Data as to the Position of Ae3.*

BY H. M. HOWE,† NEW YORK, N. Y.

(New York Meeting, October, 1913.)

PART I. INTRODUCTORY.

§ 24. INTRODUCTION.—This paper discusses the chief existing data as to the temperature, in iron-carbon alloys, of Ae3, the upper limit of the transformation range when in equilibrium, as distinguished from Ac3, the temperature at which the end of the transformation is noted in heating up, and from Ar3, that at which its beginning is noted in cooling down. The former is in fact carried above Ae3 and the latter below Ae3 by lag. I touch incidentally on the influence of manganese on Ae3 (§ 29, p. 614).

The sections, tables, and illustrations are numbered consecutively with those in the accompanying papers by Howe and Levy on Determination of the Position of Ae3 in Carbon-Iron Alloys, and by Burgess, Crowe, and Rawdon on Thermal and Microscopical Examination of Professor Howe's Standard Commercial Steels, to which the present paper forms a sequel.

§ 25. The *contradictoriness of our present evidence* is shown by grouping it together in Fig. 14. For instance, both Goerens and Meyer (Series 2) and Meuthen (Series 12) used practically stationary temperature, and thus reduced the influence of lag very greatly; yet Ae3 for 0.32 carbon steel according to the former investigators is 894°, but according to Meuthen it is at or below 770°, a discrepancy of 124° or more. In view of our ability to measure these high temperatures within a few degrees, the existence of such a discrepancy is as unwelcome as it seems needless. I attack the thankless task of bringing some order out of this chaos.

In Part II. (§§ 29 to 39) I consider the chief causes of error. In Part III. (§§ 40 to 48) I apply certain corrections to some of the existing data, review those data, weight them, and calculate a prob-

* A contribution from the Metallurgical Laboratories of Columbia University. MS. received May 14, 1913.

† Professor of Metallurgy, Columbia University.

able position of Ae3, which is shown in Fig. 14 as the broad black line *AAA*. But this calculated line is not entitled to great weight, because of the weakness of the evidence. Much better data are needed, reached with pure materials and with the many causes of error reduced to a minimum. In particular, great chemical homogeneousness should be assured by microscopic examination; surface decarburization should be prevented; and lag should be reduced to a minimum by using stationary-temperature methods, such as the micrographic and dilatational, and preferably by using two such methods on the same specimens. The thermal methods, useful as they are for reconnaissances, should be used only as a check. This is a task worthy of a national laboratory, especially in view of the industrial importance of Ae3.

§ 26. *The Industrial Importance of Ae3.*—Industrially this is one of the most important lines in the carbon-iron diagram, and perhaps the most important. When medium-carbon and high-carbon hypoeutectoid steels, such as are fitted for forgings, the better classes of shafting, and like engineering purposes, remain undisturbed for any important length of time at temperatures anywhere between 700° and the upper part of the transformation range, their ferrite coalesces into harmfully large particles. This is true, though in a less degree, of the alloy steels which are likely to compete for such purposes with these carbon steels, such as low-nickel steel and low-manganese steel. To undo this damage and give the metal even approximately the best properties of which it is capable, a first step is to cause the reabsorption of that ferrite, and the effacement of the local segregation and the heterogeneousness which its coalescence constitutes. This reabsorption and this effacement are brought about by heating to Ae3, and holding there long enough to allow diffusion to become thorough, and the ferrite to be reabsorbed, or in short by "grain refining." This refining or diffusion process should be followed by as rapid a cooling through the transformation range as is practicable, because a slow cooling through that range would enable the ferrite again to coalesce into masses which are harmfully large, though of course much smaller and less harmful than those which form when that cooling is from a much higher temperature still.[15]

[15] When forging is done properly, and in particular is continued till the temperature has sunk at least to the lower part of the transformation range, the degree of coalescence of the ferrite in the remaining cooling is relatively slight. Even that slight coalescence may be removed by the process of grain refining. But if the cooling down from this grain refining is slow, the whole process is likely to do more harm than good, because coarser ferrite coalescence would arise in the slow cooling of a large forging from Ae3 than ought to remain at the end of the forging operation. Unless the cooling can be made fairly rapid

The grain refining of the core of case-hardened objects requires a like reheating to Ae3.

These engineering objects are now so important, and are likely to become so much more important, that trustworthy knowledge of the temperature of this therapeutic line Ae3 is much needed.

Observers have not hitherto sought Ae3, but have been content to determine points easier to find, such as the beginning of Ar3, which must lie an indeterminate distance below Ae3, or even the maximum of Ar3, which lies an indeterminate distance lower yet.

§ 27. *Industrial A3 vs. A3 of Cooling-Curve Method.*—Osmond showed what we should naturally expect, that the lag increases with the rapidity of cooling. Thus it occurs that, in the determination of A3 by the usual method of taking cooling curves and heating curves in which cooling or heating is relatively rapid, there is greater lag than in the heating of steel for industrial heat-treatment, for here the temperature usually rises slowly, especially as it approaches the peak temperature aimed at, and in general it remains stationary at that peak temperature. Hence in these industrial heatings the actual Ac3, the temperature at which the reabsorption of the pro-eutectoid ferrite is actually complete, lies nearer to the equilibrium temperature of A3, Ae3, than the A3 as determined by cooling and heating curves does. Indeed, it is probable that in the industrial heating of large objects the lag is negligible, the reabsorption of ferrite practically completing itself at each spot as the temperature of that spot reaches Ae3, though of course the heating of the interior may lag many degrees behind that of the outside. Hence at first sight we infer that the temperature to which heating should be directed should be exactly Ae3, above the beginning of Ar3 as found by cooling curves and below the beginning of Ac3 as found by heating curves.

§ 28. *Should Industrial Grain Refinings Aim at Ae3 or at Some Other Temperature?*—This inference may have to be modified because of a practical consideration. Are the best mechanical properties given by exactly completing the reabsorption of the ferrite, or by leaving that reabsorption slightly incomplete? We must remember that though in industrial heatings we aim at a specific degree of temperature, what we really hit is a range, in about the middle of which that degree lies. We do not bring even the immediate vicinity of our thermo-junction, calorimetric ball, or whatever, to the specific degree

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at least to 700°, grain refining should be replaced by a moderate reheating sufficient to relieve the forging stresses and to give the needed ductility, usually to between 500° and 650°. In the case of large objects the cooling from this should of course be slow, lest new stresses arise.

at which we aim, but into a range including that degree, a range more or less narrow according to our skill and care. But the temperature of that vicinity is not the temperature reached by the other parts of the objects we are treating. These other parts may be hotter or they may be colder than that vicinity; so that we bring these other parts not even into the relatively narrow range to which we bring that vicinity, but only into a still wider range of temperature, within which range lies the specific degree of temperature at which we aim. Not only parts but important parts of our objects may rise well above that specific degree, or may not rise to within a considerable range of it.

Therefore we ask whether it is better to err on the high or on the low side; and how the benefit from completing the last of the reabsorption of the ferrite in the cooler parts of our object compares with the harm done by simultaneously carrying the hotter parts of that object appreciably above A3, and thus inducing a certain coarsening of the austenite grains, and of the resultant ferrite network formed on re-cooling.

Indeed, there is evidence to suggest that even before the temperature reaches A3, *i. e.*, even before the last of the ferrite is reabsorbed, there is a material coarsening of the grain of the austenite which is mixed with that ferrite, a coarsening which is likely to be harmful.

This question whether it is exactly A3 or some temperature slightly higher or slightly lower that we should aim at industrially must be answered by direct experiments, of which some are in hand.

Part II. The Chief Causes of Error.

§ 29. *The Influence of Manganese.*—I have pointed out[16] that manganese has a general sluggardizing or retarding influence on the chemical and structural changes in steel, retarding (1) the formation and (2) the break up of the ferrite network; (3) the coagulation of ferrite into recognizable masses; (4) the growth of the ferrite grains, thus lessening the size which they reach; (5) the growth of the lamellæ of pearlite, thus lessening their size, probably both directly and through lowering the temperature of Ar1 at which they are born; (6) and the growth of the austenite grains, thus lessening the size which they reach: and (7) lessening the coarseness of the ferrite network based on that grain size. Further, (8) that we may read in Osmond's diagram, Fig. 8, a like retarding effect on the part of nickel; for though the lowering of the Ac points indeed indicates that nickel

[16] *Proceedings of the American Society for Testing Materials*, vol. xi., p. 365 (1911).

lowers the Ae points, Ae1, Ae2, and Ae3, yet the continuous widening of the gap between the Ac and the Ar points as the nickel content increases explains itself as a result of this retarding action, for such an action should have just this effect, as regards the Ar points reinforcing the effect of the lowering of the Ae points, but as regards the Ac points opposing that effect.

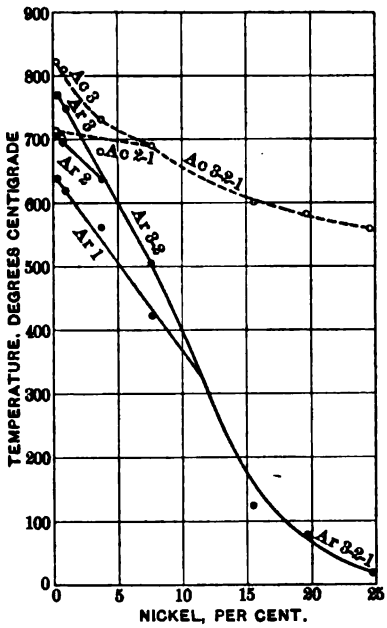


FIG. 8.—OSMOND'S IRON-NICKEL DIAGRAM.
(After Institution of Civil Engineers, Proceedings, Vol. 138, 1898–99).

Further evidence that nickel has such a retarding influence is given by its causing some structural effects like those which I have just credited to the retarding action of manganese; for instance, retarding the growth of the austenite grains, and hence of the ferrite network based on those grains, so that nickel steel coarsens and embrittles on long heating less readily than carbon steel of like carbon content. To a like sluggardizing effect we may refer the fact that with 25 per cent. of nickel, or with 11 per cent. of manganese plus 1 per cent. of carbon, the transformations are so far retarded that the austenite state persists even after common slow cooling, whereas with an intermediate quantity of either element the transformation in a common slow cooling goes only as far as the state of martensite.

TABLE III.—*Influence of Manganese on the Temperature of the Limits, A1 and A3, of the Hypo-Eutectoid Transformation Range.*

No.	Authority.	Composition C. Per Cent.	Composition Mn. Per Cent.	Ar1. Beginning. Observed.	Ar1. Beginning. Actual Lowering.	Ar1. Beginning. Lowering Calculated for 1% Mn.	Ar1. Maximum. Observed.	Ar1. Maximum. Actual Lowering.	Ar1. Maximum. Lowering Calculated for 1% Mn.	Ac1. Beginning. Observed.	Ac1. Beginning. Actual Lowering.	Ac1. Beginning. Lowering Calculated for 1% Mn.	Ac1. Maximum. Observed	Ac1. Maximum. Actual Lowering.	Ac1. Maximum. Lowering Calculated for 1% Mn.
1	.....	0.57	0.23	.....	.....	.....	**661**	.....	.....	.....	.....	.....	.....	.....	.....
2	Osmond	0.29	0.27	.....	.....	.....	**660**	.....	.....	.....	.....	.....	.....	.....	.....
3	(uncor-	0.32	0.50	.....	.....	.....	640	20	80	.....	.....	.....	.....	.....	.....
4	rected).....	0.42	1.00	.....	.....	.....	625	35	47	.....	.....	.....	.....	.....	.....
5	.....	0.46	1.08	.....	.....	.....	620	40	45	.....	.....	.....	.....	.....	.....
6	.....	0.36	0.42	**680**	.....	.....	**675**	.....	.....	**720**	.....	.....	**7 5**	.....	.....
7	.....	0.48	0.45	**670**	.....	.....	**665**	.....	.....	**715**	.....	.....	**735**	.....	.....
8	.....	0.36	0.52	660	15	170	655	15	170	720	.....	.....	735	.....	.....
9	Saladin.....	0.22	0.53	655	20	200	645	25	250	710	.....	.....	735	.....	.....
10	.....	0.46	0.63	680	.....	.....	665	5	25	702	.....	.....	720	.....	.....
11	.....	0.66	0.71	680	.....	.....	665	5	18	720	.....	.....	735	.....	.....
12	.....	0.31	1.66	630	45	37	610	60	45	730	.....	.....	745	.....	.....
13	.....	0.90	4.16	620	55	15	585	85	25	730	.....	.....	740	.....	.....
14	Carpenter, Hadfield &	0.47	trace	**770**	.....	.....	**6**	.....	.....	.....	.....	.....	.....	.....	.....
15	Longmuir.	0.47	0.95	706	64	67	665	25	26	.....	.....	.....	.....	.....	.....
16	.....	0.38	0.51	**675**	.....	.....	**683**	.....	.....	**712**	.....	.....	**725**	.....	.....
17	.....	0.44	1.23	655	20	28	666	17	24	730	.....	.....	745	.....	.....
18	.....	0.46	1.23	660	15	21	666	17	24	735	.....	.....	742	.....	.....
19	Sargent.....	0.56	1.28	652	23	30	655	28	36	725	.....	.....	740	.....	.....
20	.....	0.58	1.28	652	23	30	657	26	34	720	.....	.....	735	.....	.....
21	.....	0.62	1.66	642	38	29	655	28	24	720	.....	.....	737	.....	.....
22	.....	0.21	0.05	**682**	.....	.....	**678**	.....	.....	**754**	.....	.....	**762**	.....	.....
23	Burgess	0.14	1.16	630	52	47	620	58	52	738	16	14	745	17	15
24	and Crowe	0.40	0.16	**695**	.....	.....	**688**	.....	.....	**754**	.....	.....	**757**	.....	.....
25	.....	0.46	1.21	648	47	45	646	42	40	730	24	23	736	21	20
26	.....	0.21	0.05	.....	.....	.....	**695**	.....	.....	.....	.....	.....	**730**	.....	.....
27	Howe and	0.14	1.16	.....	.....	.....	645	50	45	.....	.....	.....	730	0	0
28	Levy.....	0.40	0.16	.....	.....	.....	**705**	.....	.....	.....	.....	.....	**735**	.....	.....
29	.....	0.46	1.21	.....	.....	.....	664	41	40	.....	.....	.....	728	7	6
30	.....	0.86	0.05	.....	.....	.....	.....	.....	.....	**730**	.....	.....	.....	.....	.....
31	Stead.....	0.88	0.56	.....	.....	.....	.....	.....	.....	728	2	4	.....	.....	.....
32	.....	0.89	0.94	.....	.....	.....	.....	.....	.....	720	10	11	.....	.....	.....
33	.....	1.00	2.04	.....	.....	.....	.....	.....	.....	715	15	7	.....	.....	.....

No.	Authority.	Composition C. Per Cent.	Composition Mn. Per Cent.	Ar3. Beginning. Observed.	Ar3. Beginning. Actual Lowering.	Ar3. Beginning. Lowering Calculated for 1% Mn.	Ar3. Maximum. Observed.	Ar3. Maximum. Actual Lowering.	Ar3. Maximum. Lowering Calculated for 1% Mn.	Ac3. End. Observed.	Ac3. End. Actual Lowering.	Ac3. End. Lowering Calculated for 1% Mn.	Ac3. Maximum. Observed	Ac3. Maximum. Actual Lowering.	Ac3. Maximum. Lowering Calculated for 1% Mn.
34	.....	0.57	0.23	**750**	.....	.....	**695**	.....	.....	.....	.....	.....	.....	.....	.....
35	Osmond	0.29	0.27	**780**	.....	.....	**717**	.....	.....	.....	.....	.....	.....	.....	.....
36	(uncor-	0.32	0.50	740	37	148	700	15	60	.....	.....	.....	.....	.....	.....
37	rected).....	0.42	1.00	725	41	55	665?	42	56	.....	.....	.....	.....	.....	.....
38	.....	0.46	1.08	725?	37	45	658?	15	54	.....	.....	.....	.....	.....	.....
39	.....	0.03	0.26	**889**	.....	.....	**870**	.....	.....	**916**	.....	.....	**908**	.....	.....
40	Burgess	0.21	0.05	**820**	.....	.....	**796**	.....	.....	**865**	.....	.....	**852**	.....	.....
41	and Crowe	0.40	0.16	**754**	.....	.....	**712**	.....	.....	**828**	.....	.....	.....	.....	.....
42	.....	0.14	1.16	786	62	62	763	60	60	854	30	30	836	28	28
43	.....	0.46	1.21	724?	10	9	.....	.....	.....	784	30	29	.....	.....	.....

NOTES TO TABLE III.—**1** to **5** and **34** to **38**. Osmond, *Transformation du fer et du carbone*, pp. 41, 48, and 93 (Paris, 1888). Rolled or forged rods, apparently 5 to 7 mm. in diameter or width, and 60 cm. long, were heated in a porcelain tube in a gas furnace with incomplete exclusion of air. The rate of cooling was between 1 and 2 sec. per degree.

6 to **13**. Saladin, *The Iron and Steel Magazine*, vol. vii., p. 237 (1904). For Nos. 6, 7, 9, and 11, the curves were "obtained in 10 minutes" or at the rate of about $\frac{1}{3}$ or $\frac{1}{4}$ sec. per degree. For Nos. 8 and 10 the cooling from 1,050° to 450° lasted one hour, or at the rate of 6 sec. per degree. The heating from 0° to 1,050° lasted one hour, or at the rate of about 3.5 sec. per degree. For Nos. 12 and 13 the rate of cooling is not given. The specimens appear to have been small cylinders.

14 to **15**. Carpenter, Hadfield, and Longmuir. Seventh Report Alloys Research Committee, *Proceedings of the Institution of Mechanical Engineers*, 1905, pp. 916 to 921. Cylinders $\frac{5}{8}$ in. long and $\frac{5}{8}$ in. in diameter, machined from cast material, were heated in an

electrical tube furnace in air. Cooling from 900° to 50° required about 2.25 hours, or at the rate of 9.5 sec. per degree.

16 to 21. G. W. Sargent, Metallurgist of the Crucible Steel Company of America, private communication, May 1, 1911. Cylinders 0.75 in. long and 0.75 in. in diameter were used. The rates of heating and cooling are not given.

22 to 25 and 39 to 43. Burgess and Crowe. See p. 605, of this volume. Rolled cylinders 0.5 to 1 in. long and $\frac{1}{8}$ in. in diameter were used. The rate of heating and cooling was about 5 to 7 sec. per degree.

26 to 29. Howe and Levy. Rolled cylinders about 1 in. long and $\frac{1}{8}$ in. in diameter were used. The rates of heating and cooling varied from 3 to 12 sec. per degree. Their specimens and those of Burgess and Crowe were cut from the same bars.

30 to 38. Stead, *Proceedings of the North-East Coast Institution of Engineers and Shipbuilders*, vol. xxix., 1912–1913, p. 19 of reprint.

Having thus found that nickel both lowers Ae1 and Ae3 and also retards the transformations and some at least of the structural changes; that manganese certainly retards these structural changes and also lowers thermal Ar1 as shown in 1888 by Osmond; and that manganese and nickel behave alike in a general way, we ask whether this resemblance does not go farther, the lowering effect of manganese applying not only to Ar1 but also to both Ae1 and Ae3, and its retarding effect leading to a widening of the gap between the Ac and the Ar points like that caused by the retarding effect of nickel. Let us consider the evidence.

(1) Thermal A1.—Ar1. The evidence collected in Table III. bears out Osmond's early discovery that manganese lowers Ar1, but shows that it does not always lower Ac1. To show this I have set in broad faced type in that table the data for steels relatively poor in manganese, which may be used as a standard with which to compare the data for those beneath them richer in manganese, the average of *1* and *2* as a standard to try 3 to 5 inclusive, the average of *6* and *7* for 8 to 13, *14* for 15, etc. This shows a lowering of both beginning and maximum of Ar1 in every case except lines 10 and 11, exceptions which may indeed be due to the difficulty of interpreting accurately diagrams on so small a scale as that of these most interesting ones of Saladin's.

Ac1. But in the majority of cases there is either no lowering at all of the beginning or the maximum of Ac1, or at most a lowering which is within the wide limits of experimental error. Indeed on an average both the beginning and maximum of Ac1 appear to be somewhat raised by manganese. These majority data have such advantage as there is in being autographic; but on the other hand they are by two experimenters only, and against them must be weighed the more sensitive determinations of Burgess and Crowe, which show a moderate lowering of both beginning and maximum of Ac1, lines 22 to 25, and the most ingenious determinations of Stead, which show a

constant though slight lowering of Ac1–2–3, in the extreme case at the rate of only 11° per 1 per cent. of manganese.

The widening of the gap caused by this excess of the lowering effect of manganese on Ar1 over its effect on Ac1 is shown graphically in Fig. 9.

The inference is strong that manganese increases lag, from the contrast between the almost invariable marked lowering of Ar1, and the rarer lowering of Ac1.

(2) Thermal A3.—In the 0.14 and 0.46 carbon steels of Burgess and Crowe, Tables III. and IV., about 1 per cent. of manganese lowers Ac3 about 30°, and Ar3 about 60° in one case, but in the other, by a smaller quantity, which cannot be estimated confidently.

In Stead's eutectoid steel manganese lowers Ac1–2–3 slightly, at the rate of 11° per 1 per cent. of manganese in the extreme case as already indicated under Ac1.

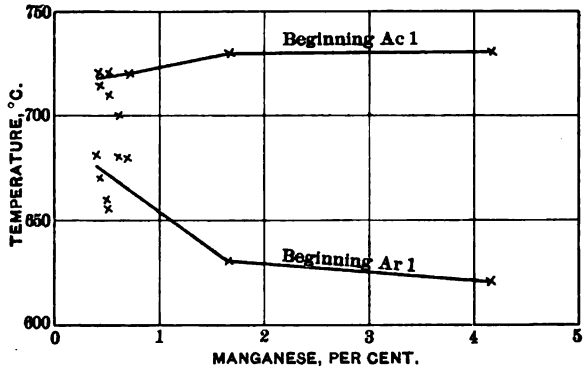


Fig. 9.—Manganese Widens the Gap between Ac1 and Ar1, Calculated from Saladin's Data.

(3) Micrographic A3.—Though the manganese in the four high-manganese steels of Howe and Levy runs up to 1.21 per cent., yet the lowering of their micrographic Ac3 in no case exceeds 13°. Indeed in two cases, carbon 0.14 and 0.46 per cent., it is about 20° less than the lowering of the Burgess and Crowe thermal Ac3 in these same specimens. This paradoxical result may indeed be due to error in detecting the end of the reabsorption of the ferrite, or it may mean that, in these sluggish high-manganese steels, a small microscopically recognizable segregated residue of ferrite may persist for some 20° after the retardation in the heating curve caused by the ferrite reabsorption has become so slight as to be undetectable thermally.

These results are recapitulated in Table IV.

TABLE IV.—*Lowering Effect of Manganese on the Transformation Points Ac3 and Ar3.*

No.	C. Per Cent.	Mn. Per Cent.	Micrographic (Howe and Levy). Ac3. Lowered by	Thermal (Burgess and Crowe). Ac3 (End) Lowered by	Ar3 (Beginning) Lowered by	Gap Between Thermal Ac3 and Ar3.
VI.	0.03	0.26	.....	Nil	Nil	.....
125	0.11	0.60	Nil	.....	.....	.....
III A.	0.14	1.16	< 10°	30°	63°	Widened by 33° (about 47 per cent).
B.	0.32	0.41	< 10°	.....	.....	.....
II.	0.46	1.21	< 13°	30°?	Very little?	Narrowed by an indeterminate amount.

To sum this up, as regards both A1 and A3 manganese lowers the thermal Ar point very materially.

The Ac points are not clearly lowered by manganese in the results of Saladin, Sargent, and Howe and Levy, but they are in Stead's results and in the sensitive but scanty determinations of Burgess and Crowe.

I infer that manganese probably lowers both Ae1 and Ae3 slightly. In the first place this is antecedently probable from the general close similarity between the effects of manganese and those of nickel, and from the fact that nickel evidently lowers the Ae points. In the second place, the effects of manganese are explained far more readily by the hypothesis that it lowers Ae1 and Ae3 slightly than by the opposite. That it increases lag may be admitted. But unless it lowered the Ae points, its increasing lag would lead to its raising the Ac1 points invariably. In the relatively rapid heatings of Saladin and Sargent the great increase of lag which manganese causes should have a very marked raising effect on Ac1, but in fact the raising effect of even as much as 1.66 and 4.16 per cent. of manganese is only a very few degrees (lines 12 and 13). The invariable slightness of the effect of manganese on the Ac points, taken with the varying sign of such effect as there is, indicates a double effect of manganese on these points. In that it increases lag it tends to raise these Ac points, and in that it lowers the Ae points it tends to lower the Ac points as well. When the former effect is the greater of the two the net effect is to raise the Ac point; when it is the less of the two the net effect is to lower the Ac point. The amount of its lowering of the Ae points should be constant for each specimen, but the amount of its raising effect through lag should vary with the conditions of heating.

Does Manganese Lessen the Eutectoid Carbon Content? The general

belief that it does may be due to its retarding the coalescence of the pro-eutectoid ferrite, with the result that, in steels which are only slightly hypo-eutectoid, the pro-eutectoid ferrite present may escape detection, and thus give the impression that the steel is eutectoid.

If manganese did lessen the eutectoid carbon content, it would probably thereby steepen the line A3, so that, for given high manganese content, the narrowing of the gap between A3 and A1 which increasing carbon content causes should be more rapid than in low-manganese steel, as sketched in Fig. 10. Here the broken lines show qualitatively how the expected effect of manganese would affect the transformation range, if in addition to lowering the observed position of the thermal Ar points from their position *GOS*, *MO*, and *PS*, it also lessened the eutectoid carbon content. Here the distance *GG'* and

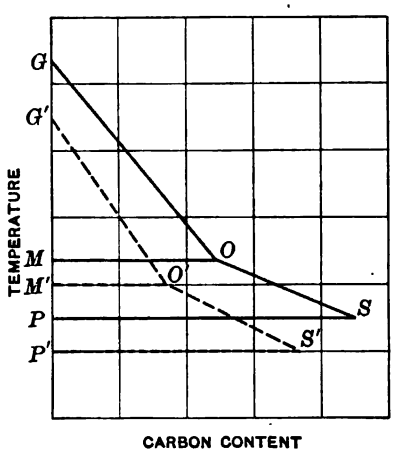


FIG. 10.—TO LESSEN THE EUTECTOID CARBON CONTENT STEEPENS A3, *GO* and *OS*.

MM' and *PP'* would represent the lowering of the transformation points of carbonless or nearly carbonless iron by manganese, but the steepening of *GO* to *G'O'* and of *OS* to *O'S'* would represent the lessening of the eutectoid carbon content.

I have long been on the watch for data throwing light on this lessening. The only ones I have found, those of Burgess and Crowe collected in Table III., do not indicate such a lessening, though they are too scanty to carry much weight. Thus, though the A1 points are lowered by manganese in the 0.14 carbon steel about as much as in the 0.46 carbon steel, the A3 points are, if anything, lowered less in the higher than in the lower carbon steel, pointing rather to an increase than to a decrease of the eutectoid carbon content.

§ 30. *The Influence of Phosphorus.*—§ 21 shows that, at least in unforged castings of about 0.34 per cent. of carbon, phosphorus raises the temperature of Ae3 as found micrographically. How this effect of phosphorus is related to the carbon content will be discussed in § 43.

After consultation with Dr. Stead, I offer the following interpretation of the structural peculiarities of these high-phosphorus specimens shown in Plate 6, Rows 15, 16, and 17, and described in § 21.

The persistence of the dendritic markings represents the extreme slowness with which phosphorus diffuses.

The dark median layers represent phosphoric concentrations, perhaps the phosphorus eutectic in irresoluble masses, persisting because of this same slowness of diffusion.

For the position of the phosphoric matter within the ferrite two reasons suggest themselves. First, in the slow cooling down through the austenite stage, from the solidus towards the transformation range, the phosphoric matter differs enough from the remainder of the austenite to be treated like a foreign body. The growing grains of austenite eject it to their boundaries as the eye ejects a grain of sand, or as every organism strives to eject foreign matter, by surface tension and whatever other means it has. In the passage down through the transformation range that same austenite in turn ejects the ferrite to which it now gives birth, and it must needs pile this later ejection on the earlier, which, like slag particles, no more attracts ferrite than the underlying archaic rocks attract the palæozoic. They simply bear to them their inevitable stratigraphic relation.[17]

The second reason is seen best in the case of the dendrites in Fig. *A*, Row 15, Plate 6. The phosphoric concentrate looks as if it were in the axis of the solidification dendrites, imbedded in the later deposited ferrite, and this in turn surrounded by the dark pearlite, the last of all to solidify. But we readily see the fallacy in this interpretation. Clearly the phosphoric concentrate must have been the last to solidify, so that its position is not in the axis of the dendrites but in the filling between them. The true dendrites, *A* in Fig. 11, are the matter which is now dark and pearlitic; the light ferrite forms the filling, *B*, between these dendrites; and the phosphoric concentrate the axis, *c*, of these fillings. This is the reverse of the initial relation. Initially the low-carbon parts formed the dendrite axes, *A*, and the high-carbon parts the filling, *B*, between.

This reversal is clearly brought about in cooling through the trans-

[17] See the Author, *Proceedings of the American Society for Testing Materials*, vol. xi., p. 375 (1911), and discussion of J. E. Stead's paper, *Proceedings of the International Association for Testing Materials*, VI. Congress, III., to appear.

formation range by a very simple mechanism. Because phosphorus raises A3, and because the phosphorus content shades off from *c* towards *A*, in cooling the beginning of the precipitation of ferrite will occur much earlier at *ccc* than at *A*, and will occur later and later as we pass from *ccc* towards *A*. We have but to assume that the very earliest precipitate of all will adhere to the phosphoric concentrate *ccc* alongside which it is generated, for this would lead to a progressive withdrawing of the austenite towards *A* as it generates ferrite at *ccc*. As each successive concentric layer farther from *ccc* begins generating ferrite, that generation is continuing in the layers *ccc* which earlier started this generation; and through this generation of ferrite which coheres with the earlier formed ferrite against which it is born, the residual austenite is gradually elbowed away from *ccc*. Thus the carbon movement which occurred in solidification is re-

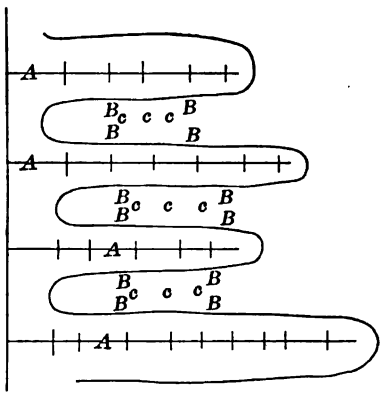


FIG. 11.—TRANSFORMATION THROWS THE CARBON INTO THE AXIAL REGION OF THE DENDRITES.

versed, and the carbon which the differentiation of solidification had concentrated in the fillings, *B*, is by the differentiation of transformation concentrated about the lines *AAAA*, which had been the dendrite axes at the time of solidification.

In a word, the ferrite surrounds the phosphoric concentrates partly because as a later deposit it must needs lie on the earlier, and partly because the enrichment in phosphorus of the surrounding austenite leads to both an earlier and a more abundant precipitation of ferrite along the shore of those concentrates, and thus to displacement of the residual mother austenite away from them.

Having admitted this for the dendritic structure of Fig. *A*, Row 15, Plate 6, we see that it suffices to account for the median position of the dark phosphoric masses in the network of Figs. *E* and *F* of Row 16, and for the encircling of the dark phosphoric dots with ferrite in Figs. *C* to *F* of Row 15.

TABLE V.—*Influence of the Chief Causes of Error in Determining A3 (GOS) and Amc (SE).*

	Cause of Error.	Hypo-eutectoid Steel, A3.					Hyper-eutectoid Steel Amc.			
		Method.					Method.			
		Dilatational.	Thermal.		Micrographic.		Thermal.		Micrographic.	
1		3 Cooling down.	4 Cooling Curves.	5 Heating Curves.	6 Precipitation.	7 Reabsorption.	8 Cooling Curves.	9 Heating Curves.	10 Precipitation.	11 Reabsorption.
		Influence of Cause of Error Tends to Make Observed Temperature.								
1	Lag	Slightly too low; avoidable.	Too low.	Too high.	Too low; avoidable.	Slightly too high; avoidable.	Too low.	Too high.	Too low; avoidable.	Slightly too high; avoidable.
2	Surface decarburization		Too high; avoidable.	Too high; avoidable.	Too high; avoidable.	Too high; avoidable.	Too high; avoidable.	Too low; avoidable.	Too low; avoidable.	Too low; avoidable.
3	Persistence of solidificational segregation (of phosphorus and carbon)		Too high.	Too high.	Too high.	Too high.				
4	Persistence of transformational segregation		Too high.	Too high.	Too high.	Too high.	Too high.	Too high.	Too high.	Too high.
5	Precipitation of "quenching" ferrite or cementite				Too high; avoidable.	Too high; avoidable.			Too low.	Too low; avoidable.
6	Martensite mistaken for ferrite				Too high; avoidable.	Too high; avoidable.				
7	Failure to detect first (or last) traces of pro-eutectoid element				Too low (slight).	Too low (slight).			Too low.	Too low.
8	Failure to identify first (or last) traces of retardation.		Indeterminate.				Indeterminate.			

§ 31. *Other Causes of Error.*—These are set forth in Table V. Let us pass them in review.

Lag, Undercooling, or Hysteresis.—The failure of pure water to freeze till after the temperature has fallen materially below 0°, or of the recalescence in steel to set in till the temperature has fallen many degrees, perhaps even 100°, below the temperature at which it is due, Ae1, explains itself as a sort of molecular inertia, or reluctance to break away from the initial grouping and adopt that which thus becomes due. This may be called lag proper.

It is asserted that the melting of a pure substance is not subject to lag of this kind; and this may be true of the transformations which occur in a pure solid substance in heating up. That is to say, both for the freezing point, and for the transformation points which occur below the freezing point in the solid metal, lag proper may occur only in cooling and never in heating up. This philosophically important question need not concern us here. Suffice it to point out that if the solid is composite its actual melting point, and the actual temperature at which a given transformation within it attains a detectable velocity in heating up, may be raised materially above the temperature at which melting or transformation is due, as the least reflection shows. Mix spherical quartz pebbles with spheres of hard corundum and others of hard burnt lime, in the fusible proportion of 6 SiO_2, Al_2O_3, 3CaO, the melting point of which is reported as 1,300°. Set this mixture in a furnace which is at 1,600°. When the spheres reach 1,300° no fusion will occur, because nowhere can any mixture in that fusible proportion actually exist. Silica is in contact with lime at certain points and with alumina at others, but at no point is it in contact with both. No fusion will occur till the temperature reaches the melting point of the most fusible silicate of lime or of alumina, the most fusible mixture of substances actually in contact; because though the silicate of lime and alumina is more fusible than any of its components that silicate cannot form except where those components are in actual contact. Spheres of these three near but not touching each other at all could be held forever at 1,300° without melting, if we ignore sublimation.

Heat up pearlite, and its constituent lamellæ of ferrite and cementite may indeed begin to unite to form austenite at their actual surfaces of contact at the instant of reaching Ae1; but the particles of that ferrite and that cementite which lie back of that contact cannot unite till, by the dissolving action of the resultant contact austenite, they are brought together. As the temperature rises above Ae1 the eutectoid austenite formed at the initial junction is able to dissolve

some of the cementite which adjoins it on one side and some of the ferrite which adjoins it on the other; and these two diffusing out into that austenite enable its alongshore layers to dissolve more ferrite and cementite, and thus the whole of the pearlite at last transforms into austenite. But as long as the temperature is only strictly Ae1, that initial contact mass of austenite can dissolve neither ferrite nor cementite from the masses between which it forms the contact; only as it rises above Ae1 can that dissolving begin, and only as it rises materially above Ae1 can that dissolving be rapid.

The quantity of ferrite and cementite which can unite at exactly Ae1 is evidently extremely small; indeed we could maintain that union can extend only the width of one molecule from each side of the junction line. The heat evolved by this is of course too small to detect. As the temperature rises, and as through this process of diffusion the contact austenite dissolves ferrite and cementite from either side, the resultant heat absorption accelerates till it becomes detectably large. But it seems clear that, other things being equal, the faster the temperature is rising the farther will it have risen at the time when this dissolving process has reached such rapidity as to cause a detectable heat absorption, *i. e.*, the higher will be the observed beginning of the transformation.

This sort of diffusional lag evidently applies to the melting of composite masses including eutectics, and to the transformations in such composite solid masses including eutectoids in heating up. Its circumstances are different from those of the lag in the freezing of a homogeneous liquid, or the transformation of a nominally chemically homogeneous substance like austenite in cooling. But it seems to be subject to the same general influences, and in particular to increase with the rapidity of heating as lag proper increases with the rapidity of cooling. As we shall see in the next section, the grosser form of heterogeneousness caused by solidificational segregation raises the end of Ac3 and the beginning of Ar3, and presumably to a degree which increases with the rapidity with which the temperature is rising or falling.

Of course the heating lag should be much less than the cooling lag. Because there are no nuclei of ferrite present to induce the precipitation of ferrite on cooling to the true solubility line Ae3, that precipitation probably can never begin till the supersolubility or lability line is actually reached, so that a certain degree of lag in cooling is probably inevitable. But it is not clear that any measurable lag need occur in heating up, reabsorption completing itself at Ae3, if the heating is slow enough.

How wide this metastable range is between the solubility curve Ae3 and the supersolubility curve above which precipitation cannot occur on cooling, our present data are too contradictory to show. The wide gap between the end of Ac3 and the beginning of Ar3 in the data of Table IX., after p. 648, indeed point to a pretty wide metastable range. On the other hand the fact that the very careful Ar3 data of Heyn, Series 2 of Fig. 14, are either above or but slightly below the Ac3 data of Burgess and Crowe, Series 4, goes to show that this range is narrow, and so does the fact that in the stationary temperature micrographic determinations of Goerens and Meyer, and in those of Howe and Levy, Ac3 and Ar3 occurred within the same rather narrow temperature gap. It is to be remembered that, though because of lag, the gap between the observed Ac3 and Ar3 may exceed the width of this range by an indeterminate amount, yet it can never be narrower than this range.

Because lag increases with the rate of heating or cooling, the thermal method is at a disadvantage compared with the stationary temperature methods, such as the micrographic, and such as the dilatational may be made, because in these methods the holding at stationary temperature may be made long enough to lessen the effects of lag very greatly, possibly to a quantity which is negligible for our present purposes. It may be possible to carry out the thermal method with coolings and heatings so extremely slow as to reduce lag greatly, indeed to a negligible quantity, but in the experiments here in question this has not been done. The difficulties, especially in the way of a very slow yet regular rise of temperature, are serious.

The actual lowering of the beginning and maximum of Ar1 by hastening the cooling as found by Osmond is as follows:

TABLE VI.—*Influence of Rate of Cooling on Ar1.*

Cooled.	Semi-Hard Steel, 0.57 Per Cent. C.			Hard Steel, 1.25 Per Cent. C.		
	Time of Cooling Between 685°-658°.	Recalescence Ar 1. Beg. ° C.	Max.	Time of Cooling Between 705°-678°.	Recalescence Ar 3. 2. 1. Beg. ° C.	Max.
1. Gently in tube	116 seconds	653	656	49 seconds	671	675
2. As usual in tube	24.5 seconds	648	655	20 seconds	670	680
3. Rapidly in air	Not measured	637	640	Not measured	642	651
4. Very rapidly, quenched in water	Not measured	Absent		Not measured	Absent	
Lowering from lines 1 to 3		16°	16°		19°	24° (29)

Incidental variations in the rate of heating or cooling of the steels used in this investigation caused a change of about this same order of

magnitude in the temperature of Ar1 maximum, but no recognizable change in the temperature of Ac1 maximum, as is shown in Table VII.

TABLE VII.—*Influence of Rate of Heating or Cooling on Thermal Ac1 and Ar1 (Howe and Levy).*

Steel. Series No.	Carbon. Per Cent.	Manganese. Per Cent.	Rate of Heating. Seconds per 1° C.	Ac1. Maximum °C.	Rate of Cooling. Seconds per 1° C.	Ar1. Maximum °C.
I.	0.40	0.16	4	736	.....	.....
.....	.....	.....	8	735	.....	.....
.....	.....	.....	12	731–736	.....	.....
II.	0.46	1.21	4	729	6	661–665
.....	.....	.....	8	730	6½	661–666
.....	.....	.....	14	731	16	666–671
VII.	0.92	0.12	5	739	.....	.....
.....	.....	.....	8	734	.....	.....
.....	.....	.....	11	738	.....	.....
VIII.	1.14	0.24	3	738	.....	.....
.....	.....	.....	3½	737	.....	.....
.....	.....	.....	13	739	.....	.....

Yet Dr. Burgess's more sensitive observations lowered the end of Ac3 by 5° on retarding the rate of heating from 3.9 to 4.3 seconds per degree, and by 20° on retarding it to 9 seconds per degree, as is shown in Table VIII., p 645.

That the lag of A3 may be very considerable is shown by the observations of Burgess and Crowe as pointed out in § 10. With their sensitive measurements there was a gap of from 38° to 74° between the end of Ac3 and the beginning of Ar3 for given specimens.

Turning to line 1 of Table V., both for the hypo-eutectoid and for hyper-eutectoid steel, lag is likely to lower the observed position of Ar3 as noted in cooling curves, and to raise Ac3 as observed in heating curves, and such effect as it has in stationary temperature methods should be of this same kind.

§ 32. *Errors from Local Lowering of the Carbon-Content*, (2), (3), and (4).—In general surface decarburization, and uneffaced segregation, whether solidificational or transformational, tend to raise the observed temperature of A3 of hypo-eutectoid steels above Ae3. Error from this cause is especially to be dreaded in the thermal method, but it may be of importance in the other methods, such as the dilatational and the electrical resistance and even in the micrographic method. A glance at the conditions of the thermal method makes this clear.

The temperature of A3 rises rapidly as the carbon content falls. Hence, if there is superficial decarburization, the A3 of the outside

of the specimen lies correspondingly higher than the A3 which is sought, the A3 corresponding to the chemical composition of the specimen undecarburized. In taking a cooling curve with such superficially decarburized specimen, if for simplicity we ignore lag, a break in the curve will begin as soon as the cooling reaches the Ar3 of the decarburized outer layer, and before it reaches the Ar3 corresponding to the undecarburized steel. We are likely to take the higher first break as the Ar3 of the specimen, because it is the beginning of the break that we look for. The more sensitive the method the higher will it detect such a slight break.

What is thus true when a local impoverishment in carbon is caused by surface decarburization is true when it is caused by segregation, whether solidificational or transformational. We usually look at segregation as the enrichment of one part, *e. g.*, in carbon; but the enrichment of one part must needs cause the impoverishment of another. Clearly the upper limit of A3, the highest temperature which our methods of observation detect, is likely to be the A3 of the part poorest in carbon, because A3 rises as the carbon content falls.

The dilatational method is affected in like manner by error from these sources.

The micrographic method has an advantage over others in that it may be applied to the undecarburized interior of the specimen, if this is large enough to have such an undecarburized interior, and in that it enables us to detect and reject specimens in which any considerable degree of solidificational segregation persists. For that matter the fitness of the specimens used in other methods should always be tested micrographically.

§ 33. *How Rapid is Surface Decarburization?*—It is very rapid in atmospheric air, rather rapid in an atmosphere chiefly of carbonic oxide and nitrogen, and not without effect even in a good vacuum. Thus heating in an air vacuum "usually better than 0.1 mm." at the Bureau of Standards for the purpose of taking heating and cooling curves, caused noticeable surface decarburization in the steels of 0.03, 0.21, 0.40, and 0.46 per cent. of carbon of Howe and Levy (Series IV., VI., and II.), though not of the steels of 0.16, 0.92, 1.14, and 1.45 per cent. of carbon. (See § 23.) In the case of the 0.40 per cent. carbon steel there was a "very thin skin entirely decarburized." Yet I found but slight suggestions of tarnish in this very surface. (See § 23.)

§ 34. *The Persistence of Solidificational Segregation* has received too little attention. The parts which it impoverishes in carbon behave like correspondingly low-carbon steel, whether in the thermal, the

dilatational, or the micrographic method, and thus tend to give too high positions for A3. Heyn [18] showed macroscopically how extraordinarily persistent this structure is, finding that an 8-day annealing at a red heat failed to wipe it out. N. T. Belaiew [19] has lately confirmed this persistency in a most striking manner, in particular showing that the initial dendritic structure of a casting was affected little if at all, to judge from his illustrations, by a series of heat treatments which included a sojourn of 24 hours at 1,160°.

Perhaps the most striking case of the persistence of solidificational segregation is Stead's.[20] A bar of 1.2 per cent. carbon steel was immersed in a 3-ton mass of blast-furnace slag, with which it cooled, of course extremely slowly. Though the highest temperature must have fallen far short of the liquidus, because the bar retained not only its shape but also the maker's stamp marks undulled, yet a drop, consisting in part of eutectic and as a whole containing more than 3 per cent. of carbon, liquated out and hung down. The liquating shows that the temperature must have risen above the solidus, because below it diffusion and equalization occur, and only above it can melting take place.

That part of the metal should become fluid enough to migrate in a bar which as a whole retained this degree of solidity shows that, in the heating and rolling operations in which the original bar was prepared, a large degree of local enrichment in carbon must have persisted, thus showing the slowness with which diffusion effaces segregation. For those particles which melted must have been so far enriched in carbon as to be above their own liquidus; and their being thus above their liquidus means that that liquidus had been lowered by their excess of carbon content, so as to bring it below the existing temperature, a temperature far below the liquidus of the bar as a whole, as is shown by the degree of solidity which it retained. Hence the evident great gap between the local liquidus of these residual spots rich in carbon and the liquidus of the bar as a whole, and hence the implied persistence of uneffaced initial local enrichment in carbon.

This slowness of diffusion is shown here in an additional way. Stead's illustration indicates an extremely sharp carbon gradient at the junction of the liquated drop and the body of the bar. The sharpness of this gradient indicates the feebleness with which diffusion effaces segregation even at this very high temperature close beneath

[18] *Mikroskopische Untersuchungen, Verhandlungen des Vereins zur Beförderung des Gewerbfleisses*, p. 359 (1904).

[19] *Revue de Métallurgie*, vol. ix., pp. 669, 671 (Sept., 1912).

[20] The Practical Use of the Iron-Carbon Equilibrium Diagram, *Proceedings of the North-East Coast Institution of Engineers and Shipbuilders*, vol. xxix. (Jan. 24, Feb. 14, 1913).

the eutectic melting point, 1,130°. For it is clear that in this extremely slow cooling the sojourn at temperatures but slightly below 1,130° must have been very long; and unless diffusion were very slow it would have flattened the carbon gradient at this junction as well as elsewhere.

That such segregation, though it is no doubt made less extreme by diffusion in long heating, may yet persist to a marked degree in gray cast-iron is shown by Figs. *C* and *D*, Row 14, Plate 5.[21] These represent the sodium picrate etching of a dendrite which persisted after five heatings to 800° each followed by a slow cooling, in a button made by melting Professor C. F. Burgess's electrolytic iron with sugar charcoal in a magnesia crucible brasqued with sugar charcoal. It contained combined carbon, 1.598; graphite, 1.356; total carbon, 2.954; silicon, 0.040; manganese, none; phosphorus, 0.050; sulphur, 0.035. After this sodium picrate etching the cementite and graphite present would be dark. The great contrast between the lightness of the dendrite trunks and branches and the darkness of the fillings between them shows that very marked solidificational segregation of carbon persisted even after these heatings.

I must admit that the persistence in the cases which I have quoted may have been due to the presence of phosphorus, because in only one of them is the phosphorus known, and in this it is high enough, 0.05 per cent., to exaggerate the persistence materially. In the Stead case the phosphorus was probably low, because round bars of steel of 1.20 per cent. carbon stamped with the maker's name usually contain not more than 0.03 per cent. of phosphorus at most.

We reasonably expect the effect of solidificational segregation to be most serious in unforged castings, because the drawing out of the metal in forging or rolling lessens proportionally the distance to be covered by diffusion, and probably for other reasons, such as the structural instability which the mechanical distortion causes. The highest two series, those of Heyn and of Goerens and Meyer, 2 and 3 in Fig. 14, are unforged castings. The high position of Series 4 is referable to its being an Ac series.

It is true that a comparison of the heating and cooling curves of certain unforged castings of nickel steel with the curves of the same material after forging gave Carpenter, Hadfield, and Longmuir[22] no indication of such segregation, at least none that can be recognized

[21] See the Author's Fig. 9, *Trans.*, xliv, p. 469 (1912). It represents one of the dendrites in this same specimen.

[22] Seventh Report, Alloys Research Committee, *Proceedings of the Institution of Mechanical Engineers*, 1905, p. 927.

on the scale of their published plottings. Whether this is referable to the influence of nickel in restraining segregation, to an effective diffusing annealing, or to what other cause I do not know. In spite of this evidence I believe that uneffaced solidificational segregation is likely to raise the observed position of A3 materially, in view of the facts, (1) that the first deposited layers in iron-carbon alloys are so much poorer in carbon than the average of the specimen; (2) that etching indicates that this local impoverishment may be effaced only very slowly; (3) that the shortage of ferrite found by Heyn and by Howe and Levy itself points towards the peristence of such impoverishment; (4) that if such impoverishment persisted it would inevitably lead to a local elevation of A3.

§ 35. *The Persistence of Transformational Segregation.*—The data of Goerens and Meyer[23] and of Jung[24] have been interpreted as proving that this segregation is effaced rapidly; but however well they may show that the carbon gradient is rapidly flattened very materially, they are hardly competent to show how rapidly that flattening out is completed.[25] The persistent reports that after grain refining the position of the old ferrite grain boundaries may be disclosed by again heating and cooling slowly, argue that a considerable degree of transformational segregation may long persist, the impoverishment of the old coarse grain boundaries, which the grain refining aimed to efface, remaining to a degree sufficient to induce a more abundant precipitation of ferrite there in the subsequent slow cooling. If, as is believed, this persistence of the old grain boundaries is greater in phosphoric than in purer steels, that points to its being due to per-

[23] *Metallurgie*, vol. vii., p. 310 (1910).

[24] *Internationale Zeitschrift für Metallographie*, vol. i., p. 215 (1911).

[25] Goerens and Meyer found that, when hypo-eutectoid steel was cooled to 840°, within the transformation range for their steel, held there, and then quenched, the quantity of ferrite recognizable increased on prolonging the holding from 5 to 15 min., but not on further holding. But very careful and extended planimetric measurements would be needed to prove that this further holding did not actually increase the quantity of ferrite detectable. Indeed Howe and Levy found a very great increase in detectable ferrite on prolonging their holding at 750° from 36 to 108 min., so that 36 min. is clearly far from long enough to complete the structural equilibrium at 750° (*Internationale Zeitschrift für Metallographie*, vol. iii., p. 4 (1912)).

Jung found that some pro-eutectoid cementite in his steel of 1.33 per cent. of carbon precipitated on cooling to 950°, so that the line *SE* cuts this carbon content above 950°. He further found that, when this same steel was heated up to 975° and held there, the pro-eutectoid cementite initially present failed to redissolve completely in 3 min., but dissolved so far as to be unrecognizable in 4.5 min. This certainly implies a pretty rapid flattening of the carbon gradient at 975°; yet it does not show that the completion of this flattening is rapid even at this temperature. To be more specific, it implies that, with this high carbon content, the transformational segregation residual at the end of 4.5 min. is not enough to raise the observed position of *SE* by 25°.

sistence of transformational segregation, because it is known from other sources that the diffusion of phosphorus is slow. Again, the great persistence of solidificational segregation makes the persistence of transformational segregation antecedently probable.

The dark lines in Fig. D, Row 8, Plate 3 (Steel E), argue for the persistence of this transformational segregation. This specimen, after coarsening, was heated apparently to above the end of Ac3, held there for 30 min., and quenched in water. The dark lines evidently represent the old coarse grain boundaries, such as show white in specimens quenched from below the end of Ac3. Their being dark can be interpreted as meaning that the transformation has proceeded further here than elsewhere in the direction of troostite, on the ground that the further the transformation goes the darker is the etching, and this further transformation may be interpreted as meaning that the carbon content here is lower than elsewhere, on the ground that the lower the carbon content the faster is the transformation; and this in fine as meaning that the transformational impoverishment in carbon here has not been completely effaced. But on the other hand the darkening is capable of being interpreted as meaning that there is still in these old boundaries a quantity of unreabsorbed ferrite which, though too small to be detected microscopically, is yet sufficient to intensify the etching by difference of potential.

The darkening clearly represents the persistence of some old transformational segregation, and not the grain boundaries of the austenite formed in the present heating, because these grains must needs be very fine. Since the time when the austenite was coarsened so as to give rise to the coarse boundaries the loci of which now show dark, the metal has cooled below the transformation range, thus destroying that austenite, and has been heated up through that range again, giving rise to a new and finer set of grains, the boundaries of which are not here shown. The dark lines are the persisting segregations caused by a grain system which itself has ceased to exist.

Moreover, the shortage, inferred in § 37, of recognizable ferrite in the micrographs of Howe and Levy argues for considerable segregation in their specimens. The fact that their specimens are rolled bars indicates that there has been good opportunity here for the effacement of solidificational segregation. This raises a probability that the segregation which those specimens are inferred to have is transformational at least in part, and that it has persisted through their 30 or even 60 min. holding at or above A3.

§ 36. *Specific Influence of the Persistence of Segregation whether Solidificational or Transformational.*—I will now explain a specific effect

which this segregation must needs have, an effect which is prominent in the data of Heyn and of Howe and Levy. To fix our ideas let us assume that such segregation actually persists, and that it has left the spots *LLL* in Fig. 13 impoverished in carbon, and the spot *H* enriched in carbon, with a carbon gradient of unknown shape from *L* to *H*. Let this figure refer to a steel of 0.40 per cent. of carbon,

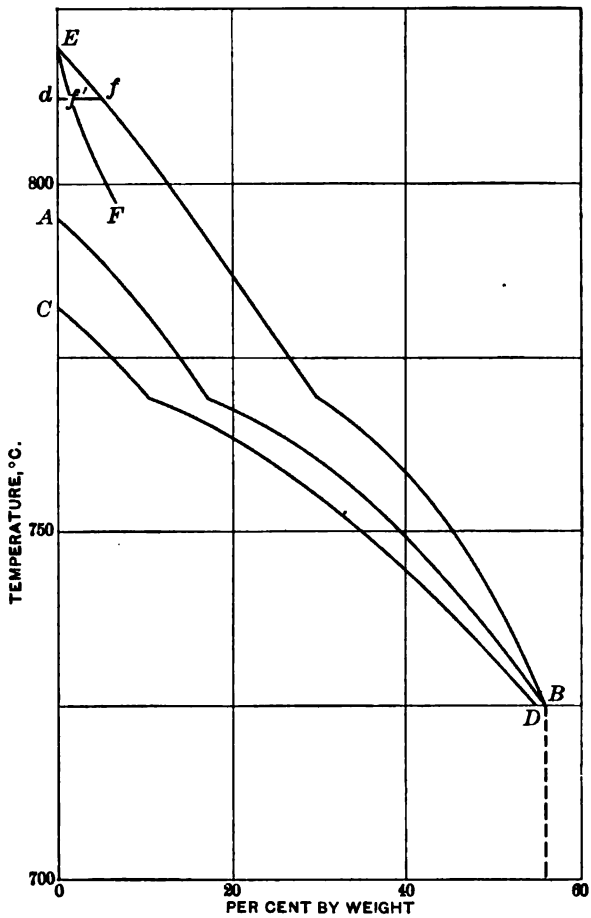


FIG. 12.—CALCULATED QUANTITY OF PRO-EUTECTOID FERRITE PRECIPITATED: *AB* by a homogeneous steel of 0.40 per cent. of carbon. *EF* by such a steel if segregated.

and let us consider the precipitation in falling temperature. That which is true of precipitation is true also of reabsorption, *mutatis mutandis*.

The curve *AB* in Fig. 12 represents the percentage of pro-eutectoid ferrite which should be present in such a steel at various temperatures

in the transformation range, on the assumption that the line *AAA* of Fig. 14 is the true Ae3.

The precipitation of ferrite at given temperature at each spot in a segregated steel such as represented in Fig. 13 corresponds, not to the average carbon content of the whole specimen, but to the carbon content of that spot. Because the spots *LLL* have been impoverished in carbon, the precipitation of ferrite in cooling will begin, not at the Ae3 of the specimen as a whole, but at the higher Ae3 of these particular spots. But as we naturally take the temperature at which this first precipitation is observed to be Ae3 for the carbon content of the specimen as a whole, we incorrectly put A3 for this 0.40 carbon steel at *E*, Fig. 12, above *A* where it truly belongs. If our whole series of specimens with which we are determining the locus of A3 is segregated in like manner, we shall set this locus throughout correspondingly too high. Calculating from this locus the quantity of ferrite due at different points within the transformation range of our 0.40 carbon steel, we shall get a false curve *EB* instead of the true curve *AB*.

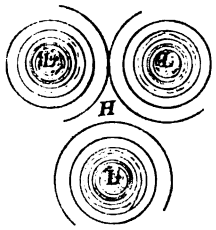


FIG. 13.—PRECIPITATION OF FERRITE ABOUT SPOTS IMPOVERISHED IN CARBON SEGREGATION.

But the quantity of ferrite actually precipitated will follow a curve like *EF*. Thus on cooling to *d* the whole of the specimen would precipitate a quantity of ferrite *df* if *E* were truly the Ae3 for 0.40 per cent. of carbon, and if the false high position found for the locus of Ae3 were the true locus. But in fact it is only the very spots *LLL* that will precipitate ferrite in that proportion, *df*. At temperature *d* the shore layers about *LLL* will precipitate less and less as their distance from *LLL* increases, till at a very short distance from *LLL* the carbon content is so much greater than that of *LLL* that no ferrite at all will precipitate at temperature *d*. Thus there will be a marked shortage of precipitated ferrite in the specimen as a whole below that calculated from the observed position of the curve A3. The ferrite actually precipitated will be *df'* instead of *df*.

As far as this specific cause of error goes the gap between *EF* and *AB* should decrease progressively, but *EF* should never cross *AB*, for there is nothing here to make the quantity of precipitated ferrite less than that due according to the true Ae3.

§ 37. *Shortage of Recognizable Ferrite.*—The reasoning in § 36 enables us to interpret the shortage of recognizable ferrite in specimens quenched within the transformation range, observed by Heyn and by Howe and Levy, a shortage which means that, as the temperature descends below that at which the first precipitation of ferrite is observed, the increase in the quantity of ferrite recognizable, instead of following a curve of the family of *AB* and *CD*, follows one like *EF*. From § 36 we may take this to mean that there is marked heterogeneousness in both sets of specimens, and the existence of this heterogeneousness would tend to raise the observed locus of Ae3 above the true position of Ae3.

Diffusion probably flattens down pretty rapidly the extreme sharpness of the carbon gradient which the expulsion of ferrite into the austenite grain boundaries causes in cooling through the transformation range; but the further flattening of that gradient is a self-retarding process. Diffusion completes itself only asymptotically. In fact a gradient steep enough to raise the observed position of A3 measurably is probably very persistent, because Howe and Levy found that the quantity of unreabsorbed ferrite decreased only extremely slightly on prolonging the holding period at their quenching temperatures from 30 to 60 min. (§ 14.)

In interpreting this shortage of ferrite we should remember how deceptive these proportions often are. On inspecting pearlite one would often infer that the proportions of ferrite to cementite in it were about equal instead of being 6 : 1. Mr. Levy and I have pointed out how greatly the apparent proportion of pro-eutectic cementite may exceed the true proportion, especially on low magnification.[26] And for that matter the quantity of recognizable ferrite even in our original coarsened specimens, Figs. *A* of Rows 4 to 9, Plates 2 and 3, seems on inspection much less than that which is due theoretically.

§ 38. *The Precipitation of "Quenching Ferrite" or Cementite, i. e.*, of the pro-eutectoid element during the quenching proper, tends to raise the observed A3 above Ae3. If, equilibrium being reached at Ae3, there is no free pro-eutectoid element present before quenching, yet because the incipiency of the cooling cannot be extremely rapid in the interior of the specimen, it may result that some ferrite or cemen-

[26] The Life History of Pro-Eutectic Cementite, *Proceedings of the International Association for Testing Materials*, VI. Congress, II., 1912, and the discussion.

tite precipitates. Inferring that this was present at the quenching temperature itself we should infer that this temperature was below Ae3. To raise the quenching temperature somewhat higher would be to induce a more rapid cooling from Ae3 down, and thus perhaps to prevent detectable precipitation of pro-eutectoid element. We should thus come to take such higher temperature for Ae3. In short this cause of error tends to raise the observed position of A3 to above Ae3. But it is unlikely to cause any important error except in the case of very low carbon steel, with less than say 0.10 per cent. of carbon, in view of the extreme sluggishness with which Mr. Levy and I found that ferrite coalesces into ready visibility in steel of 0.40 per cent. of carbon.[27] The coalescence of pro-eutectoid cementite into visibility seems to be still slower.

§ 39. *Failure to Detect the First (or Last) Traces of the Pro-Eutectoid Element.*—We might well fail to recognize the earliest precipitated ferrite because of the tardiness with which it coalesces into recognizable masses.

This same slowness of coalescence may in another way prevent our recognizing the beginning of precipitation and the end of reabsorption in specimens quenched even after a long holding at constant temperature, as in the experiments of Howe and Levy. For even at such nominally constant temperatures there are slight unnoticed oscillations of temperature, and moreover the solution pressure of the ferrite is different towards different austenite masses present. Both these things should lead to a constant slow passage of ferrite into solution and its reprecipitation in the form of an emulsion, and because this freshly precipitated ferrite coalesces so slowly into recognizable masses, there may be at all times a certain quantity of unrecognizable ferrite, a quantity which would be the smaller the less the oscillations of temperature. The temperature at which we should recognize the first or last traces of ferrite would be that at which the quantity of ferrite actually present was in excess of that which thus long remains emulsified, that excess having coalesced into recognizable masses, a temperature evidently below the true Ae3.

The effect of this failure would be to make the observed quantity of ferrite lie in a curve like *CD*, Fig. 12, below *AB*, but approaching it gradually as the temperature falls, because the larger the quantity of ferrite present the more thoroughly should it coalesce, somewhat as the quantity of unconsumed carbon left in the ash of coal decreases

[27] Belated Coalescence vs. Balling Up as the Cause of the Degradation of the Network Structure of Hypo-Eutectoid Steel, *Internationale Zeitschrift für Metallographie*, vol. iii., p. 1 (1912).

from specimen to specimen as the quantity of ash decreases, or as a rich milk churns into butter faster than a poor one.

A consideration of the ideas set forth in Figs. 12 and 13 inclines us to think that this cause is not likely to lower the observed position of A3, as observed micrographically, to materially below Ae3. I indeed admit that if the precipitation (to fix our idea) of ferrite follows a curve like *EF*, the rate at which the precipitated ferrite increases may be so slow that a considerable gap may exist between the higher temperature at which ferrite begins locally to precipitate, and the lower temperature at which its precipitation reaches such a quantity that some of it becomes recognizable; so that failure to detect the ferrite actually precipitated might lower the observed position of A3 considerably below *E* of Fig. 12. But in doing this it is merely correcting another error, that due to local impoverishment in carbon; it is only lowering the observed temperature from the falsely high point *F* towards the true point *A*. The important thing to recognize is that, ignoring lag, the shape of *AB* shows that the increase in the quantity of ferrite due to precipitate is so extremely rapid as the temperature sinks below this true point *A*, that before the temperature sinks materially below *A* the precipitation of ferrite should be so abundant that we hardly expect it to escape detection.

Part III. Discussion of the Chief Data as to A3.

§ 40. *In General.*—The relations between the chief data are shown in a general way by plotting them in Fig. 14, after p. 648, but perhaps better by assembling them in Table IX., in which they are numbered from 1 (Osmond's thermal end of Ac3) to 13 (Burgess and Crowe's thermal Ar3 for high-manganese steels), with the intermediate ones in their general temperature order, the order which fits them best for comparison, in spite of the fact that in some cases it is locally reversed by their crossing each other.

Fig. 3, § 11, shows the micrographic results of Howe and Levy and the thermal upper limit of A3 of Burgess and Crowe, for the high-manganese steels, Series 13, together with the corresponding lines for the lower-manganese steels of the same observers reproduced for comparison from Fig. 1.

§ 41. Corrections.—In drawing curves of Fig. 14, I have taken certain liberties which seem necessary in the search for truth. First, I have rejected certain thermal beginnings of Ar3 and endings of Ac3, on the ground that neither where placed by their observers nor elsewhere in these cooling or heating curves is there any inflection so marked as to carry appreciable weight when in conflict with strong

evidence. The points thus rejected are given in Fig. 14, but inclosed in circles to indicate that they are neglected.

Second, I have lowered summarily some of these beginnings and endings from the position assigned by their observers, thus lessening the discrepancies. In Series 1 and 5 there is reason to fear important surface decarburization, and in Series 3 and 7 there is reason to fear that the combined effects of such decarburization as there was, and of uneffaced solidificational segregation due to the unforged cast state of the specimens, are considerable. Here we may assume reasonably that the last faint tailing out of Ac3 and the first creeping in of Ar3 represent these parts, thus impoverished in carbon. For the purpose of our present comparison we may neglect these extreme ends, and take that part of the heating or cooling curve in which the inflection is strong enough to indicate that it represents a considerable fraction of the whole specimen. In this way allowance may be made for surface decarburization where it is not extreme, but hardly for solidificational segregation in unforged castings. The first deposited layers have about half the carbon content of the specimen as a whole, and such diffusion as occurs in the cooling of a small casting from the liquidus down is likely to leave a very large fraction of the whole materially impoverished in carbon, and hence with a materially higher A3 than that of the specimen as a whole.

Series 11 hardly needs such a correction, both because of the shape of its curves and because segregation and decarburization were probably very slight in it. It would be hard to apply such a correction to the micrographic results, Series 2 and 6, without very careful planimetric determination of the rate of increase of the ferrite present; and indeed in Series 6 the effects of decarburization are removed by ignoring the outside of the specimens. To the dilatational results no such correction can be made, because one only of the observed curves is accessible. But corrections have been applied to certain of the data in Series 1, 3, 5, and 7 as explained under Fig. 14.

§ 42. *What are the Discrepancies?*—Lag must necessarily raise the Ac3 results above and lower the Ar3 results below Ae3. Were lag the only cause of discrepancy, then the temperature-order should be,

1st and highest, the thermal Ac3 results in the order of the rapidity of heating; Series 1 and 4;[28]

2d, the electric resistance Ac3, Series 8, in reaching which the

[28] Lest the reader be confused by assertions lately made that there is no lag in transformations in rising temperature, let me point out that in these assertions the word "transformation" is used in the sense of "fusion," and not in the usual sense of "a transformation within the solidified metal," as in the present case.

temperature, though indeed rising, rose so slowly that this series should come close to the stationary-temperature ones;

3d, the stationary temperature results, Series 2, 6, and 12;

4th, the dilatational Ar3 results, Series 9, in reaching which the temperature probably fell decidedly more slowly than in the thermal Ar3 series;

5th, the thermal Ar3 results, inversely in the order of rapidity of cooling, the slowest highest and the fastest lowest, Series 7, 11, 3, (10), 5;

or to sum up, 1, 4, 8, 2, 6, 12, 9, 7, 11, 3 (10), 5.

With a reasonable allowance for capriciousness of lag, the conditions of which are so little known, the discrepancies between Series 1, 4, 5, 6, 7, 8, 9, 10, and 11 are not greater, indeed they are less, than should be expected from observational errors. Thus the stationary temperature, series 6, lies as it should below the thermal Ac3 series 1 and 4, which two moreover lie in their expected order; and it lies above the electric resistance and dilatational series 8 and 9, and these in turn lie above the thermal Ar3 series 10 and 11. That series 6, which ought to be above the thermal Ar3 series 5, is 20° below one and 13° below another of its points is hardly surprising in view of the serious opportunity for decarburization in series 5, of the irregularity of its results, and of probable error in correcting Series 5 for error in the temperatures then used for calibration. The one point of Series 8 which deserves weight is so ill defined in the original curve that the surprise is rather that it fits in so well with the other series than that it happens to lie a few degrees on the wrong side of the thermal Series 7. That some points of this latter series lie above instead of below the probably slower cooled Series 9, the distance on the wrong side reaching in only one case as much as 16°, is not surprising in view (1) of the cast state of Series 7, (2) of the extreme sensitiveness of the observational method of Series 7, both of which would tend to raise the observed beginning of Ar3, (3) of the possibility that a calibrational error has lowered the observed temperatures of Series 9.[29]

[29] Charpy and Grenet, *Bulletin de la Société d'Encouragement pour l'Industrie Nationale*, 1903, I., p. 475, calibrated their couple for the boiling point of sulphur, 448°, and for the melting point of gold, 1,060°, and assumed that between these temperatures the deflections of the galvanometer were proportional to the temperature. This is not strictly true for most couples. An examination of the calibration of five Pt, Pt–Ir–couples in use in the metallurgical laboratories of Columbia University shows that this assumption that the curve is straight between 445° and 1,063° would introduce an error of about 10° in two cases, of about 15° in two, and none in the fifth. The temperatures which they assign to their two fixed points are very close to those now given by the United States Bureau of Standards, 444.7° and 1,063 ± 3°.

Thus nine out of the twelve series are on the whole surprisingly concordant, in view of the many sources of error. There remain three important Series, 2, 3, and 12, which are less closely in harmony with the others. Indeed 2 and 12 for part of their length differ widely from the rest.

Series 2, which, because it is a stationary temperature series, ought to lie near to Ae3, below the Ac3 and above the Ar3 series, is for most of its length far above all other series. Series 3, which, because it is a thermal Ar3 series with rather rapid cooling, ought to lie well below the thermal Ac3 series, No. 4, and the stationary temperature series 6 and 12, lies much farther above some of them even as it is corrected than should be explicable by any errors in temperature measurement. Series 12, which, because it is a stationary temperature method, should lie above all the thermal Ar3 series, 3, 5, 7, and 11, lies for much of its length below 3, 5, and 7. That Series 12 is for most of its length above Series 10 deserves little weight, because Series 10 represents maxima of Ar3 which are an indeterminate distance below the beginning of Ar3.

§ 43. *The Discrepancies in Detail.*—Let us address ourselves to these three discrepancies.

Series 2.—Its unduly high position is at once explained by the fact that its specimens are both in the cast state and rich in phosphorus, a combination of conditions which, as shown in § 21, raises A3 greatly. Attention is called to the fact that this raising effect of phosphorus is greatest midway of the hypo-eutectoid range, apparently shading off to a small quantity at the ends of this range. This agrees with Osmond's observation that in a very low-carbon iron (carbon 0.05 per cent.) the presence of even 0.38 per cent. of phosphorus did not raise the maximum of Ar3 at all. I indeed find in his curves that this phosphorus content raised the beginning of Ar3 by about 10°, but this is only a small fraction of the raising effect of the smaller quantity of phosphorus in Series 2, midway of the hypo-eutectoid range. The greatest depression of Series 12 below the mass of the other Series also lies midway of this same range, decreasing rapidly towards either end, a depression which may be due in part to the pretty high manganese content of the specimens. But this is probably only a coincidence, for no such relation between the depressing effect of manganese and the carbon content can be traced among the high-manganese steels represented in Fig. 3.

§ 44. *Series 3*, which because it is an Ar3 series with rather rapid fall of temperature should lie below the stationary temperature Series 6, in fact lies nearly 20° above it. Because its rate of cooling is

greater than that of the Ar3 series 7 and 11, it should lie below them, yet in fact it lies from 25° to 55° above the former and from 45° to 55° above the latter, confining ourselves for the moment to the left of the break or point *O*. Its being above Series 6 and 11 may be referred to its uneffaced solidificational segregation, and its being above Series 7 to its being raised more than Series 7 by that segregation. Such data as are given suggest that in Series 3 the sojourn above A3 was not long enough to efface transformational segregation, which thus may have helped to raise the temperatures of Series 3. The correction already introduced probably suffices to cover surface decarburization, but not segregation.

§ 45. *Series 12*.—The low position of Series 12 I cannot explain. In these experiments, which were *in vacuo* throughout, the specimen was heated to the predetermined temperature, held there for between 10 and 20 min., and then dropped into an ice calorimeter, by means of which the heat evolved in the further slow cooling to 0° was determined. Thanks to the vacuum this cooling in the calorimeter was slow enough to yield lamellar and even granular pearlite. Plotting this heat as abscissa, and the temperature of entry into the calorimeter as ordinate, gives concordant curves in most of which Ac1 and the end of Ac3 are indicated clearly by sharp breaks, the total heats above A3 falling in a straight line, and those between A3 and A1 in another straight line.[a] The point at which these lines if produced would intersect is indicated for each specimen by a four-sided star in Fig. 14, while the lowest observed temperature thus found to be above A3 and the highest thus found to be below A3 are represented by the ends of the vertical lines drawn there for the several specimens.

At first one tries to explain the low position of Ar3 thus found by the rather large manganese content of the specimens. But we have seen (§ 29) that the effect of manganese in lowering the transformation range is chiefly through increasing lag, and but little through lowering the Ae points. Hence in Series 12, with its long stay at stationary temperature and its consequent lessening of lag, manganese should have had the relatively small effect which it has in Series 13. But actually its effect is much greater, as is shown by considering specimens 3 of Series 12, and B and II of Series 13:

	C.	Si.	Mn.	P.	S.	A3.	Ac3. Ar3. Of Burgess and Crowe.
3. Series 12...	0.32	0.075	0.45	0.054	0.057	750–770	.....
B. Series 13...	0.32	0.122	0.41	0.004	0.022	810–820	845 777 from their line.
II. Series 13..	0.46	0.094	1.215	0.017	0.015	765–775	785 725 from their points.

[a] When A2 is separated from A3, there is a break in this second line.

3 of Series 12 and B of Series 13 are practically identical in composition, yet the A3 for 3 of Series 12 is not only some 50° below that of B of Series 13 but slightly below the bottom of the gap between the Ac3 and Ar3 for this same specimen as found by Burgess and Crowe, though Ae3 should lie above the middle of this gap, so that it is far below the probable temperature of Ae3 which the data of Burgess and Crowe imply. Indeed its 0.45 manganese seems to have had a lowering effect much greater than that of the thrice as great manganese content of II of Series 13, to judge from the probable position of Ae3 in this latter specimen as indicated by the micrographic data of Howe and Levy and the thermal data of Burgess and Crowe.

The manganese content of 3 of Series 12 cannot explain the very wide gap, 75°, between its A3 and the Ar3 of Series 3, or the wide gap between it and the Ar3s of Series 5 and 7, without assigning to manganese an effect in lowering Ae3 far beyond what the other available data imply.

A systematic temperature error in Series 12 does not explain the discrepancy, because for the low-carbon specimen of this series the position of A3 agrees with that of other observers. Moreover though the temperatures of Meuthen's A1s suggest that his temperatures in general are slightly too low, yet it is by an amount considerably smaller than that by which his A3s lie below those of Series 3 and 6.

§ 46. *A Weighted Average* is calculated for what it is worth, assigning to the various series the weights indicated in column 11 of Table IX. These weights are based on the principles

(1) of assigning a maximum weight of 12;

(2) of deducting 2 for the cast as distinguished from the forged state; and

(3) of deducting 2 for data based on heating or cooling curves instead of stationary temperature.

Beyond this I deduct arbitrarily as follows:

1 from Series 5, because the fixed temperatures on which its calibrations were based are not those of to-day. Though I have corrected these temperatures in accordance with Osmond's own indications, yet even as thus corrected a certain residual error is strongly to be feared.[30]

3 from Series 6, because of probable error due to inability to detect the last traces of unabsorbed ferrite, and to uneffaced transformational segregation, the opportunity for which is greater here than in

[30] *Journal of the Iron and Steel Institute*, vol. lxxi., 1906, No. iii., p. 451, foot-note.

the other series. A smaller deduction might be justified in view of the fact that these two errors are of opposite sign.

2 from Series 9, because with one exception the original curves are not accessible.

Because of the small number of specimens represented at the left of the break, point *O*, I deduct 1 from Series 2, 4, 5, 11, and 12, and 0.5 from Series 9.

Because of impurity of the specimens I deduct 6 from Series 2, 1 from Series 4, 5, 6 and 11, and 2 from Series 12.

Because of temperature error indicated by the low position of A1, 0.5 from Series 12.

I do not weight Series 1 because it is confined to so very short a range of carbon content, and also because of Osmond's calibration conditions just noted; nor Series 8 (*a*) for the former of these same reasons, (*b*) because the number of points is so small, and (*c*) because I cannot identify the points with any confidence in the original curves; nor Series 10 because it represents maxima of Ar3, which lie such an indeterminate distance below Ar3, itself an indeterminate distance below Ae3.

§ 47. *GO and OS.*—Series 9 runs only to carbon 0.24, so that, were we to use its weight as far as this carbon content and then omit it, we should there cause a purely fictitious break. A like difficulty occurs in regard to Series 4, 5, and 11; how may we meet it? Let Series 9 serve as an example.

It is extremely probable that Ae1-2-3 lies very near 725° and between 0.85 and 0.90 per cent. of carbon. But the direction of the upper part of all the curves of Fig. 14, from 2 to 8 inclusive, would bring them to 725° far at the left of carbon 0.85, and perhaps even at the left of carbon 0.70. Because it is in the highest degree improbable that Ae1-2-3 lies so far to the left, it is probable that Ae3 in descending turns to the right, and all the curves of Fig. 14 which bear on this question indicate such a turning, 3, 6, and 7 indicating that it occurs between 765° and 790°, where Roberts-Austen's line for the maximum of Ar3 also shows such a break, while Series 5 puts it only slightly higher, at about 810°. This turning may well be progressive; but the present crude state of our knowledge is fitted better by representing it as a break between two straight lines, as has been done by most writers. The temperature of this break may provisionally be put at about 770°, or at about the maximum of A2, the rapid magnetic changes at which would naturally correspond to such a break. The part of Ae3 above this we may call *GOe*, and the part below this *OSe*.

Producing Series 9 to 770°, I use its weight in calculating the line *GOe* but not *OSe*. So *mutatis mutandis* with Series 4, 5, and 11. Indeed the data for *OSe* are so much scantier than those for *GOe*, and in my opinion carry so much less weight than the belief that *Se* is at 725° and carbon about 0.90, that we may provisionally adopt this position, and draw *OSe* as a straight line from it to where *GOe* as found by our weighting cuts the abscissa of 770°.

§ 48. *The Result.*—The broad black line *AAA* in Fig. 14 gives the position of Ae3 thus calculated.

This line should be received with great caution and only as a temporary expedient, because of the necessary arbitrariness in calculating it, and because the thermal data are such a poor foundation. Indeed an average calculated even from an infinite number of absolutely accurate thermal determinations would be untrustworthy without much more than our present knowledge of the influence of the rate of heating and cooling on the lag. Some may question the wisdom of presenting this calculated line, holding it better to trust that what I have said of the contradictoriness of our data, of the causes of error, and of the industrial importance of Ae3, would induce some national laboratory to determine this line with these causes of error reduced to a minimum. The close agreement between *AAA* and the line *HLM* of Howe and Levy is evidently in large part fortuitous, and was not foreseen when the weightings were assigned to the several series of data.

§ 49. *The Calculated Line Tested Against Late Determinations of A3 in Very Pure Iron.*—Table VIII. condenses the position of A3 for extremely pure iron found in three late investigations. If the line *AAA* calculated for Ae3, shown in Fig. 14, is correct, we naturally expect its prolongation to cut the vertical axis at a point which agrees with these late data for A3 of carbonless iron. Both it and the line *HLM* bear this test better than Heyn's line, which till now has, I believe, seemed the most trustworthy. *AAA* cuts the axis at about 919°, and *HLM* at 912°, temperatures compatible with all the data in Table VIII., because they are below all the Ac3 ends and above all the Ar3 beginnings.

Heyn's line even as corrected cuts the axis at about 931°, and it therefore has the disadvantage of being above three of the Ac3 ends, which it could not be if those ends are correctly observed. Again, because Heyn's line is an Ar3 beginning it should be tested against the Ar3 beginnings of Table VIII.; but its intersection with the axis is from 27° to 41° above those beginnings, a fact which tends to support my suspicion that his results are raised above the Ae3 by

TABLE VIII.—*Late Determinations of A3 in Nearly Chemically Pure Iron.*

No.	Observer.	Composition. C.	Si.	Mn.	P.	S.	Condition.	Rate of Heating Seconds per 1 °C.	End of Ac3. °C.	Beginning of Ar3. °C.	Gap. °C.	
1	G. K. Burgess.....	Very pure from Prof.	electrolytic iron C. F. Burgess.					3.9	955	.....	51	
2	G. K. Burgess.....							4.3	950	.....	46	
3	G. K. Burgess.....							9	935	.....	31	
4	G. K. Burgess.....							3.2–4.4	.....	904	.....	Mean of 4 series.
5	Guillet and Portevin	Unweighable	Nil	Nil	0.025	Nil	As deposited	.....	937	902	35	As = 0.011.
6	Guillet and Portevin						After heating to 900° for 2 hours	.....	932	902	30	
7	Carpenter.....	0.008	0.014	0.009	0.002	Trace	Specimen 1, 4th heating	2 to 9	948	894	54	
8	Carpenter.....						Specimen 1, 5th heating	2 to 9	948	894	54	
9	Carpenter.....						Specimen 1, 6th heating	2 to 9	952	894	58	
10	Carpenter.....						Specimen 2, 4th heating	2 to 9	926	890	36	
11	Carpenter.....						Specimen 2, 5th heating	2 to 9	930	890	40	
12	Carpenter.....						Specimen 2, 6th heating	2 to 9	926	890	36	

NOTE TO TABLE VIII.—**1** to **4**. G. K. Burgess, private communication, August 10, 1911.

5 to **6**. Guillet and Portevin, *Comptes Rendus*, 1913, vol. clvi., p. 702. A Saladin-Le Chatelier apparatus was used.

7 to **12**. Carpenter, Advance Proof, *Journal Iron and Steel Institute*, May, 1913. The rate of heating varied from 2 sec. per degree at the lowest temperatures to 9 sec. per degree at the highest temperatures, and the rate of cooling from 3 sec at the highest temperatures to 8 sec. at the lowest temperatures.

The specimens were cathode sheets of extremely pure electrolytic iron, heating and cooling curves of which were taken repeatedly *in vacuo*. The results reached in the first three heatings of each specimen are not included here, because they were much affected by the hydrogen occluded in the iron as deposited.

[This table as it appeared in the *Bulletin* for June, 1913, had certain errors which were pointed out in the discussion of Prof. H. C. H. Carpenter. These have been corrected in the table as it stands above.]

uneffaced solidificational segregation of carbon. But if the strong inflection which Heyn himself puts in the upper part of his line is preserved, this objection to it falls to the ground.

The Ac3 determinations of Table VIII. agree in a general way with the late results of Broniewski,[31] who found a break in the electric resistance curve of "pure iron" at 950°. His dilatation results are less easily interpreted, showing as they do a maximum at about 950°, then a sharp contraction to about 975°, and then a re-expansion at about the previous rate.

§ 50. *Summary.*—1. Industrial A3 is probably lower than the end of Ac3, but above the beginning of Ar3 of the thermal method. (§ 27, p. 613.)

2. Experiments are needed to show whether the best grain-refining temperature is Ae3 or some temperature a little above or a little below it. (§ 28, p. 613.)

3. Manganese lowers the thermal Ar1 and Ar3, at a rate which for Ar1 is in most cases between 24 and 50° per 1 per cent. of manganese. (§ 29, p. 614.)

4. Its effect on Ac1 and Ac3 varies in sign from case to case, occasionally lowering them but often raising them slightly. (§ 29, pp. 617 to 619.)

5. From 4 it is inferred that manganese probably lowers Ae1 and Ae3, but by a degree so small as often to be masked by its increasing the lag. (§ 29, p. 619.)

6. The present evidence is insufficient to show whether manganese lessens the eutectoid carbon content. (§ 29, p. 619.)

7. An explanation of the effects of phosphorus on the structure is attempted. (§ 30, p. 621.)

8. Lag tends to bring the observed Ar points below and the observed Ac points above the Ae points. (§ 31, p. 624.)

9. Local lowering of the carbon content through surface decarburization and uneffaced segregation, whether solidificational or transformational, may raise the observed Ac3 and Ar3 points. (§ 32, p. 627.)

10. Evidence is given tending to show that the raising effect of this uneffaced segregation may be material. (§§ 34, 35, pp. 628 and 631.)

11. The observed shortage of recognizable ferrite in specimens quenched within the transformation range is interpreted as indicating that the observed A3 is above Ae3 in these cases. (§§ 36, 37, pp. 632 and 635.)

[31] *Comptes Rendus*, 1913, vol. 156, p. 699.

12. The precipitation of ferrite or cementite during the quenching itself is unlikely to cause important error in determining the temperature of A3 micrographically. (§ 38, p. 635.)

13. Failure to recognize the first or last traces of precipitated or unreabsorbed ferrite may bring the position of Ac3 and Ar3 observed microscopically below Ae3. (§39, p. 636.)

14. The chief data touching A3 are discussed. All but three series are found reasonably consistent. (§§ 40 to 45, pp. 637 to 644.)

15. A weighted average of the more important determinations gives the formulæ:

$T° = 917° - 306 \times C$ for *GO*, and

$T° = 820° - 105.5 \times C$ for *OS*.

Here C = percentage of carbon, and

T° = temperature in degrees Centigrade, for Ae3.

This line agrees fairly with the late determinations of A3 in nearly chemically pure iron. It implies the following points:

	Carbon.	Temperature.
Point *G*	0	917°
Point *O*	0.483	769°
Point *S*	0.9	725°

NOTE TO FIG. 14.—As explained in § 41, certain points in the line A3 of certain observers have been omitted, and certain other have been lowered, as indicated in the following table:

Observer.	Points Omitted. Carbon Content.	Points Lowered. Carbon Content.	Points Raised. Carbon Content.
Series 1. Osmond, Ac3	0.16	.....	
Series 3. Heyn, Ar3	0.75	0.08, 0.20, 0.39, 0.50	
Series 5. Osmond, Ar3	.....	.....	0.08
Series 7. Carpenter and Keeling, Ar3.	0.12	0.01, 0.02, 0.05, 0.16, 0.24 0.38, 0.47, 0.53	
Series 10. Roberts-Austen, Ar3	0.22	.....	

Moreover, though Carpenter and Keeling do not give any temperature as the beginning of Ar3 for their steels of 0.61 and 0.81 per cent. of carbon, I have ventured to select a point in the cooling curve of each of these steels at which the inflection seems to me so strong as to form valid evidence that Ae3 lies at least as high as this. It is some justification that these points, though selected from the cooling curves without knowing how they would fit in with the points selected for their other steels, actually form a fairly consistent curve with them.

The thermal data of Charpy and Grenet are not included in this figure, because their dilatational results appear to be much better evidence.

Series 12. In this series, as indicated in § 45, p. 641, what was actually determined was not the exact temperature of A3 but a gap of 10° or 20° within which A3 lies, though in three cases the intersection of the curves obtained points to a definite temperature within this gap, as in A3. To meet this situation Fig. 14 gives for each of the steels of this series a vertical line covering this gap, and wherever possible indicates in addition by a special cross the exact position of A3 as inferred from this intersection.

Legend for Fig. 14 *and Table Showing Actual Points Plotted in it.*

◈ SERIES 1. OSMOND, thermal end of Ac3 (corrected, from curves).
Carbon, 0.08 (0.16)
Temperature, 918 (945)

⊡ SERIES 2. GOERENS AND MEYER, micrographic (Ar3).

Carbon,	0.16	0.30	0.47	0.54	0.70	0.78
Temperature,	905	895	885 / 875	855	785 / 775	795 / 785 / 765

□ SERIES 3. HEYN, thermal beginning of Ar3 (from curves).

Carbon,	0.08	0.17	0.20	0.39	0.50	(0.75)	0.95
Temperature,	905	890	870	800	785	(7.55)	707

\+ SERIES 4. BURGESS AND CROWE, thermal end of Ac3.

Carbon,	0.03	0.215	0.40
Temperature,	916	865	828

◇ SERIES 5. OSMOND, thermal beginning of Ar3 (corrected, from curves and tables.)

Carbon,	0.08	0.16	0.29	0.57
Temperature,	890	878	810	780

HLM SERIES 6. HOWE AND LEVY, micrographic (Ac3).

Carbon,	0	0.495	0.9
Temperature,	912	768	725

△ SERIES 7. CARPENTER AND KEELING, thermal beginning of Ar3 (from curves).

Carbon,	0.01	0.02	0.05	(0.12)	0.16	0.24	0.38	0.47	0.53	0.61	0.81	0.93
Temperature,	895	892	900	(885)	850	815	765	757	755	740	720	695

B ϑ SERIES 8. BOUDOUARD, electric resistance.

Carbon, Heating (B)	0.205	0.493	Cooling (ϑ)	0.205	0.493
Temperature,	835	825? / 792		835	730

o SERIES 9. CHARPY AND GRENET, dilatational, cooling (Ar3).

Carbon,	0.03	0.07	0.15	0.20	0.25
Temperature,	912	885	860	833	808

▽ SERIES 10. ROBERTS-AUSTEN, thermal, maximum of Ar3 (from diagram).

Carbon,	0.08	0.09	0.10	0.11	0.16	0.165	(0.22)	0.30	0.34	0.54	0.69	0.81
Temperature,	882	860	877	860	835	790	(768)	790	768	730	720	685

× SERIES 11. BURGESS AND CROWE, thermal beginning of Ar3.

Carbon,	0.03	0.215	0.40	0.92
Temperature,	889	820	754	707

SERIES 12. MEUTHEN, calorimetric, heating (Ac3).

Carbon,	0.06	0.13	0.32	0.54	0.63	0.80	0.90
Temperature gap,	900 / 880	890 / 870	770 / 750	740 / 730	740 / 730	720 / 700	720 / 700

✦ Temperature of intersection, 767 738 738

AAA AVERAGE LINE.

Carbon,	0	0.483	0.9
Temperature,	917	769	725

○ In four cases certain of these symbols are enclosed in large circles to indicate that the observations represented are ignored in calculating the average line *AAA*.

0 %

E

F

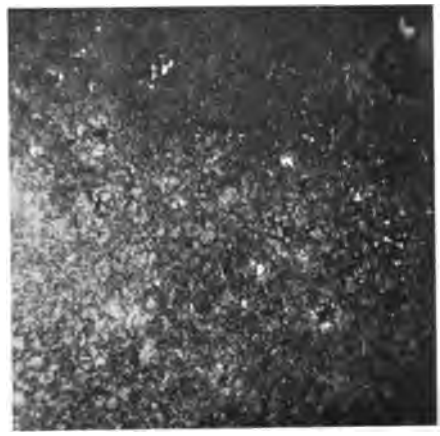
E

F

830°

18 %

840°



6 %

810°

10 %

820°

0 %

810°

0 %

E

F

E

F

1000° —> 830°.

15

PRECIPITATION.

Steel H quenched from 830° after heating to 1,000° and then holding 30 min. at 830°.

780°

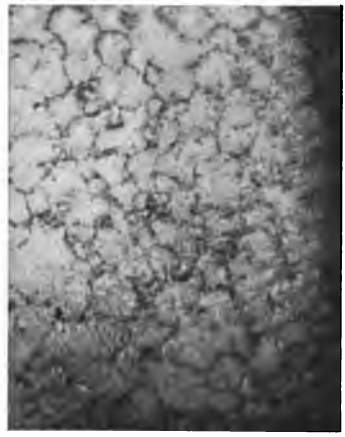
0

890°

E

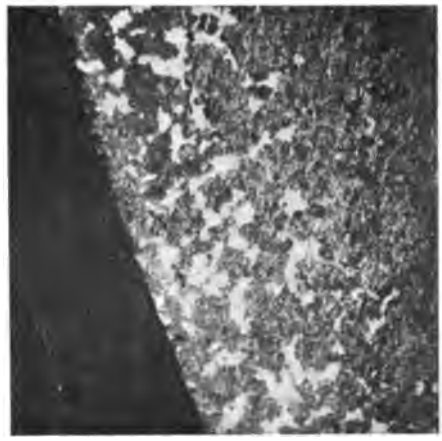
>< About 10 mm. >

G H



Row 13

× 45 × 45



× 45 × 45

Centre Edge

IV, 93, heated at 850° for 30 min., and quenched, shows much decarburization at edges.

G H

E

900°

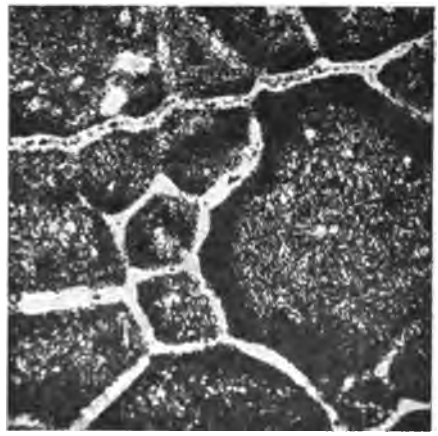
870°

E

F

950°

870°. Interior. troostitized



F

The Position of Ae3 in Carbon-Iron Alloys.

Discussion of the papers of Messrs. Howe and Levy, Burgess, Crowe, and Rawdon, and H. M. Howe, pp. 587 to 648.

ALFRED STANSFIELD, Montreal, Canada:—In Professor Howe's paper on the position of Ae3, he shows its industrial importance in determining the temperature to which steel should be heated for "grain refining." Several years ago I carried out a research on the "burning" of steel and found that while hypo-eutectoid steel should be heated to Ae3, hyper-eutectoid steel that has been very much overheated must be reheated to the curve *Sa* on the carbon-iron diagram, in order to redissolve the pro-eutectoid cementite and thus to obtain recrystallization and grain-refining. This treatment was necessarily only partly successful, and I should like to ask whether the conditions of heat treatment for refining overheated high-carbon steels have been ascertained.

Speaking of the "Persistence of Solidificational Segregation" Professor Howe quotes an experiment made by Mr. Stead in which a bar of 1.2 per cent. carbon steel was immersed in a large mass of molten blast-furnace slag and allowed to cool with it. The steel was heated above the solidus (curve *Aa* on the carbon-iron diagram), but below the liquidus (curve *AB*). A drop of eutectic carbon-iron alloy, rich in carbon, had liquated out and hung below the bar. This agrees exactly with my own observations on the burning of steel, but my interpretation differs somewhat from that of Professor Howe. Professor Howe considers that the fusion of a part of the metal (rich in carbon) shows that the solidificational irregularities of composition had persisted through the rolling and heat treatment of the steel, as otherwise no part of the steel would have been sufficiently fusible to melt. It does not appear to me that such an explanation is necessary. A piece of steel represented in temperature and composition by any point within the area *ABa* will ultimately resolve itself into a solid phase and a liquid phase in equilibrium with each other. It may appear that if the steel were absolutely uniform no fusion could take place, but the smallest nucleus of high-carbon, fusible alloy would ultimately bring about this separation into liquid and solid phases, by diffusion of the carbon from the solid to the liquid areas. A nucleus sufficient for the purpose might be provided by an area of pro-eutectoid cementite which had not become diffused during the

heating of the steel, and it is open to question whether even this nucleus would be necessary to produce the ultimate separation into solid and liquid phases.

The liquid phase, if 1.2 per cent. carbon steel were just above its solidus temperature, would contain about 2.5 per cent. of carbon, instead of the 3 per cent. observed by Mr. Stead. When steel is "burnt" (heated above its solidus temperature) the liquid phase forms between the grains of steel and may ultimately flow downward into the lower part of the piece of steel. Such a segregate, during slow cooling, would become still richer in carbon by deposition of crystals of austenite, and could thus easily reach 3 per cent. or more of carbon.

In § 29, "The Influence of Manganese," discussing the effect of manganese on the carbon content of the eutectoid, Professor Howe argues, Fig. 10, that a lowering of the carbon content of the eutectoid by the addition of manganese would probably steepen the lines *GO* and *OS*, and as no steepening has been observed, he infers that the carbon content of the eutectoid is not lowered. Without discussing the general question whether the eutectoid content is or is not lowered by manganese, I would like to point out that such a lowering might take place without any steepening of the curves GOS.

Let us suppose that the curves *GO* and *OS* remain of the same steepness, but that the points *G*, *O*, and *S* are all lowered by the addition of manganese; then if these points are all lowered equally the carbon content of the points *O* and *S* will remain as before, but if *MO* is lowered less than *G* the distance *MO* will decrease, and further, if *PS* is lowered less than *MO* the distance (*PS–MO*) will decrease; the carbon content of the eutectoid being lowered by the sum of these changes. It is clear then that the carbon content of the eutectoid, when manganese is added, depends on the relative lowering of the points, Ae_1, Ae_2, and Ae_3, and not merely on the steepness of the curves *GO* and *OS*.

HENRY M. HOWE:—We made a few determinations of the line *SE*, but I am not prepared to draw that line to-day. It is not at all of the industrial importance of the line A3, first because but a small proportion of our steels are hyper-eutectoid, and second because refining these by heating to *SE* is not usually wise. In common practice we allow free ferrite to accumulate in networks in hypo-eutectoid steel, and its re-absorption by heating to A3 is not accompanied by any special difficulties. But in the case of hyper-eutectoid steel the matters are very different. Free cementite is not usually and should not be allowed to form a coarse network, because the destruction of

that network is troublesome. If you do destroy it by reheating to *SE*, it will re-form if your cooling is slow. If you cool rapidly to prevent re forming, you may easily crack the steel. Therefore unless the conditions are such that the cooling from *SE* can be accompanied by forging to prevent the formation of a cementite network, this method of refining is unwise. It is better to agglomerate any free cementite into rounded and relatively harmless masses by holding at slightly below A1, than to attempt to refine by heating to *SE*.

G. K. BURGESS, Washington, D. C.:—The particular item of interest, it seems to me, is that the thermal study and the micrographic study of these specimens give practically the same results, within the limits of observation. This is extremely encouraging and shows that for this kind of work, which often is very delicate, we can work with a considerable amount of confidence by one or the other method, and obtain within reasonable limits the same results. I do not know that I have anything of importance to add to what Professor Howe has said in the matter, or to make any new contribution to the discussion. I should be very glad to hear from any others present and get their criticism of any of the results which have been obtained and the methods. I may say that the actual curves, cooling and heating curves, are not as satisfactory as I could have wished, as they have been obtained with an apparatus which was in a state of development at the time, such that possibly the cooling curves of this same steel could now be determined with more definiteness and precision than they were at the time they were taken.

K. W. ZIMMERSCHIED, Detroit, Mich.:—The remarks of Professor Howe on the expulsion of ferrite harmonize well with our conceptions of the phase rule. In any stable system composed of two or more phases, these will be separated by definite boundaries, and when we consider that the expulsion he mentions takes place above the Ar1 point, where we have ferrite separating from martensite, it is evident that all the necessary conditions for the phenomena he describes are present.

If we accept this explanation, it must apply also to other constituents, and we have evidence that it does. Some years ago I was investigating certain parts of the carbon diagram, and had occasion to quench a steel containing about 1.6 per cent. of carbon from above the line of first fusion. It will be recalled that, on cooling, solidification begins at the *AB* line and becomes complete at the *Aa* line; between these two we have mixtures of solid and liquid, the latter

higher in carbon than the former. On heating, the reverse takes place. Starting at Ac1 with 1.6 per cent. carbon steel, we have first the formation of martensite from the pearlite grains; on raising the temperature this martensite dissolves the excess cementite until a homogeneous solid solution results. At the *Aa* line (about 1,300° C. for this steel) a liquidus forms, but since this must be higher in carbon then the average of the mass, some local concentration in carbon must take place.

Fig. 15 shows the structure resulting from quenching this steel just above this point; grains of austenite and martensite are surrounded by films of what appears to be carbide or cementite.

Fig. 16, at a magnification of 1,000 diameters, illustrates more clearly what is happening at a point of maximum reaction—that is, the junction of four grains of austenite; the carbide here is draining out of the crystals. On quenching from a higher temperature we get an advanced stage.

Fig. 17, at 500 diameters, shows a decided increase in the amount of liquidus, which, it should be remarked, cannot be quenched fast enough to prevent its separation into two phases resembling ledeburite in structure. Fig. 18, at 1,000 diameters, gives a more intimate view of the situation.

Attention is called, in passing, to the remarkable amount of twinning in the first sample; in some fields on this sample almost every crystal is twinned.

A final test of Professor Howe's hypothesis should be made on hyper-eutectic steel. If such a sample were cooled *in vacuo*, and if the formation of ferrite on the surface in the present samples is not due to decarbonization, we should have in this hyper-eutectic steel an outside layer of carbide.

With regard to Professor Stansfield's remark concerning the proper temperature for hardening hyper-eutectic steel and its relation to the refining of hypo-eutectic steel, we would say, as a result of our experience, that an anneal above the *SE* line may be necessary to refine the grain if it is very coarse, but that such a temperature is wrong for quenching. On holding steel just below Ac1 the cementite agglomerates into spherules between the grains of what becomes globular pearlite, and hardening such material from just above Ar1 results in a martensite with undissolved cementite imbedded in the grains—a structure characteristic of a large class of most successful high-carbon parts.

FIG. 15.—1.6 PER CENT. CARBON STEEL, QUENCHED IN ICE WATER, JUST ABOVE THE TEMPERATURE OF INCIPIENT FUSION. × 500.

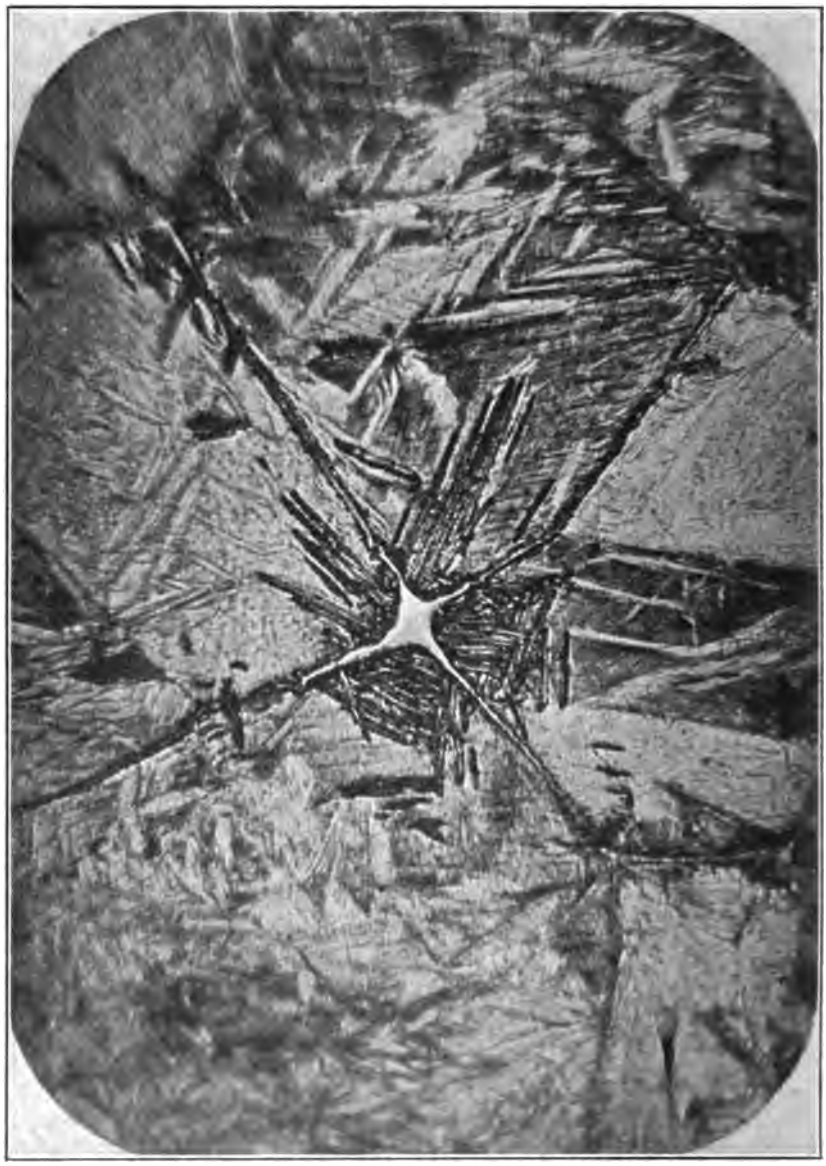


FIG. 16.—SAME SUBJECT AS FIG. 15. × 1,000.

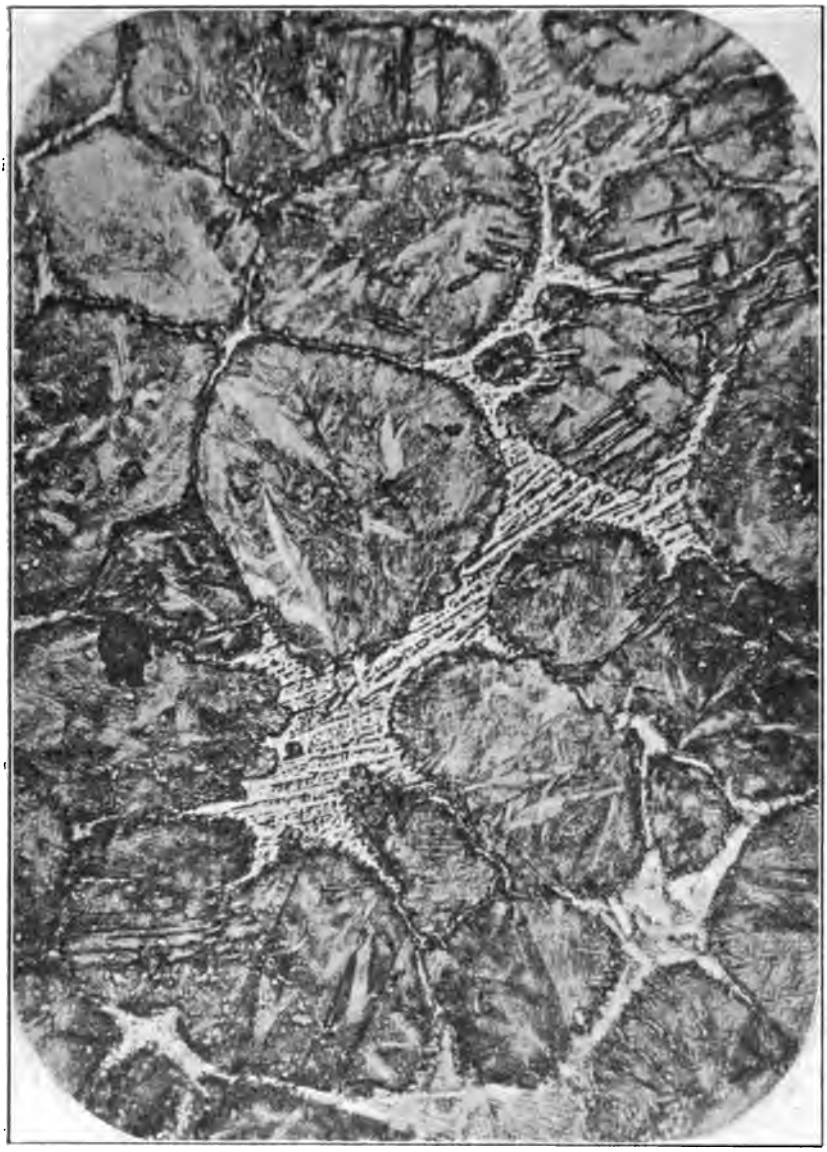


FIG. 17.—1.6 PER CENT. CARBON STEEL, QUENCHED AT MORE ADVANCED STAGE THAN FIG. 15. × 500.

coalescence into visibility possible, and the rapidity of cooling there is far greater than in the lower part. Of these 5 sec. not more than a fraction of 1 sec. would in our case be available for coalescence tending towards visibility.

Beyond this, were his objection well founded, then we should have found more ferrite in the axis than in the outer parts of our specimens, because of the slower cooling of the axis. In fact this never occurred. In passing, I am very familiar with Professor Benedicks's classical paper to which Professor Carpenter refers. Indeed it forms a very considerable part of the foundation of my paper, Why Does Lag Increase with the Temperature from which Cooling Starts,[32] in which I set out to explain some of the phenomena which Benedicks there records.

As is pointed out on pp. 601 and 635, the alleged quenching ferrite looks very different from the compact spots of pre-quenching ferrite, into which the ferrite precipitated or remanent during our 30-min. holdings coalesced. Short experience will, I am confident, convince any one of even moderate experience with the microscope of the radically different looks of these two. In our precipitation experiments we found it necessary to preheat the specimen to 1,000°, in order to make this difference between quenching and pre-quenching ferrite sharp in the case of the low-carbon steels, as indicated on p. 591. The difference, which to the eye is striking, is not well shown in a photograph, but a comparison of the sharp dots of pre-quenching ferrite in Figs. *A* and *B* of Row 10, Plate 4, with the more cloudy Piece I of Row 14, Plate 5, gives an idea of the kind of difference.

Professor Carpenter is mistaken, I am confident, in holding that the liability to error through the presence of quenching ferrite increases with the carbon content. The opposite is true. Note that the steels in which the supposed quenching ferrite could be detected had 0.40 per cent. or less of carbon. This is primarily because the progressive decrease in the quantity of ferrite precipitable as the carbon content increases retards its coalescence into visibility, and secondarily because the transformation is so much more repressible in the higher-carbon steels.[33]

[32] *Trans.*, xlv., 516 (1913).

[33] Professor Benedicks refers to finding quenching ferrite in steel of 0.42 per cent. of carbon quenched rapidly from 850.° What he took for ferrite may have been one of those unexplained light-colored bodies such as Heyn noted in what should have been pure martensite. Heyn's light-colored parts were probably lighter-colored martensite which may well be heterogeneous. On pp. 601–602 we call attention to white spottings in martensite under like conditions. The martensite quenched from the lower temperature was relatively homogeneous; that quenched from the higher temperature containing spottings which might well be taken for ferrite, but certainly were not, because they troostitized black.

While it is true that the end of Ac3 cannot be detected as sharply as the beginning of Ar3, that consideration is less important than the much greater degree of lag in Ar3 than in Ac3. For our purpose Ac3 is a better guide than Ar3 in spite of its being less sharply marked. Beyond this, the Ar points are likely to be brought below Ae1 by being not saturation but supersaturation points. That is to say, in the absence of ferrite nuclei, ferrite may refuse even in infinite time to precipitate even well below Ae3, precipitating only on reaching the supersaturation temperature, at which from being metastable it becomes labile. That there is such a metastable region between the saturation and the supersaturation lines is strongly indicated by the paper of Burgess and Crowe on The Critical Ranges A2 and A3 of Pure Iron, in which they find that, even for zero rate, Ac3 is invariably higher than Ar3.

We have to thank Professor Carpenter for calling attention to errors in Table VIII. These errors do not affect the argument in the least.

The apparently decarburized skin found by Dr. Rawdon after a vacuum heating of a previously untreated specimen (p. 607) is to be referred, not to decarburization, but to ferrite expulsion. To explain, my theory of the ferrite and cementite network is that each grain of mother austenite expels to its outside the ferrite to which it gives birth, as mothers in general expel their offspring, or as the eye expels a grain of sand. In the crystallization of salts from aqueous and other solutions the same occurs. The salt precipitates along the outside of the liquor, that is to say on the sides of the vessel, on strings or strips suspended in it, and very noticeably on the upper surface of the liquor, each of which is in effect the outside of the liquor, its surface. In like manner the outer surface of a specimen represents the outer surface of each and every austenite grain which abuts against that surface while the metal is above the transformation range.

When in machining the specimen the section cuts through an austenite grain, or the region which an austenite grain will later tend to occupy, that section thereby becomes the surface of that present or future grain, exactly as a string hung in an aqueous solution becomes thereby part of the outer surface of that solution. Therefore when in cooling through the transformation range, an austenite grain, thus abutting against the surface of the specimen, expels to its outside the ferrite which it generates, it expels it perforce to that surface exactly as if that surface were part of the network system, which indeed it is. Hence the border of ferrite about slag inclosures. To test this, speci-

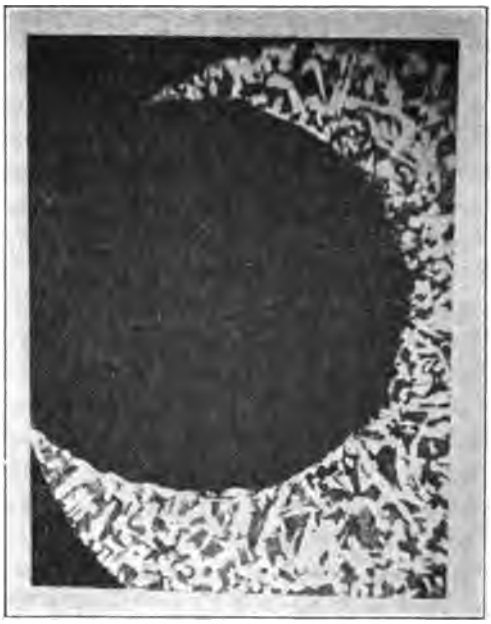


FIG. 19.—HYPO-EUTECTOID STEEL OF 0.40 PER CENT. OF CARBON.

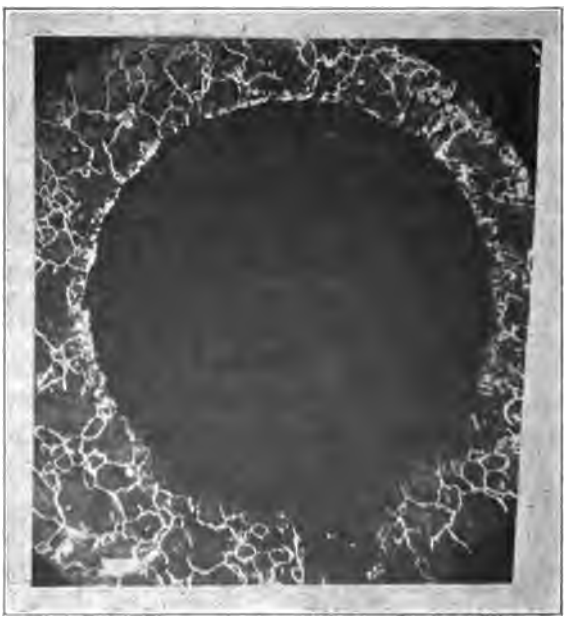


FIG. 20.—HYPER-EUTECTOID STEEL OF 1.45 PER CENT. OF CARBON.

The pro-eutectoid element is rejected to every kind of surface, including internal cavities.

mens of hypo- and hyper-eutectoid steel had small holes cut in them, which were then hammered tightly together and dipped in molten copper to seal them tight. The specimens were then heated above the transformation range in a reducing atmosphere and cooled slowly. The photographs, Figs. 19 and 20, show how the ferrite in the hypo- and the cementite in the hyper-eutectoid steel were thus crowded to the surface of the specimen, *i. e.*, to the walls of the hole. The crowding of the cementite hither cannot represent either local decarburization or local carburization, but parietal expulsion. Dr. Stead has described to me a beautiful experiment in which he crowded the cementite into the weld between two pieces of hyper-eutectoid steel. This local concentration into these artificial grain boundaries seems weaker than that into natural ones.

H. L. Le Chatelier, Paris, France (communication to the Secretary *):—I have read with great interest Prof. H. M. Howe's paper on the point A3. It constitutes the most important progress accomplished in our knowledge of the equilibrium of the iron-carbon system, since the first researches of M. Osmond. Thermal analysis permitted its author, M. Osmond, to develop quickly the essential points of changes of constitution in steel, as related to its temperature. Following his example the same method was successfully applied, particularly in the laboratory of Professor Tamman at Göttingen, to numerous alloys. This very interesting way of marking out for the first time a field of research, is, however, incapable of giving precise results. The retardations of the transformations, due to passive resistances, so obscure the chemical phenomena within the mass as of necessity to render inexact all determinations made at varying temperature; and the numerous experiments repeated under these conditions have taught us nothing new. Possibly Professor Howe has taken too much pains to analyze and classify the results of these old experiments.

On the other hand, the measurements which he has made by heating to constant temperature present very great importance. His method consists essentially in heating a steel to a fixed temperature for a period long enough to permit the establishment of structural equilibrium; then quenching it in order to fix the structure; and finally annealing it at 350° C., in order to transform the martensite into osmondite (troostite), easily recognizable by metallographic tests.

These experiments established at once, what had not been known with precision before: namely, that heating a steel to a fixed tem-

* Received Sept. 17, 1913.

perature for 30 min. suffices to bring it to its state of chemical equilibrium. This is highly important, not only from the view-point of laboratory research, but also from that of industrial operations, because it determines the period, which need not be exceeded, for rendering the metal homogeneous and regenerating its structure, whether after overheating or simply before quenching.

The experiments of Professor Howe enabled him to draw the curve of solubility of ferrite in austenite. According to his paper, the figures are:

Percentage of carbon.....	0.0	0.2	0.4	0.6	0.8	0.9	1.2
Temperature, deg. C.....	912	855	800	760	738	730	920

The determinations for hyper-eutectoid steels are not sufficiently numerous to permit the tracing of the curve of solubility of cementite in austenite to the superior limit, corresponding with the fusion-temperature of the eutectic cementite-austenite. There is room for some new determinations, to determine this second branch of the curve. These experiments are highly delicate, because, in spite of a very rapid cooling, the hyper-eutectoid austenite always decomposes during quenching, and leaves long parallel lamellæ of cementite isolated in the midst of its grains. Thus the structure is not absolutely fixed by the quenching; nevertheless, there should not be very great uncertainty in the interpretation of the results.

The method of Professor Howe permits numerous applications of perhaps still greater importance for the study of non-ferrous alloys. In the case of iron, we know at least in a qualitative way the progress of the phenomenon. There is nothing to do but to determine in addition the points of a curve of solubility, the form of which nobody doubts. This is far from being the case with certain important alloys, like those of copper, and among them most especially, brass. In this last instance, we are not even certain of the exact nature of the combinations formed.

Professor Howe's method is evidently a very slow one, since each sample must be heated half an hour (perhaps longer for the other alloys mentioned) before quenching, and the same operation must be repeated for each of a series of successive temperatures. Time might be saved by making each experiment with a large number of samples, heated at the same time and quenched in the same bath.

It would be possible, even, to operate upon alloys by superposition, the interest of which as a means of accelerating metallographic researches I pointed out long ago. It would be necessary only to scale the superposition at from 1 to 2 cm. of height, so as to be able to cut samples for chemical analyses at the transition-point for the particular temperature of quenching employed.

At all events, the work of Professor Howe constitutes a notable progress in our knowledge of alloys. Apparently, however, after so great a number of researches with regard to them, there were but few new ideas to be brought forward.

ALBERT SAUVEUR, Cambridge, Mass.:—In the work described by Professor Howe in the three papers he presents at this meeting, he has rendered to metallurgists such service as he only could render. The important contributions to metallurgical science from Professor Howe's ever productive and matchless pen constantly add to our knowledge and command our increasing respect, admiration, gratitude, and affection.

The questions which he discusses to-day are not, as he well said, of a purely academic character, for it is of much practical importance to know the exact position of the industrially important critical points.

Daring, indeed, and reckless, the one who would face Professor Howe's formidable battery of facts in an attempt to dislodge from the intrenched position where he has placed them the points Ae1 and Ae3. For my part, I am well satisfied with these positions, and I think they should be used in the construction of future equilibrium diagrams.

The determination of the position of the Ae3 point by micrographic methods by Professor Howe and Mr. Levy is a beautiful piece of work, indicative of manipulative skill of a high order. The mechanism of the reabsorption of ferrite as hypo-eutectoid steel is heated through its critical range, so well described on p. 602, is most interesting and suggestive; for upon that absorption depends essentially the effectiveness of the annealing of such steels.

Professor Howe's discussion of the existing data as to the position of Ae3 is a scholarly production of such nature as to preclude adverse criticism. The author has so well anticipated every possible objection to his treatment of the subject as to afford little comfort to the critically inclined reader,—a feature, it might be added, so characteristic of Professor Howe's writings.

I should like to say a few words in regard to the determination of the line *SE*. I should like to call attention to the fact that the determination of that line is not of the same industrial importance as the determination of the *GOS* line of hyper-eutectoid steel, because the latter indicates the proper annealing temperature for such steel, it being essentially the refining temperature of hyper-eutectoid steel, and therefore of very great industrial importance. The *SE* line is not the refining temperature, industrially speaking at least, of hyper-eutectoid steel; therefore it is not of the same importance. If we

desire to refine, *i., e.*, to destroy the existing coarseness of structure of, high-carbon steel, we heat it slightly above the A1 point; we do not bring it above the cementite line because we would lose more than we gain. The very small amount of free cementite which is present in such steel is not to be compared, as far as its action on the structure is concerned, with 40, or 50, or 60 per cent. of ferrite in hypereutectoid steel. Therefore, should we attempt, in refining, to bring the heat above the cementite line, we would be sure to cause absorption of the cementite, but we would also coarsen the austenitic structure, and therefore the resulting structure after cooling. So it is evident to me that the *SE* line, from an industrial point of view, is not of the same importance as the A3 line.

There is one more point that I should like to bring out, not that it is not known, but that it is often overlooked, and that is the influence of the temperature from which cooling begins upon the location of the A1 point. Some of us know that there is such an influence. The higher the temperature from which cooling begins—I think I am right—the lower the position of the A1 point. We have done a little experimenting at Harvard in that line, and I have some results that appear to confirm that statement.

HENRY M. HOWE:—I have not very much to say, except to say that I am profoundly touched by the kind words that you have spoken.

This matter of the lowering of the recalescence point by reason of higher and higher temperature, that has been shown so very interestingly by Professor Sauveur, was first noticed by Osmond in his original "Transformations." The reason appears to me, as I pointed out at our last meeting here, that at the higher temperatures a greater number of crystalline nuclei are destroyed. That is to say, if you heat only just above A1 or A3, as the case may be, you have a vast number of crystallized nuclei left there; as the temperature rises and as time goes on, those nuclei are destroyed. When you cool down again it is the presence of those nuclei that determines the recalescence. If the nuclei were completely destroyed, you would probably have to go to a pretty low temperature to bring about recalescence. At a temperature only relatively above A1 there is still an abundance of those nuclei, and the transformation is therefore brought about or precipitated by those nuclei.

I cannot sit down without expressing orally to those who are present my great gratitude for the assistance and support which my collaborator, Mr. Levy, has given me in all this work. It would have been impossible to carry it out without him. His faithful, as well as extremely intelligent collaboration, I want to acknowledge.

The Critical Ranges A2 and A3 of Pure Iron.

BY G. K. BURGESS AND J. J. CROWE, BUREAU OF STANDARDS, WASHINGTON, D. C.

(New York Meeting, October, 1913.)

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THE question of the allotropy of iron, in spite of a vast amount of experimental work and perhaps an even greater amount of theorizing, is not yet settled. That there is a definite transformation in iron near 900,° the A3 point, is generally recognized, as well as the fact that the temperature of this transformation is lowered by the addition of carbon and metallic elements. On heating, the transformation Ac3 is always found at a higher temperature than the transformation on cooling, Ar3. Whether the A3 transformation is sharp like the melting point of a pure substance, or extends over a considerable range of temperatures, embracing perhaps as a lower limit the A2 change, appears to be still an open question for pure iron. The nature and identity of the A2 transformation, it would appear from recent publications, has not yet been satisfactorily settled.

In the present paper a critical, historical summary is given of the experimental investigations of the location of A2 and A3 in pure iron, together with brief mention of some of the theoretical aspects of the subject, and an account is given of a series of experiments carried out with several samples of very pure iron which were studied by two methods of thermal analysis in the range from 500° C. to 1,000° C., and which it is believed furnish a contribution of considerable interest as to the location and nature of the A3 and A2 transformations.

Great attention has been given to the details of experimental manipulation and to the preparation of the samples, as it was soon found that some of the discrepancies noted in the work of many experimenters may be traced largely to lack of precautions which we have found essential.

For example, to anticipate somewhat, it is necessary to provide a suitable practically gas-free furnace, the heating and cooling of which may be regulated to a nicety. To render a minimum the deforming effects on the cooling and heating curves of the poor conduction of the sample, it must be of a form and mass to pass on its heat rapidly and completely to the thermocouple; and finally the sample itself must be freed from gases which, if present, further mask the definiteness of the heating and cooling curves. We have also varied within wide limits the several factors which may influence the determination of the exact location and shape of the transformation ranges, especially the mass and preparation of sample, rate of heating and cooling, design of furnace; and finally, as nearly as it is experimentally possible, we have worked with a substance approaching pure iron, free from occluded gases and other impurities, and contained within a vacuum.

A preliminary notice of some of the experiments carried out in 1911 and 1912 has already been published,[1] but since this work the experimental method has been greatly improved and several newly prepared samples have been studied.

The methods of this investigation are described in detail, as they are the same now being used in a new study of the iron-carbon system, and because they are believed to be of some interest in themselves.

Theories of the Allotropy of Iron.

Several explanations have been offered for the existence and nature of the transformations in iron, and the discussion of this subject has usually been linked with the transformations occurring in the iron-carbon system. For the consideration of *pure* iron, however, it seems simpler and sufficient to forget for the moment the iron-carbon system, which has no more relation to pure iron than, say, the iron-manganese or the iron-sulphur series of alloys. Furthermore, if there were agreement as to the facts concerning these transformations, the establishment of a satisfactory explanation in terms of well-known physico-chemical principles should not be difficult. But we are in the presence of a double uncertainty, both as to facts and as to theory. Until the facts are well established and recognized, any theory can be at best only a working hypothesis.

Briefly stated, the current hypotheses regarding the A3 and A2 transformations in iron are:

(1) The critical points A3 and A2 divide iron sharply into three allotropic forms: γ iron, above A3; β iron, between A3 and A2; and α iron, below A2.

(2) The critical point A3 is the seat of an allotropic change, but the A2 point is not, the transformation at A2 being associated mainly with the loss of magnetism, which is accompanied by a small thermal change. Hence, there is no β iron according to this theory, and the A2 transformation may be abrupt or spread over a considerable temperature interval, as experiment may require.

(3) The critical range A2 is not independent of A3 but forms a part of the A3 transformation. Here, again, we have no β iron, but to account for an evolution of heat on cooling at A2 requires assumptions regarding the mutual solubility of γ iron in α iron, and even the simultaneous existence in metastable equilibrium of two kinds of molecules of the same substance.

[1] *Journal of the Iron and Steel Institute*, vol. lxxxvii. (1913, I), p. 335; *Journal of the Washington Academy of Sciences*, vol. iii., p. 329 (1913.)

This last type of physico-chemical equilibrium has been studied both theoretically and experimentally by Smits for several chemical systems, and for any particular case may be verified by noting the change in location of the transformation temperatures with rate of cooling.

Benedicks has put forth a theory of allotropy in which the allotropic change may be of several types, as illustrated in Fig. 1. To iron he would assign Type II*a*, which relegates A2 to the end point of A3 or the temperature at which γ and α irons cease to interact. If A2 can be shown to be constant in position independently of rate of cooling, then Smits's analysis will not apply, and if A2 persists in magnitude and location for heating and cooling through the A2 range and holding the sample a long time below A3 but above A2, Benedicks's hypothesis becomes of little meaning.

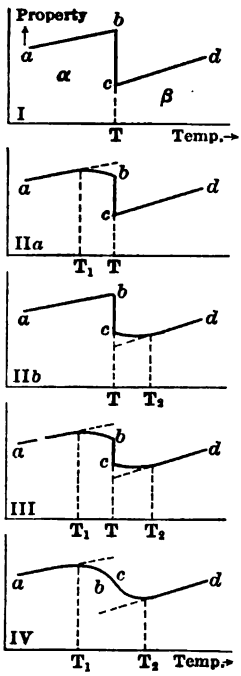

FIG. 1—TYPES OF ALLOTROPY, BENEDICKS.

There are various other minor modifications of the above three hypotheses, all of which have been useful, and perhaps none of which is without value in furnishing some part of a satisfactory explanation of the transformations in iron.

PREVIOUS DETERMINATIONS OF A2 AND A3 IN IRON.

We shall pass in review briefly the experimental evidence as to the existence and nature of transformations in iron at A2 and A3 as shown from observations on the physical properties of iron, including dilatation, electrical and magnetic changes, specific heat, evolution and absorption of heat, crystalline structure, and deformation under stress. As we are dealing with a single element, purely chemical changes would not be expected to shed any light on the question of such transformations, and there appear to be no strictly chemical data bearing on the subject, except some inconclusive work on the dissolving power for carbon.

In quoting the numerical values of the temperatures assigned to transformations as noted by the several observers (See Tables I and II), it should be borne in mind that there are considerable discrepancies in the temperature scales used, and sometimes the method of

experimental operation leads to incorrect temperatures of the phenomena measured. No general attempt is made here to correct the temperatures as given by the several observers, as for the most part such correction would be extremely uncertain. This uncertainty is further enhanced in some cases by the impurity of the samples of iron operated upon.

In this historical summary, we are more concerned with the qualitative aspect of the transformations—their very existence, in fact—than their exact location, which latter we believe is most exactly defined by our own experiments.

Critical Ranges as Determined by Expansion.—Both from the work of Charpy and Grenet and of Broniewski, observations on the expansion of iron give no indication of the existence of A2. The former place the contraction observed as lying in the interval from 860° to 890° for iron with 0.03 carbon, while the latter finds it above 950° for electrolytic iron.

The disagreement here is great enough to again raise the question, as was done by Le Chatelier in 1899, of the speed and other conditions of heating, and also whether the sensibility of the methods used was great enough to detect A2. Even its non-existence, in so far as expansion is concerned, would prove only that iron immediately above and below A2 has the same coefficient of expansion, or simply that the A2 transformation is unaccompanied by appreciable change in volume, a not unreasonable possibility. The more recent experiments of Rosenhain and Humfrey would appear to indicate a slight volume change accompanying A2.

Critical Ranges by Thermoelectric Observations.—This method of observation is open to the serious objection for the exact location and description of critical ranges, that the iron which forms one element of a thermocouple must be partly in a hot region and partly in a cold. The reactions due to whatever transformations take place will be progressive along the iron, and thereby may mask any transformation on heating, for example, after the one taking place at the lowest temperature.

Using a Fe-Cu thermocouple, Broniewski finds for electrolytic iron a hazily defined point of inflexion at 730° and another at 950° on heating (or at 850° for 0.07 per cent. carbon), but with a Fe-Pt couple he is able to detect only the point at 1,020°, which Müller claims is due to hydrogen. Both Harrison and Belloc fail to get any thermoelectric discontinuity with Fe against either Cu (Harrison) or Pt (Belloc), but appear to find a maximum for the curve of thermoelectric power at about 800°.

Thus the thermoelectric data on the critical ranges of iron appear to be meager and of little value, and the thermoelectric method cannot register transformations sharply except with an indefinitely short furnace, or one having no appreciable length between the inside and outside temperatures along the iron sample.

Critical Ranges in Terms of Crystalline Structure.—There appears to be practical unanimity of opinion concerning the evidence offered by the various investigations (Osmond and with Cartaud, Stead and Carpenter, Rosenhain and Humfrey, and others) concerning the existence of changes of crystalline structure: namely, that the change occurring in the A2 region is not accompanied by any appreciable crystalline change, while the A3 transformation in pure iron is so accompanied. All these experiments appear to show there is no crystallographic difference between β and α iron, although there does not appear as yet to be perfect accord as to the nature of the crystalline change taking place at A3.

Critical Ranges as Detected by Mechanical Methods.—The study of the mechanical properties of metals at high temperatures offers very great experimental difficulties, and, as far as approximately pure iron is concerned, the only experiments which shed any light on the detection of A2 and A3 by mechanical means are those of Rosenhain and Humfrey, who have carried out two series of measurements on tensile strengths, the first of a more qualitative nature, in which an iron 99.76 pure, containing 0.03 carbon, was used, and the second, much more elaborate, with iron containing 0.1 per cent. carbon. Both series show a distinct discontinuity for the A2 range and for the A3 range. See Fig. 2.

Critical Ranges as Given by Electrical Resistance.—The electrical resistance of approximately pure iron at high temperatures has been studied among others by Le Chatelier, Hopkinson, Morris, Boudouard, Harrison, A. R. Meyer, and Broniewski.

The observations show, when plotted as resistance (or ratio of resistance hot to resistance cold, R^t/R_{20}) against temperature, a continuous curve which begins to inflect at temperatures ranging from 700° (A. R. Meyer) to 800° or higher (Le Chatelier, Hopkinson). At about 900° to 950° the curve becomes nearly horizontal for all observers, and according to Broniewski has another inflection at 1,020°. This observer, however, took observations with the iron in an atmosphere of hydrogen, but Müller has shown that with electrolytic iron a critical point is obtained thermally in this region only until the hydrogen is removed by successive heatings.

The observations of Meyer appear to have been taken with consid-

erable care on very pure samples of iron, and when plotted as dR/dt against t give a cusp at 700° and are strikingly similar to the magnetic susceptibility curves as ascertained by Curie and others, as shown in Fig. 2. The observations of Morris, who used iron 99.92 pure, give a sharp maximum for dR/dt at 765°, and Somerville gets one at 750° with an undefined iron, while Boudouard finds two breaks in the R : t curve, one at 775°, the other at 885°.

As in the case of nickel with its single transition point in the region about 370°, the electrical resistance curve appears to antici-

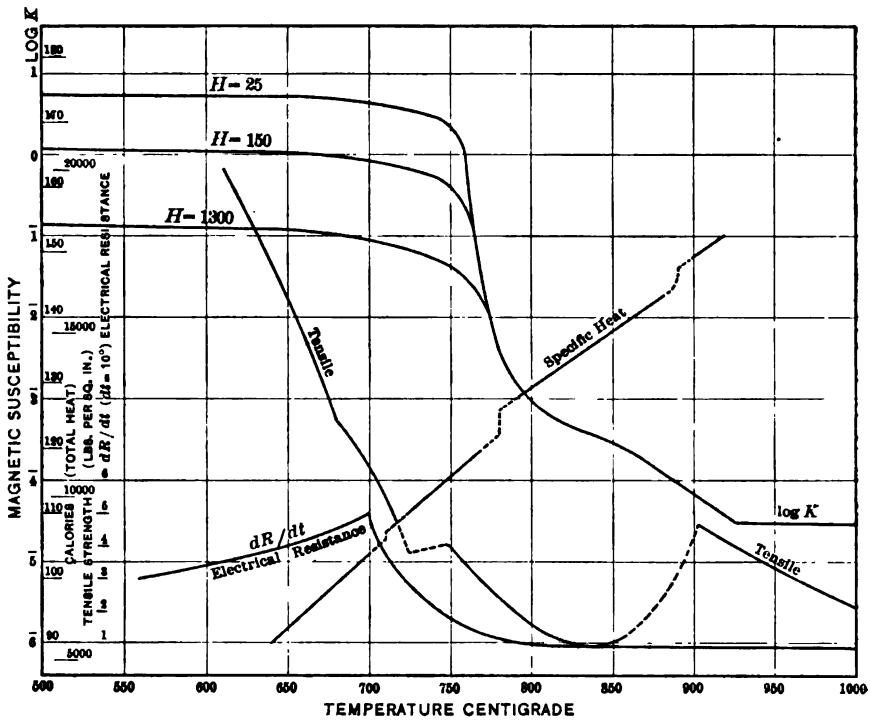

FIG. 2.—PHYSICAL PROPERTIES OF PURE IRON.

pate, as it were, the A2 transformation for iron, which, as measured by this phenomenon, may be considered as starting some 60° or 70° below the maximum; or, in Benedicks's nomenclature, is a transformation of Type II*a*. The observations of Broniewski also show a second inflection (for A3?) at about 950°, although, if present, this inflection should have been detected by Morris, Meyer, and others.

Critical Ranges as Determined Magnetically.—Hopkinson, using a wrought iron containing 0.01 per cent. of carbon and 0.362 per cent. of other impurities, mostly slag, finds a cusp in the permeability curve

at 775°, the permeability for a magnetizing force of 0.3 falling from 11,000 at 775° to 1 at 785°. With a mild steel containing 0.126 per cent. carbon and 0.244 manganese, the peak is at 730° for the same small magnetizing force.

The exhaustive experiments of P. Curie show that the intensity of magnetization (I) is markedly dependent upon the value of the magnetizing force (H) for iron containing only 0.04 per cent. carbon, and decreases (for all values of H) at first gradually with rise in temperature and above 700° with increasing rapidity, reaching almost to zero at 750°, for all values of H, where there is an abrupt turn in the I: T curve. Above 750°, the I : T curve possesses no considerable variations until 1,280° C. The magnetic susceptibility $k = I/H$ (plotted as log k : T in Fig. 2) decreases with great abruptness at about 750°, although the decrease in k begins gradually from below 625° (high values of H decreasing k at lower temperatures than low values of H). There is a further abrupt drop in k beginning at about 860° and terminating sharply at 920°. Terry finds that ferro-magnetism disappears on heating and reappears on cooling at 785°.

The magnetic behavior of iron in the region about 750° appears in all respects similar to the behavior of other ferro-magnetic substances, such as nickel, cobalt, and magnetite, in the region of their single magnetic transformation points, which for these substances, it is of importance to note, are also transformation regions as measured by other physical properties, such as electrical resistance, thermoelectricity, and thermal or calorimetric effects.

This last is well shown by the experiments of Weiss and his associates, who are able to compute from purely magnetic data obtained from higher temperatures the transformation points by means of the equation $k(T - \theta) = \text{constant}$, and compare them with the values obtained calorimetrically. In this way Weiss and Beck find: Transformation points $= \theta - 273$:

	Calculated Magnetically.	Observed Calorimetrically.
Iron	753°	758°
Nickel	376°	376°
Magnetite	588°	580°

Later magnetic observations by Weiss and Foëx place the Curie point, θ (= A2), at 774° for electrolytic iron using less than 0.5 g., divide the β region into two parts, β_1 and β_2, and locate the transition from β_2 to γ iron with great sharpness at 920°.

Magnetically speaking, therefore, it appears reasonable to claim that the critical range A2 for iron is strictly similar to the trans-

formation taking place generally in ferro-magnetic substances as they pass over into the feebly magnetic state. In addition, the iron undergoes a further distinct magnetic modification accompanying the A3 transformation.

Critical Ranges as Determined Calorimetrically.—There have been several determinations of the specific heat of iron at high temperatures, but for most of them, in the range from 700° to 950° the observations are not closely enough spaced to locate the critical ranges with definiteness.

Weiss and Beck were able to identify the Curie point, computed from magnetic data, with an abrupt change in the specific heat of iron at about 755° to 760° (or A2), but did not go high enough in temperatures to reach A3.

Meuthen, using the vacuum furnace and calorimeter of Oberhoffer, has been able to locate both A2 and A3 with considerable exactness for iron containing 0.06 per cent. carbon, in terms of the specific heat. See Fig. 2. He places A2 at 770° to 790° and A3 at 880° to 900°. Also A2 remains constant in temperature to the point *O* in Fig. 3.

The calorimetric data, therefore, give positive evidence of the existence of A2 for iron containing about 0.06 carbon.

From Meuthen's calorimetric observations, the amount of heat evolved at Ar2 is about the same as at Ar3, while by cooling-curve methods all observers find the heat effect at A3 at least three times that at A2.

Critical Ranges of Iron from Heating and Cooling Curves.—An enormous amount of work has been done on the location by thermal methods of the critical ranges of steels and other ferrous alloys, much of which is very contradictory. This is discussed at length in several papers and especially in a recent one by Prof. H. M. Howe.[2] While some light may perhaps be thrown on the position and properties of A3 for pure iron from an examination of the behavior of impure irons, for the A2 transformation in pure iron we would expect to learn little or nothing from a study of impure iron on account of the very small quantity of heat involved in the transformation, which one would expect to be considerably masked or attenuated by the impurities present.

It appears to be a remarkable fact, however, that, generally speaking, whenever A2 is detected at all, it is always found at the same temperature, whatever the composition of the iron alloy. For example, the discussion of Professor Howe would place the break at *O*

[2] H. M. Howe: Discussion of the Existing Data as to the Position of Ae3, p. 611, this volume.

in the line *G O S*, Fig. 3, at 769° C., which is exactly the temperature we find experimentally for A2 max. in pure iron. In other words, the A2 line for the Fe-C system is horizontal, as shown from a discussion of practically all existing data, and the A2 point, viewed in this way, would appear to have the characteristics of a Fe-C eutectoid with the possible important if not crucial exception, of which we do not appear as yet to have sufficient experimental proof, that the quantity of heat involved in the A2 transformation does not vary appreciably with the carbon content from zero to 0.48 per cent. This constancy of heat of transformation together with its constancy in temperature, if both be definitely proved by using a pure Fe-C system, lend force to the idea that A2 is a transformation independent of the carbon present, and from the work of Moore and others, A2 appears to be independent of metallic components.

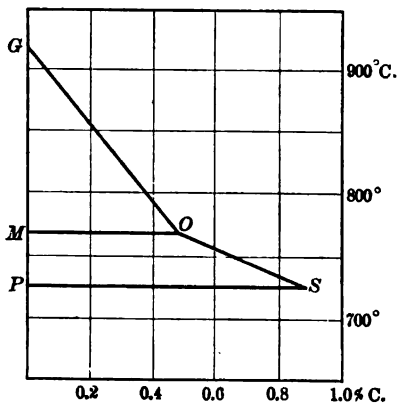

FIG. 3.—PART OF FE-C DIAGRAM.

As to A3, the comprehensive discussion by Professor Howe leads to an equilibrium temperature Ae3 of 917° for pure iron. This temperature, A3 = 917°, also satisfies the observations of practically all recently published work with very pure iron.

Returning to the A2 range in pure iron and its location by heating and cooling curves, we are confronted by considerable contradictory evidence as to its existence, location and type, whether possessing one or two cusps. Professor Benedicks[3] discusses the evidence given by the thermal experiments of Osmond, Roberts-Austen, Arnold and others and gives as well a concise critical summary of the whole subject of the nature of A2 in pure iron as shown by experiment, and concludes that A2 is not an independent transition point but forms a

[3] C. Benedicks: On Allotropy in General and that of Iron in Particular. *Journal of the Iron and Steel Institute*, vol. lxxxvi (1912, II), p. 242.

part, a tailing off, of A3, which latter is taken as a transformation of Type II*a* (see Fig. 1), and "According to this, the nature of β iron is to be α iron containing in solution a limited amount, increasing with temperature, of γ iron."

Since Benedicks's publication, we have the experiments of Carpenter, which he would have taken as verifying Benedicks's (or Smits's) theory; of Guillet and Portevin, who appear to find A2 and A3 as distinct transformations; and of Broniewski, whose results are ambiguous.

Let us consider the experiments of Carpenter more closely. His observations of 1904, taken in association with Mr. Keeling on iron containing 0.01 per cent. carbon, showed Ar2 sharply defined at 762° and Ar3 at 900°. Carpenter's observations of 1913, using iron with 0.007 per cent. carbon or of a carbon purity hardly detectible in difference from his iron of 1904, also show Ar2 as present but not sharply defined and ranging from 768° to 741°, while Ac2 is not in general clearly detectible from his curves except as a swell. The main differences in these two series appear to be, that in the earlier solid, rolled cylinders of iron $\frac{5}{8}$ by $\frac{5}{8}$ in. were used, while in the later sheets of electrolytic iron 0.01 in. in thickness were rolled into a cylinder weighing 42 g. The curves differ in the main as one would expect, the poorer conductivity and the presence of gases in the wound-up sheets of otherwise untreated electrolytic iron producing a flattening out, distortion, and displacement of the critical regions, especially of the feebler A2.

Several experimenters, notably Arnold, insist that A2 has a double cusp for pure iron, while it would appear from the work of several others, notably Carpenter and also Rosenhain, that A2 is erratic, showing sometimes double and sometimes single.

Considering all the hitherto available data, there can be hardly any doubt that, thermally, there is a critical range for pure iron in the neighborhood of 770° (A2); but whether this be separate from the higher critical range near 900° (A3) and whether it (A2) be single or double cusped, the existing thermal observations do not prove conclusively, but a fair inference would be that A2 is single and independant of A3.

Summary of Previous Determinations of A3 and A2 in Pure Iron.— Without, for the moment, considering any theory concerning the meaning of the transformations in pure iron, let us consider what is the present experimental basis for the existence of A2 and A3 either as separate or associated transformations.

A summary of most of the experiments that have been made is

given in Tables I and II; the former includes only heating and cooling curve methods and the latter all others. These tables, taken in connection with the preceding paragraphs, show that there is overwhelming evidence for a discontinuity in properties of pure iron for the intervals of temperature roughly defined as 700° to 780° and 850° to 950°, or corresponding to the critical ranges A2 and A3. (See also Fig. 2.)

TABLE I.—*Thermal Maxima of Critical Ranges of Pure Iron by Various Observers.*

Observer.	Date.	Carbon Content.	Iron Content by Difference.	Ac2.	Ar2.	Ac3.	Ar3.	Remarks.
		Per Cent	Per Cent.					
Osmond	1887	.....	.....	.....	750	.....	890	Ar3 corrected from 855. Two max. for Ar2.
Roberts-Austen	1891	0.007	.....	.....	?	.....	895	
Arnold	1894	0.003	99.967	.....	750	.....	854	Ar2 is double.
Charpy	1895	0.007	.....	740	730	865	840	
		0.009	.....	744	731	903	860	
Osmond	1899	Electrolytic Fe		800	780	905	880	
Charpy and Grenet	1903	tr.	.....	775	730	935	900	
		0.05	.....	755	730	885	860	
Carpenter and Keeling	1904	0.01	99.803	.....	762	.....	900	
Harkhort	1907	.....	99.89	691 to 768	764 to 759	910 to 917	888 to 875	
A. Müller	1909	0.017	99.803	770	763	917	894	23 observations in *vacuo*, all agreeing very closely.
Rosenhain and Humfrey	1909	0.029	99.66	770	763	941	904	
E. Colver-Glauert	1910	Fe "Kahlbaum"	.....	.....	.....	.....	.....	A2 is double.
H. C. H. Carpenter	1913	0.008	99.967	?	768 to 741	902 to 938	898 to 886	Ac2 not detected: 2 samples of same analysis; 10 observations.
Guillet and Portevin	1913	Not detected	99.66	791	778	937	902	as deposited
				788	778	982	902	annealed } Electrolytic

The existence of A3 is not open to question, it having been readily located by practically all methods of physical analysis with the possible exception of electric resistance and thermoelectricity, for which any discontinuity is not very definite.

The magnetic change is also small but abrupt and unmistakable. The thermal observations, which give the sharpest definition of A3, show that the maximum of Ar3 is always lower in temperature than that of Ac3.

Controversy has raged warmly for a quarter century not only about the nature and extent but as to the very existence of A2, which has, however, been generally identified with the loss of ferro-magnetism in iron. The point of inflection of the electrical resistance curve appears to be associated with the A2 region, as well as an abrupt change in specific heat of the same magnitude as at A3. The thermo-

electric effect appears not less uncertain than at A3; there is a small but an abrupt change in tensile strength at A2; and there is a small thermal effect on heating and cooling which appears to remain at a constant temperature for the addition of carbon or metallic components in considerable quantities. Apparently, the only phenomenon studied which has hitherto always given negative results for A2 is crystalline structure.

TABLE II.—*Critical Ranges in Pure Iron by other than Cooling-Curve Methods.*

Observer.	Date.	Carbon Content.	Iron Content by Difference.	A2.	A3.	Method and Remarks.
		Per Cent.	Per Cent.			
P. Curie	1895	0.04	99.9+	750	920	Magnetic.
Morris	1897	0.006	99.925	765	.....	Electrical resistance.
		0.010	.....	770 to 780	.....	Magnetic.
Hopkinson	1889	0.006	99.515	?	880	Electrical resistance.
		0.010	99.128	775	.....	Magnetic, cusp — vs. T curve.
Le Chatelier	1890	.....	.....	780	.....	Electrical resistance.
	1899	0.057	99.763	?	840	Expansion.
Osmond	1900	Electrolytic		?	detected	Crystallographic.
Harrison	1902	?	?	Uncertain		Thermoelectric Cu-Fe couples, H atmos.
				760 ?		Electrical resistance.
Belloc	1903	"Pure	Iron"	?	?	Thermoelectric.
Charpy and Grenet	1908	0.03	.....	.....	860 to 890	Expansion.
Boudouard	1903	"Commercial"		775	885	Electrical resistance.
Weiss and Beck	1908	?	?	760	.....	Calorimetric; A3 not measured.
Rosenhain and Humfrey	1909	0.029	99.767	detected	detected	Tensile and crystallographic, qualitative only.
	1913	0.106	99.374	750	910	Tensile strength.
Terry	1910	0.012	99.30	785	910	Magnetic.
Somerville	1910	0.012	"Iron"	750	?	Electrical resistance.
A. R. Meyer	1911	0.00	99.976	700	?	Fe "Kahlbaum." Electrical resistance, cusp of dR/dt curve.
		?	99.94	700	?	Ingot iron.
		0.00	99.85	705	?	Electrolytic.
Weiss and Foëx	1911	Electrolytic		774	.....	Magnetic; A3 not measured.
Meuthen	1912	0.06	99.960	780	890	Calorimetric.
Broniewski	1913	0.07	99.87	730	850	Thermoelectric.
					950	[couples.
		Electrolytic		730 (a)	1,020 (b)	(a) by Fe-Cu, (b) by Fe-Pt
				750 to 850	950 and 1,020,890	Electrical resistance.
				700?	to 950	Expansion.
Stead and Carpenter	1913	0.007	99.967	Not detected	Detected	Crystallographic.
	1913	.....	.....	.....	.....	Magnetic

Most, although not all, of the phenomena appear to indicate that A2 differs from A3, in that for the latter the transformation is abrupt but taking place at a higher temperature with ascending temperature than with descending, while for the former the transformation is more gradual (of Type II*a*, Fig. 1), but has the same location of maximum on heating and cooling. That A2 and A3 are parts of single transformation, A2 being subordinated to A3, is a conclusion

to be drawn with some difficulty from the data, and as pointed out by Guertler it is difficult to construct any plausible physico-chemical theory which would admit of a distinct heat evolution at A2 and still have A2 as a part of the A3 transformation, for "The end point is never marked by a sudden evolution of a new and constant thermal effect at a given temperature."

The Present Experimental Investigation.

The object of the present investigation is to determine, with several samples of pure iron of different sizes, prepared by various methods, the exact location and nature of the A2 and A3 ranges, using the most refined methods of thermal analysis and keeping the samples free from the masking influences of occluded gases.

The Experimental Method.—Two methods of taking heating and cooling curves have been used simultaneously on the same sample: one, the *inverse-rate* method of Osmond, in which the times required for the specimen to rise or fall successive, equal temperature intervals are noted in terms of the temperature or $(\mathrm{d}t/\mathrm{d}\theta \text{ vs } \theta)$; the other, the *derived-differential* method of Rosenhain, in which the observations are taken by the *differential* method of Roberts-Austen using a "neutral" of platinum,[4] and in which for equal decrements or increments of temperature the difference in temperature between specimen and neutral is taken in terms of the temperature of the specimen $(\theta - \theta' \text{ vs } \theta)/\Delta\theta = \text{constant}$. The results so obtained by the differential method are transformed into the derived differential curve by the operation of dividing the differences, $\theta - \theta'$, for each temperature interval $\Delta\theta$ by this interval and plotting in terms of θ, or giving

$$\mathrm{d}\frac{(\theta - \theta)}{\mathrm{d}\theta} \text{ vs } \theta.$$

These two methods of thermal analysis supplement each other most excellently, as each is subject to different disturbing effects; and although obtained by totally different operations, the resulting heating or cooling curves are strictly similar except for certain minor details which we shall mention later. To operate both methods simultaneously requires no more apparatus or observers than the differential method alone, although in this latter case the chronograph may, less conveniently, be replaced by any sufficiently exact timepiece. It is preferable to have two observers, although this is not essential.

The inverse-rate method is subject to error due to drafts, sudden

[4] For a detailed description of the methods of taking cooling curves, see G. K. Burgess, Reprint 99, *Bulletin of the Bureau of Standards* (1909).

changes in current feeding the furnace, or any cause which may impress upon the specimen thermal changes not proper to it. Inversion of $dt/d\theta$, at recalescence, or an actual increase in temperature on cooling, is not readily expressible by other than a horizontal line in the plot; this weakness of analysis is of minor import, however, as the data on which the inverse-rate curve is based give, nevertheless, very sharply the beginning, maximum, end, and also, when present, the elevation in temperature accompanying a transformation. This last effect, of course, only accompanies a violent evolution of heat and is of no interest here.

The derived-differential method is less subject to error on account of extraneous influences; recalescence cannot be any more readily expressed by this method of plotting than by the other. The presence and properties of a neutral free from transition ranges, as is platinum, have no appreciable effect on the location of the critical points of a substance such as iron under examination.

Arrangement of the Apparatus.—The general appearance and arrangement of the apparatus is shown in Plate I, and a schematic diagram, Fig. 4, gives the layout of connections.

The Measurement of Temperature.—For this purpose, thermocouples were used connected to a five-dial, low-resistance potentiometer designed to be free from thermal electromotive forces, of the Diesselhorst type as constructed by Otto Wolff.

The moving-coil galvanometer (from Leeds & Northup) used with the potentiometer had a constant sensibility for all parts of the range of the potentiometer as used, and gave when critically damped a deflection of 25 mm. on scale and telescope at 2 m. for each 20 microvolts of electromotive force of the thermocouple. As used, it had a resistance of 57 ohms and a period of 2 sec. This sensibility, which was ample, could have been increased if necessary. The zero shift was not troublesome and the instrument was glass inclosed, reducing temperature inequalities. The method of operation, whether for calibration, inverse-rate or differential curves, was to note time, preferably on the chronograph, and this invariably when taking inverse-rate curves, every time the zero of the scale passed the cross hair of the telescope; the dial of the potentiometer was then turned say two steps, corresponding to about 2° C., throwing the zero of the scale back about 25 mm., and the operation repeated indefinitely. The sensibility is about 0.01° C. with the Pt-Rh thermocouples.

The Measurements of Time.—For recording the time measurements, necessary for the inverse-rate method and convenient with the calibration and differential methods, use was made of a cylindrical,

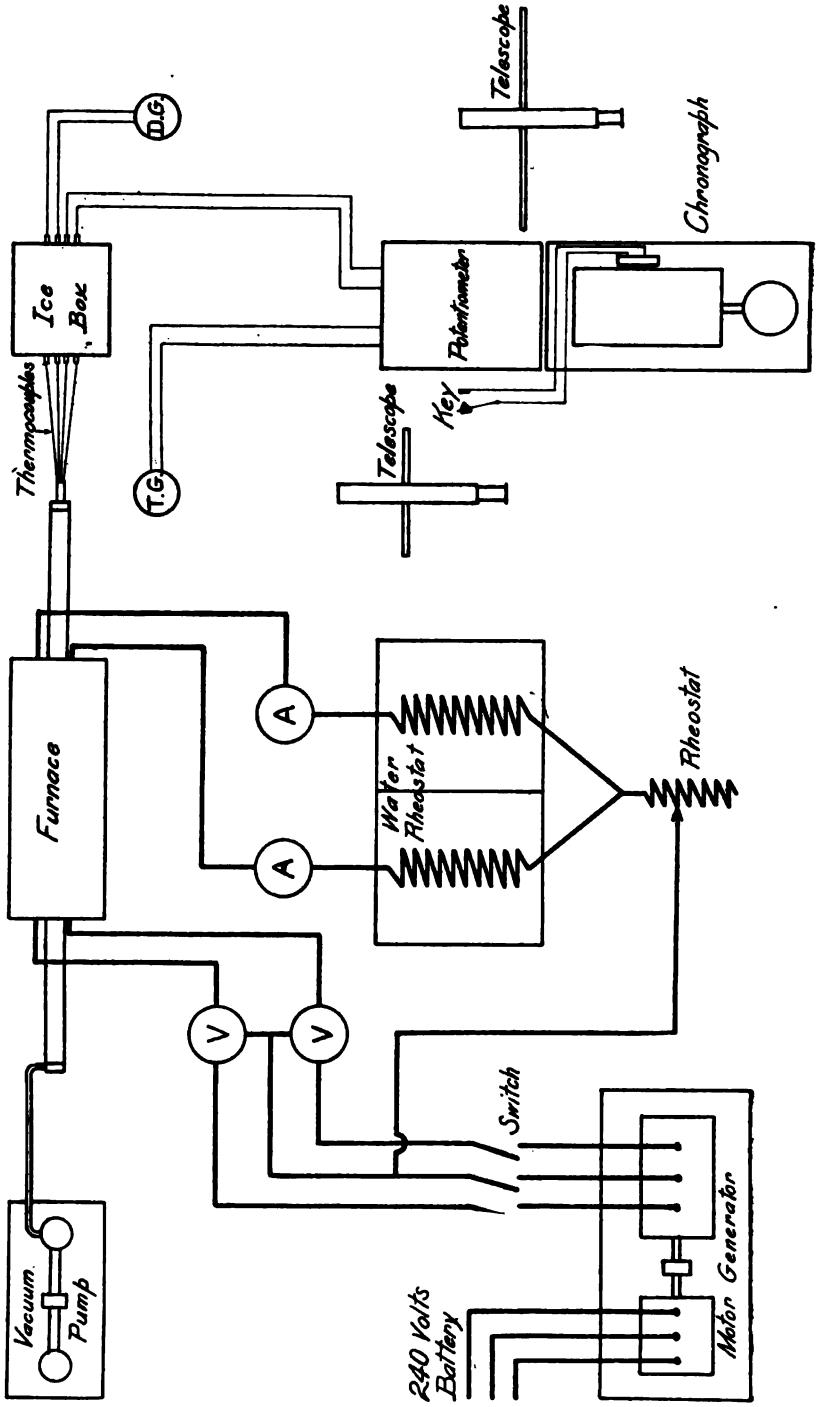

FIG. 4.—DIAGRAM OF APPARATUS.

Geneva motor-driven chronograph together with a telegraph key which, when depressed, gave a dash on the chronograph sheet easily distinguishable from the seconds marks made on the same sheet with the same pen when actuated through a relay by a standard clock situated in a distant constant-temperature room. The time observations were easily obtained to 0.1 sec.

The Thermocouples.—In some of the preliminary work, various thermocouples of 0.6 mm. diameter were used, but the later more reliable measurements were all taken with three Heraeus 0.4 × 150 mm. thermocouples of platinum, 90 platinum—10 rhodium, marked M_1, M_2, and M_3, and all cut off the same batch of wire. These couples were frequently interchanged during the investigation. A differential thermocouple, M_4, for measuring the difference in temperature between the iron sample and the platinum neutral, was made by fusing a length of 45 mm. of 90 Pt — 10 Ir wire between two lengths of platinum wire of the same grade as in the temperature-measuring couples. The cold-junctions of all thermocouples were always kept at 0° C. in a suitable ice box, Fig. 4.

Temperature Scale and Calibration of Thermocouples.—The temperature scale used is that defined by the freezing points of the following pure metals:

	Degrees C.
Zinc	419.3
Antimony	630.0
Silver	960.0
Copper	1,083.0

The calibration of the thermocouples was carried out as follows:

The metals were melted in crucibles 14 cm. deep of Acheson graphite of about 300 cc. capacity in a Heraeus electric-resistance furnace wound with platinum foil. The thermocouples, while immersed in the metal, were protected by out-glazed Berlin porcelain tubes of about 5 mm. diameter. Both freezing and melting points were taken several times for a first calibration, and the couples were occasionally checked at the silver point during the progress of the investigation, and recalibrated at the close, when they showed no appreciable change. All (M_1, M_2, M_3) satisfied the same formula between 400° and 1,100° C., namely:

$$EMF = -313.8 + 8.259\ t = 0.001666\ t^2.$$
$$t_0 = 0.$$

Where EMF is expressed in microvolts and t is temperature centigrade. All thermocouple wires were electrically annealed at about 1,400° C. before using,

It is interesting to note the excellent behavior of these thermocouples in direct contact with iron. The fact that there was practically no oxidation of the iron, together with the fact that the metals were always in a dry vacuum of the order of 0.01 mm. of mercury, probably accounts for this constancy.

Mounting of Thermocouples.—One junction of the differential couple together with the hot-junction of the temperature-measuring couple was inserted in the iron specimen undergoing study and the other differential junction, which was about 4.5 cm. away, was inclosed within the neutral, a platinum cylinder weighing 98 g. The bare wires were in direct contact with the iron, and in fact when very small samples were used, as 0.7 g. of iron from Professor Carpenter, the iron was hammered on to the couples. The sketch, Fig. 4, gives an idea of the arrangement when small cylinders (from 10 to 30 g.) were used.

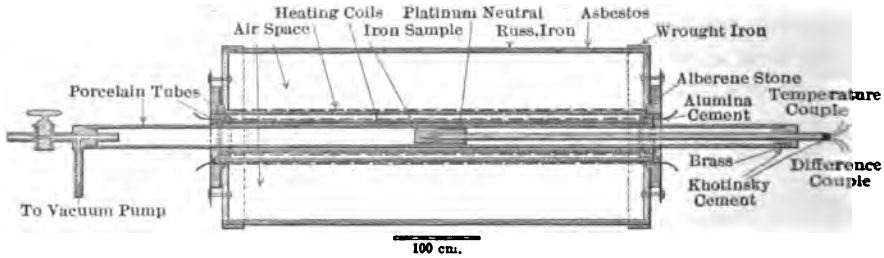

FIG. 5.—FURNACE.

The Differential Measurements.—The differential couple was connected directly to a Siemens & Halske moving-coil galvanometer of 200 ohms resistance. Deflections were read by means of a telescope and scale 2.5 m. distant. As connected, the galvanometer gave a deflection of 60 mm. for 100 microvolts, corresponding to a sensitiveness of nearly 0.01° C. with the Pt-Ir couple used. This was ample, and there would have been no advantage in increasing this sensitiveness, as could easily have been done fourfold by changing the galvanometer connections.

The Furnace and Accessories.—The furnace, shown in Fig. 5, was constructed in the laboratory, and consists essentially of four concentric tubes, three of Berlin porcelain and an outer one of iron backed with a $\frac{1}{8}$-in. layer of asbestos. The innermost tube, which carries the specimens under study, is glazed both inside and out, so as to hold a vacuum, and extends beyond the furnace to receive seals of hard wax and metal for pump and thermocouple. There was no trouble in holding a vacuum of 0.01 mm. of mercury to 1,050° C. The two unglazed porcelain tubes each carry a separate heating coil

of platinum foil 2 cm. wide by 0.001 cm. thick. In some of the preliminary work a furnace was used in which a single heating coil was wound directly on the glazed tube carrying the specimen. This earlier arrangement is less satisfactory than the one above described, since the tube deteriorates sooner and the uniformity of temperature is less well maintained. As will be noted, the furnace is provided with no other insulation than these concentric tubes, thus permitting, when desired, the use of faster rates of cooling and reaching more conveniently lower temperatures than when the furnace is packed with insulating material.

With the construction described, there was no trouble whatever from drafts. The furnace was tested for freedom from critical ranges by taking several blank series of heating and cooling curve observations, using the usual platinum neutral and replacing the iron specimen with a cylinder of palladium. Some of these blanks are shown in Plate VIII.

The vacuum pump used is a two-cylinder motor-driven Geryk pump kept in condition to maintain a vacuum of 0.01 mm. of mercury in the furnace as measured with a mercury gauge, and provided with drying tubes of phosphorus pentoxide.

The energy supply for heating the furnace was furnished by a 5-kw. Siemens-Schuckert motor-generator set delivering alternating current to the furnace and run from a 240-volt storage battery. Two of the three phases generated by the alternator were used at a voltage which could be set anywhere between 50 and 170 volts. This somewhat elaborate arrangement was found to be highly desirable when, as here, one is seeking minute thermal effects and every extraneous cause of variation in the rate of heating or cooling of the sample must be eliminated. There are two further advantages gained by using alternating current instead of direct for heating the furnace: it permits the use of a liquid rheostat without polarization or electrolysis and it eliminates any magnetic field about the iron sample. This last, if present, might conceivably influence the magnetic transformation in iron.

The rheostat, Fig. 6, deserves special mention, as it was specially designed to give automatically a constant rate of heating and cooling, variable at will within wide limits. In addition to wire rheostats a salt-water rheostat was placed in the heating circuit.

It consisted of a cypress box of two compartments, each measuring 30 by 35 by 35 cm., in each of which were two copper plates (35 by 26 cm.), placed 30 cm. apart at the bottom and 1 cm. apart at the top. A metal box through which cold water was circulated for the purpose

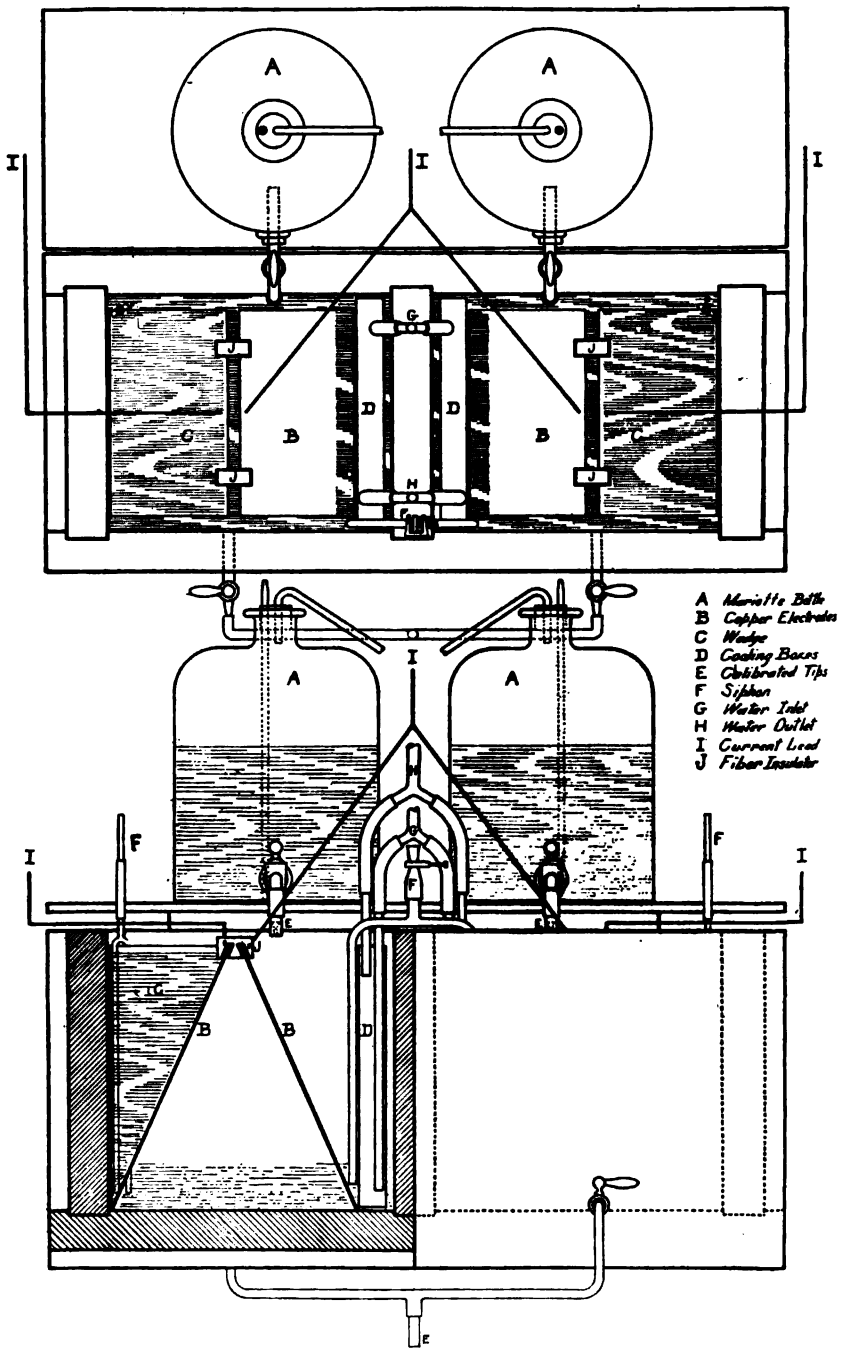

FIG. 6.—RHEOSTAT.

of keeping the salt water cool was placed on each side of the partition. The outside copper plate in each compartment, besides performing its functions as an electrode, also formed one side of a wedge which opened at the top and could be filled with water before starting a run. This was done to cause the water which was fed at a constant rate from a Mariotte bottle to rise between the copper plates at a faster rate as the temperature of the furnace increased, thus overcoming the greater radiation losses to which the furnace is subjected at the higher temperatures and thereby giving a very constant rate over the whole range.

When taking cooling curves the water in the wedges was kept at the same level as the water in the compartment by connecting the two with a siphon; thus all the water was siphoned off at the same rate. A series of brass outlets were constructed and calibrated for this rheostat, which gave a series of definite times of heating or cooling of the furnace, ranging from a few minutes to several hours.

The Materials Investigated.

Sources of Iron.—It was considered worth while to examine the thermal behavior of several samples of pure iron prepared by different methods and subjected to various preliminary treatments. We are indebted to Prof. C. F. Burgess, of Wisconsin, who furnished three samples of his electrolytic iron in the form of anode plates with analysis; to Messrs. H. Goldschmidt and Cuntz, who furnished electrolytic plates and analysis of iron obtained from the German firm of Langheim-Pfanhauser; to Prof. H. C. H. Carpenter, of Manchester, England, who kindly sent us some of the electrolytic iron he had used in his experiments on the critical ranges in pure iron, and which had been analyzed by Prof. J. E. Stead. Several samples of electrolytic iron were also prepared for this investigation at the Bureau of Standards by Messrs. Cain and Cleaves, using a method which is to be described elsewhere. There is also included a sample of "ingot iron," for which we are indebted to the American Rolling Mill Co., of Middletown, Ohio.

Preparation and Description of Samples.—The samples were prepared for taking heating and cooling curves by cleaning with alcohol and ether. In the case of the untreated electrolytic irons of C. F. Burgess and Langheim-Pfanhauser, the samples 1, 16, 17 and 26 a, in the form of plates, from $\frac{1}{8}$ to $\frac{1}{4}$ in. thick, were bored out to receive the thermocouples. Some preliminary but less satisfactory measurements were made with small pieces packed about the thermocouples and within a thin-walled platinum cylinder of 3.5 cm. length and 1.5 cm. diameter. With this last arrrangement, the

transition points were not always very sharply defined, due apparently to a progression of the reaction from one piece to another.

The first curve, and often several of the succeeding curves taken with untreated electrolytic plates which are known to be heavily charged with gas, especially hydrogen, was erratic, an effect due for the most part to the presence of these gases within the sample producing parasitic evolutions and absorptions of heat. See Plate XI. This gas effect has been noticed by other observers.

To obviate this troublesome effect, it was decided to melt some of the samples of iron before taking the heating and cooling curves, which melting was carried out successfully both in a vacuum electric furnace and in gas-fed furnaces. This melting of the iron, provided contamination from crucible or furnace atmosphere is avoided, has the great advantage of further purifying the iron by the removal of all or nearly all of the contained gases. Electrolytic iron, for example, bubbles very violently on first melting *in vacuo*, showing that a considerable quantity of gas remains occluded to the temperature of fusion and is only removed by melting the iron. Melting *in vacuo*, as is shown for samples 25 and 26 as compared with 1 and 26a, Table III, appears also to lower the already very small carbon content of the iron to almost nothing, again increasing the purity with respect to the most undesirable element, carbon. Finally, electrolytic iron, which has been melted either *in vacuo* or in gas furnaces, possesses a sharply defined heating curve the first time such iron is reheated, so that a very troublesome source of uncertainty is removed by this melting. That the presence of gases in the iron has a considerable effect on the location and range of the critical ranges is also shown in later paragraphs.

For all these reasons it appeared to us desirable, when making a thermal study of iron and its alloys which occlude or contain gases, to remove these gases by a previous melting of the metal, preferably *in vacuo*.

There was considerable range in mass from one sample to another used in taking heating and cooling curves, as shown in Tables IV and VI, or from 0.7 to 35 g. It is our experience that it is advantageous to work with as small samples as the precision of the experimental method will permit when it is desired to locate the exact temperatures of the critical ranges of the material. The apparent advantage to be gained with larger samples, which absorb or set free greater quantities of heat, appears to be more than offset by the conductivity of the larger sample not being sufficient to transmit promptly enough the heat generated to the thermometric device. If there is

non-uniformity of temperature throughout the sample, this effect is enhanced; and a phenomenon which really takes place at a single temperature will then appear to be spread over a range of temperatures.

We believe this spreading and attenuation of the reaction is a principal reason why Professor Carpenter was not able to detect AC2 with certainty using 42 g. of metal in the form of a thin sheet coiled into a cylinder, while with only 0.7 g. of his iron we were able to locate AC2 with great sharpness. (See Plate VI.)

For melting *in vacuo*, the Arsem furnace was used, the iron being contained in a crucible of pure magnesia placed within a tube of Marquardt porcelain. For melting in the gas furnace, covered clay crucibles brasqued with pure fused magnesia were used.

TABLE III.—*Description and Chemical Analyses of Pure Iron.*

Source.	Description.	Analysis Furnished by	No.	C.	S.	P.	Si.	Mn.	Cu.	Fe by Difference.
H. C. H. Carpenter.....	Electrolytic 0.01-in. sheet.....	Stead.....	F5	0.008	Tr.	0.002	0.014	0.009	.....	99.967
Am. Rolling Mill Co.....	Ingot iron remelted in gas furnace	Bur. Stds.....	F1	0.012	0.020	0.001	0.003	0.029	0.03	99.905
J. R. Cain.....	Electrolytic remelted in gas furnace	Bur. Stds.....	F2	0.008 0.009	0.045 0.043	Tr.	0.004	Tr.	>0.001	99.941
Langheim-Pfanhauser...	Electrolytic remelted in gas furnace	Bur. Stds.....	F3	0.007	0.011	>0.001	0.004	Tr.	.....	99.977
C. F. Burgess	Electrolytic remelted in gas furnace	Bur. Stds.....	F4	0.009	0.010 0.009	>0.001	0.006	Tr.	0.006	99.968
J. R. Cain.....	Electrolytic remelted in gas furnace	Bur. Stds.....	F6	0.019 0.013 0.015	>0.001 by solu	Tr. tion	0.007	Tr.	>0.001	99.975
J. R. Cain.....	Electrolytic remelted in gas furnace	Bur. Stds.....	F7	0.014 First	>0.001 burnin	Tr. g 0.003	0.003 secon	Tr. d 0.006C	>0.001	99.971
C. F. Burgess	Electrolytic remelted *in vacuo*.....	Bur. Stds.....	25	0.003	0.006	0.004	0.004	0.000	.....	99.983
Langheim-Pfanhauser...	Electrolytic remelted *in vacuo*.....	Bur. Stds.....	26	0.001	0.004	0.006	0.007	0.000	.....	99.982
Langheim-Pfanhauser...	Electrolytic plates.....	Bnr. Stds.....	26a	0.008	0.004	0.001	0.006	Tr.	0.011	99.970
		H. Goldschmidt.....		0.00	0.00	0.007	0.00	0.00	0.008	99.98
C. F. Burgess	Electrolytic plates.....	Bur. Stds.....		0.009	0.003	0.001	0.006	Tr.	0.006	99.975
		C. F. Burgess	1	0.012	0.00	0.004	0.013	0.00	.....	99.971

Chemical Analyses.—The chemical analyses of the different preparations of iron used are shown in Table III, including mention of who furnished the analyses. Those at the Bureau of Standards were made by Messrs. Cain, Cleaves, Tucker, and Witmer. In all cases the analyses were made of the metal in the condition stated in the column headed "Description," and the pieces for analysis were cut from the cylinder or plate used for taking the heating and cooling curves. It will be noted that the purest iron is 99.983 Fe, obtained by remelt-

ing electrolytic iron *in vacuo* in magnesia crucibles. The two samples remelted *in vacuo* are also the lowest in carbon content, 0.003 and 0.001 per cent., respectively. The average carbon content of all samples is 0.009, and the range, from 0.001 to 0.015. It is of interest to note that with the exception of the Mn and Cu contents, the "ingot iron" is nearly as pure as the average of the electrolytic irons.

Where there are check analyses by different chemists, and in the case of the carbon determinations by different methods, when one considers the difficulty of exact determination of such small quantities of impurity, the agreement is extremely satisfactory, and in most cases may well be within the degree of homogeneity of the samples. None of the samples was analyzed for oxides, but their amount, if present, must be very small.

The Experimental Data.

We have divided the data into two chronological portions:

(1) A preliminary series of observations carried out with a furnace and rheostat of somewhat less satisfactory form than that of Figs. 5 and 6, with less precaution for holding a good vacuum and steady current, and in part with the iron samples in a less concentrated form; nor was the thermocouple calibration as carefully checked in all cases. These preliminary observations, although we do not place the reliance on all of them, for the exact location of temperatures, that we do on the definite series, are, nevertheless, quite instructive in showing some of the things that should be avoided in the exact thermal analysis of a substance occluding gases, and also furnish data for discussing the effects of varying some of the factors that influence the location of the critical ranges.

(2) The definite series of observations were then carried out with all the precautions and improvements in method and manipulation our preliminary measurements had shown to be necessary; it is on this definite series, together with the last two of the preliminary series, that we mainly base our conclusions regarding the allotropy of iron as shown by thermal analysis.

In all cases, attention should be given mainly to the location of the *maxima* of the critical ranges, as the beginnings and endings, especially the former on heating, are, in general, not to be located with exactness.

The Preliminary Observations have already been reported on briefly, but are here included in full for the sake of completeness. See Table IV. The numerical values here given are in some cases slightly different from those previously published, due to some of the indications of one of the thermocouples needing correction, Three Pt-Rh thermocouples, C_1, W_{11} and W_{14}, were used in these preliminary observations.

TABLE IV.—*Preliminary Series of Operations.*

Date.	Couple.	Sample No.	Mass in Grams.	Rate–Degree-Seconds. Up.	Rate–Degree-Seconds. Down.	*Beginning.* Ac_2	Ar_2	Ac_2–Ar_2	Ac_3	Ar_3	Ac_3–Ar_3	*Maximum.* Ac_2	Ar_2	Ac_2–Ar_2	Ac_3	Ar_3	Ac_3–Ar_3	*End.* Ac_2	Ar_2	Ac_2–Ar_2	Ac_3	Ar_3	Ac_3–Ar_3
7-25-11	W11	1	.....	0.194	0.138	.....	.....	.....	894	904	— 10	785	768	+ 17	944	896	+ 48	.....	.....	.....	957	879	+ 78
7-26-11	W11	1	.....	0.254	0.224	.....	776	.....	915	899	+ 16	812	754	58	936	896	40	829	724	+105	957	872	+ 85
7-27-11	W11	1	.....	0.100	0.189	766	773	— 1	906	903	+ 03	772	758	14	922	895	27	798	728	+ 75	935	879	+ 56
7-28-11	W11	1	.....	0.255	0.308	769	.....	.....	901	901	00	788	754	34	933	892	41	814	.....	.....	943	867	76
					Means:	768	774	— 2	904	902	+ 2	789	758	31	934	895	39	814	724	+ 90	948	874	74
11- 8-11	W14	16	15	.....	0.150	.....	776	.....	.....	902	.....	.....	754	+.....	.....	893	+.....	.....	711	.....	.....	883	.....
11- 8-11	W14	16	15	0.349	0.284	770	761	+ 09	885	915	— 30	796	739	57	901	887	14	813	714	99	908	863	+ 45
11- 9-11	W14	16	15	0.083	0.063	717	888	—171	902	904	— 02	773	766	7	918	898	20	785	.....	.....	926	888	38
11-11-11	W14	16	15	0.118	0.132	779	779	00	902	902	00	782	761	21	918	897	21	791	.....	.....	939	880	50
11-11-11	W14	16	15	0.284	0.221	773	761	+ 12	915	907	+ 08	791	755	36	930	892	38	817	.....	.....	944	878	66
11-13-11	W14	16	15	0.069	0.088	763	776	— 13	904	901	+ 03	769	764	5	916	898	18	789	.....	.....	924	889	35
					Means:	760	790	— 52	902	905	04	782	757	25	917	894	22	799	712	99	926	880	.....
11-21-11	W14	17	20	0.104	0.124	.....	.....	.....	.....	896	.....	767	756	+ 11	911	890	+ 21	.....	.....	.....	925	863	+ 62
11-21-11	W14	17	20	0.192	0.196	709	767	— 58	.....	894	.....	773	763	10	915	887	28	.....	.....	.....	935	.....	.....
11-23-11	W14	17	20	0.057	0.061	.....	.....	.....	.....	897	.....	770	767	3	913	893	20	775	.....	.....	921	.....	.....
11-27-11	W14	17	20	0.140	0.157	751	.....	.....	.....	897	.....	771	761	10	915	889	26	775	.....	.....	923	865	58
12- 4-11	W14	17	20	0.055	0.065	.....	.....	.....	.....	897	.....	.....	765	.....	.....	893	.....	.....	.....	.....	.....	875	.....
					Means:	730	767	— 58	.....	896	.....	770	762	8	913	890	24	775	.....	.....	926	868	+ 60
2-16-12	W14	22	.....	0.118	0.127	.....	781	.....	875	902	— 27	768	772	— 04	908	890	+ 18	782	742	+ 40	925	875	+ 50
2-19-12	W14	22	.....	0.150	0.124	737	.....	.....	893	895	— 02	768	772	— 04	913	887	+ 26	784	.....	.....	926	873	+ 53
2-19-12	W14	22	.....	0.319	0.256	746	784	— 38	893	893	00	768	772	— 04	913	881	+ 32	793	.....	.....	937	866	+ 71
2-21-12	W14	22	.....	0.160	0.202	756	794	— 38	893	896	— 03	768	772	— 04	913	887	+ 26	784	.....	.....	926	862	+ 64
2-23-12	W14	22	.....	0.175	0.153	.....	784	.....	893	902	— 09	768	772	— 04	913	887	+ 26	788	.....	.....	928	866	+ 62
2-26-12	W14	22	.....	0.137	0.148	756	784	— 28	893	893	00	769	772	— 03	913	885	+ 28	784	751	+ 33	928	866	+ 62
3- 5-13	W14	22	.....	0.066	0.062	746	784	— 38	893	902	— 09	768	772	— 04	913	892	+ 21	782	756	+ 26	924	878	+ 46
3-12-13	W14	22	.....	0.167	0.156	746	784	— 38	875	895	— 20	772	771	+ 01	917	887	+ 30	.....	.....	.....	932	866	+ 66
					Means:	748	785	— 36	888	897	— 09	769	772	— 03	913	887	+ 26	785	887	+ 26	928	869	+ 59
7-19-12	W14	25	31	0.121	0.181	.....	778	.....	909	905	+ 4	769	769	+ 0	918	900	+ 18	781	733	+ 48	931	887	44
7-22-12	W14	25	31	0.070	0.063	728	778	— 50	913	905	8	768	768	0	914	903	11	774	751	23	928	893	35
7-22-12	W14	25	31	0.247	0.289	.....	809	.....	913	904	9	768	768	0	918	900	18	779	747	32	929	884	45
7-29-12	W14	25	31	0.132	0.155	.....	821	.....	911	905	6	768	768	0	913	900	13	779	742	37	931	886	45
8- 5-12	W14	25	31	0.066	0.079	.....	772	.....	911	906	5	768	769	— 1	913	902	11	775	761	14	924	894	30
8- 6-12	W14	25	31	0.091	0.051	.....	773	.....	911	907	4	768	769	— 1	.....	904	.....	775	761	14	.....	893	.....
10-22-12	C1	25	31	0.078	0.126	.....	777	.....	909	906	3	769	770	— 1	915	904	11	775	.....	.....	929	894	35
					Means:	728	787	— 50	911	905	+ 6	768	769	0	915	902	14	777	749	28	929	890	.....
8-14-12	W14	26	21	0.217	0.248	.....	773	.....	907	909	— 2	768	768	0	911	904	+ 7	779	.....	.....	929	893	+ 36
8-15-12	W14	26	21	0.209	0.192	.....	826	.....	913	909	+ 4	765	768	— 3	918	904	14	779	733	+ 46	930	891	39
8-16-12	W14	26	21	0.096	0.142	.....	835	.....	912	910	+ 2	768	767	+ 1	915	905	10	773	737	36	926	895	31
8-16-12	W14	26	21	0.082	0.095	.....	.....	.....	913	909	+ 4	766	.....	.....	915	906	9	.....	.....	.....	925	897	28
8-21-12	W14	26	21	0.099	0.153	.....	826	.....	912	910	+ 2	767	768	— 1	915	907	8	774	742	32	925	895	30
8-28-12	W14	26	21	0.063	0.064	.....	907	.....	913	912	+ 1	768	768	0	915	908	7	775	747	28	925	899	26
8-29-12	W14	26	21	0.248	0.275	.....	819	.....	913	913	0	764	768	— 4	918	904	14	.....	.....	.....	928	891	37
					Means:	.....	831	.....	912	910	2	767	768	— 1	915	905	10	776	740	36	927	894	32

All but one of the samples of iron are electrolytic: three from Prof. C. F. Burgess, Nos. 1, 16, 17; sample No. 25 remelted *in vacuo* is from the same lot as No. 17; and No. 26 is from the Langheim-Pfanhauser A-G., also remelted *in vacuo*. The electrolytic plates were relatively thick, from $\frac{1}{8}$ to nearly $\frac{1}{4}$ in.

The mass of the samples ranged from 15 to 31 g., and for the first three series the iron sample consisted of more than one piece of metal. The first three, Nos. 1, 16, 17, were from the electrolyte plates deposited; Nos. 25 and 26 were remelted *in vacuo*, and No. 22 is ingot iron, remelted in a gas furnace, of similar quality as sample F1 of Table III.

Several facts stand out prominently from these preliminary observations. In the first place, in contrast to the samples remelted *in vacuo* before taking heating and cooling curves, which samples show the Ac2 and Ar2 maxima sharply defined at a single temperature, 768°, the untreated electrolytic samples show a wide and variable interval between the Ac2 and Ar2 maxima, an interval in some individual cases greater than 50°. With untreated electrolytic irons, Ac2 is always higher and Ar2 always lower than the single A2 point at 768° as found with the remelted irons. Again, the position of the maxima of Ac3 is higher and of Ar3 lower, with very considerable variations, for the untreated samples; or in other words the interval Ac3 — Ar3 (maxima) is considerably less, from 10° to 14°, for the samples reheated *in vacuo* than for the untreated samples, for which this interval ranges from 20° to 40°.

These results show the important rôle played by the occluded gases, and an examination of the data of the tables shows that several reheatings to 1,050° *in vacuo* are not sufficient to put thick electrolytic iron plates into a satisfactory condition for making a thermal analysis of their critical ranges. When there are several pieces making up a sample (as Nos. 16 and 17) the retarding effect of poor conduction also enters somewhat.

Remelting appears from these observations to be an essential preliminary treatment to which the iron should be subjected unless several rates are used, as we shall show below. We desire to emphasize this point, for we believe that one of the main reasons why many of the previously published results with electrolytic iron show inconsistencies and variations in the location of A2 and A3, is due to the fact that the gases have never been sufficiently removed, or the samples were not in a compact enough form, or both effects superposed, and sometimes associated with a too wide temperature interval between observations.

A property that appears to be common to all these samples is that, on the average, for A3 the *beginning* of Ac3 is at the same temperature as the *beginning* of Ar3, within the limit of accuracy with which these somewhat indefinitely defined temperatures can be located. This characteristic sharply distinguishes A3 from A2; for the latter, as we have seen, the maxima of Ar2 and Ac2 coincide when the samples are compact and gas-free.

As to the effect of rate of heating or cooling upon the location of the critical ranges, it will be noted, Table IV, that for the rates here used, 0.06° to 0.35° per second (or 17 to 3 sec. per degree), there is no effect on the location of A2 (max.) for the samples 25 and 26 premelted *in vacuo*, or for sample 22, ingot iron remelted in gas furnace, while for the untreated samples a fast rate accompanies a greater difference in Ac2–Ar2. For the A3 range the effect of rate is less pronounced for the untreated samples but appears to be appreciable for the premelted samples. Table V shows that reducing the rate to zero gives 769° for A2 (= Ac2 = Ar2) for all samples; and for A3 we find for zero rate Ac3 — Ar3 = 12°, with Ac3 = 912° and Ar3 = 900° on the average.

TABLE V.—*Maxima of Critical Ranges for Zero Rate.*

Preliminary Series.

Sample.	Ac3.	Ar3.	Ac3–Ar3.	Ac2.	Ar2.	Ac2–Ar2.
1	914	900	14	768	767	1
16	913	900	13	769	769	0
17	911	896	15	770	769	1
22	909	895	14	769	772	— 3
25	912	903	9	769	769	0
26	913	909	4	768	768	0
Means :	912	900	12	769	769	0

It therefore appears that in spite of the wide variations noted in Table IV, the nature of the sample, whether or not gas-free or whether in one or several pieces, does not influence the location of the maxima of the critical ranges when reduced to zero rate of heating and cooling.

The Definite Series of Observations is shown in Table VI, in which are recorded all the observations taken with the improved apparatus of Figs. 4, 5, and 6 on the samples here included. There are three samples of electrolytic iron prepared by J. R. Cain, F2, F′, F7; an electrolytic sample, F3, prepared by Langheim-Pfanhauser; another, F4, from Prof. C. F. Burgess; and one of ingot iron, F1, all of which were remelted in a gas furnace before taking observations. In addition, F8 and F9 are small samples from Nos. 25 and 26 respectively of the preliminary series; and F5 is a piece from an electrolytic sample which had been studied thermally by Professor Carpenter.

TABLE VI.—*Definite Series of Observations.*

Date.	Couple.	Sample No.	Mass in Grams.	Rate-Degree-Seconds. Up.	Rate-Degree-Seconds. Down.	Run No.	*Beginning.* Ac_2	Ar_2	Ac_2-Ar_2	Ac_3	Ar_3	Ac_3-Ar_3	*Maximum.* Ac_2	Ar_2	Ac_2-Ar_2	Ac_3	Ar_3	Ac_3-Ar_3	*End.* Ac_2	Ar_2	Ac_2-Ar_2	Ac_3	Ar_3	Ac_3-Ar_3
5-19-13	M1	F2	33	0.143	0.155	1	.....	776	.....	868	891	— 23	767	765	+ 2	911	876	35	774	736	38	923	849	74
5-20-13	M1	F2	33	0.101	0.131	2	748	776	— 28	858	891	— 33	769	767	2	912	879	33	774	739	35	922	858	64
5-27-13	M1	F2	33	0.121	0.093	3	.....	808	.....	874	892	— 18	768	767	1	912	882	30	775	729	46	922	.....	.....
5-27-13	M1	F2	33	0.138	0.128	4	.....	808	.....	876	889	— 13	768	768	0	912	880	32	775	736	39	923	858	65
5-28-13	M1	F2	33	0.053	0.063	5	.....	808	.....	893	893	00	768	768	0	912	883	29	772	748	24	919	863	56
5-29-13	M1	F2	33	0.075	0.083	6	.....	779	.....	.....	894	.....	768	768	0	912	881	31	773	744	29	922	858	64
6- 5-13	M1	F2	33	0.102	0.092	7	.....	819	.....	.....	.....	.....	768	768	0	.....	.....	.....	775	741	34	.....	.....	.....
6- 5-13	M1	F2	33	0.115	0.078	8	.....	775	.....	.....	.....	.....	768	768	0	.....	.....	.....	775	.....	.....	.....	.....	.....
6- 5-13	M1	F2	33	0.131	0.222	9	.....	.....	.....	.....	.....	.....	768	767	1	.....	.....	.....	774	.....	.....	.....	.....	.....
6- 7-13	M1	F2	33	0.088	0.100	10	755	.....	.....	.....	891	.....	768	.....	.....	911	880	31	773	.....	.....	920	855	65
				0.068	0.107	10	.....	790..	.....	892	889	— 03	.....	767	.....	910	879	31	.....	.....	.....	919	858	61
6-14-13	M1	F2	1.5	0.128	0.090	1	.....	804	.....	.....	888	.....	768	768	0	910	880	30	772	767	05	921	849	72
							752	794	.....	877	891	.....	768	769	+ 1	911	880	31	774	742	.....	921	856	.....
6-11-13	M1	F3	32	0.130	0.125	1	.....	776	.....	891	891	00	768	768	0	914	880	34	776	.....	.....	920	858	62
6-11-13	M1	F3	32	0.151	0.143	2	.....	776	.....	890	890	00	768	768	0	914	882	32	776	.....	.....	922	863	59
6-12-13	M1	F3	32	0.053	0.064	3	.....	775	.....	858	894	— 36	768	768	0	911	886	25	771	.....	.....	920	880	40
							.....	776	.....	880	892	— 12	768	768	0	913	883	30	774	.....	.....	921	867	.....
7- 8-13	M1	F4	37	0.164	0.144	1	762	775	— 13	894	894	00	768	768	0	916	886	30	776	743	33	925	868	57
7-10-13	M1	F4	37	0.051	0.062	2	751	771	— 20	885	897	— 12	768	769	— 1	912	890	22	771	753	18	921	879	42
7-14-13	M1	F4	37	0.136	0.081	3	759	773	— 14	897	895	+ 02	768	768	0	914	890	24	776	750	16	920	8 77	43
							757	773	— 16	892	896	— 03	768	768	0	914	889	25	774	749	.....	922	875	.....
7-17-13	M2	F5	0.7	0.131	0.107	1*	.....	769	.....	911	904	+ 07	768	766	+ 2	912	888	24	772	753	19	921	893	28
7-17-13	M2	F5	0.7	0.057	0.054	2	.....	769	.....	912	903	+ 09	767	766	1	910	889	21	771	756	15	920	895	25
							.....	769	.....	912	904	+ 08	768	766	1	911	888	23	772	754	.....	920	894	.....
7-31-13	M2	F7	13	0.147	0.125	1	.....	776	.....	876	892	— 16	768	766	+ 2	912	888	24	774	748	26	916	876	40
7-31-13	M2	F7	13	0.103	0.105	2	.....	774	.....	872	895	— 23	768	766	2	910	889	21	773	.....	.....	919	872	47
							.....	775	.....	874	894	— 20	768	766	2	911	888	23	774	748	.....	918	874	.....
7-23-13	M1	F1	35	0.177	0.112	1	.....	785	.....	880	894	— 14	768	766	+ 2	916	884	32	774	748	26	925	858	67
							.....	785	.....	867	896	— 29	768	768	0	911	889	22	772	748	24	920	867	3
7-24-13	M1	F1	35	0.057	0.043	2	.....	785	.....	874	895	— 21	768	767	1	914	886	28	773	738	.....	922	862	.....
8-11-13	M2	F8a	1.0	0.064	0.095	1	.....	785	.....	907	901	+ 06	768	766	+ 2	913	898	15	773	.....	.....	921	890	31
8-13-13	M2	F9b	2.1	0.099	0.101	1	.....	786	.....	912	905	+ 07	768	766	+ 2	916	908	13	774	.....	.....	925	897	28

* By Bureau of Standards.

a Piece of No. 25, which had been run.

b Piece of No. 26, which has been run.

Considering first the existence and location of the maxima of the A2 range, the observations on all these nine samples show most emphatically the existence of a common maximum for Ac2 and Ar2 at 768°, with agreement of 1° among the samples and as between the Ac2 and Ar2 maxima. This result is identical with that found for the samples Nos. 25 and 26, reheated *in vacuo*, of the preliminary series, and for the others of this series when reduced to zero rate. This constancy of (maximum of) Ac2 = Ar2 = 768° C. ± 0.5 is seen therefore to persist for variations in mass from 0.7 to 35 g., in rate of heating or cooling from 0.05° to 0.35° per second, for iron remelted *in vacuo* or gas furnace, whether of electrolytic or "ingot" preparation, and as determined in two differently constructed furnaces in terms of five separately calibrated thermocouples by two methods of thermal analysis.

The small, electrolytic sample, F5, from Professor Carpenter, which had been heated by him six times to 1,000° C., also behaves exactly like the others.

Turning now to the maxima of A3, we see that for all the samples Ac3 is located at 912° ± 2, while the agreement for Ar3 is apparently not so good, the range being from 880° to 903° for the different samples and the mean value of Ar3 is 889°. It is to be noted that the samples F8 and F9 preheated *in vacuo* are in agreement with Nos. 25 and 26 of the preliminary series, again showing the smallest interval, namely, Ac3 — Ar3 = 14°.

As in the preliminary series, the beginning of Ar3 is located, in general, at about the same temperature as the beginning of Ac3, although these beginning temperatures are not defined with sufficient sharpness to warrant certainly the statement that the A3 transformation *begins* at the same temperature on heating and in cooling, although this appears highly probable.

No effect of rate of heating or cooling can be seen in the location of the critical range A2. For the critical range A3, when the observations on Ac3 and Ar3 are plotted in terms of rate of heating or cooling (see Fig. 7) the anomalies in location of Ar3 largely disappear, although extrapolating Ac3 and Ar3 to zero rate again fails to give a single equilibrium temperature Ac3. For zero rate, Ac3 = 909° and Ar3 = 897°, or there is apparently a real difference of some 12° in the location of the maxima of this transformation, or in exact agreement with the preliminary series.

Considering both series, it seems safe to say that we may place Ac3 = 909° ± 1 and Ar3 = 898° ± 2 and Ac2 = Ar2 = 768° ± 0.5 for pure iron passing through A3 and A2 at zero rate, and through

A2 at any rate provided the iron is gas-free, and in the form of a single compact piece.

Description of the Plotted Curves.

An examination of the actual heating and cooling curves is also essential. For most of the samples both inverse-rate (Marked I), and derived-differential (marked D) heating and cooling curves are recorded. Observations were usually taken at 2° intervals, although others were sometimes used—*i. e.*, 1° and 4° steps—which are evident from inspection. For all curves the ordinates represent temperatures centigrade; for the inverse-rate curves the abscissæ represent time in seconds to change the temperature of the sample by the unit interval (1°, 2° or 4°); and for the derived-differential curves the abscissæ represent deflections on the differential galvanometer scale expressed in millimeters.

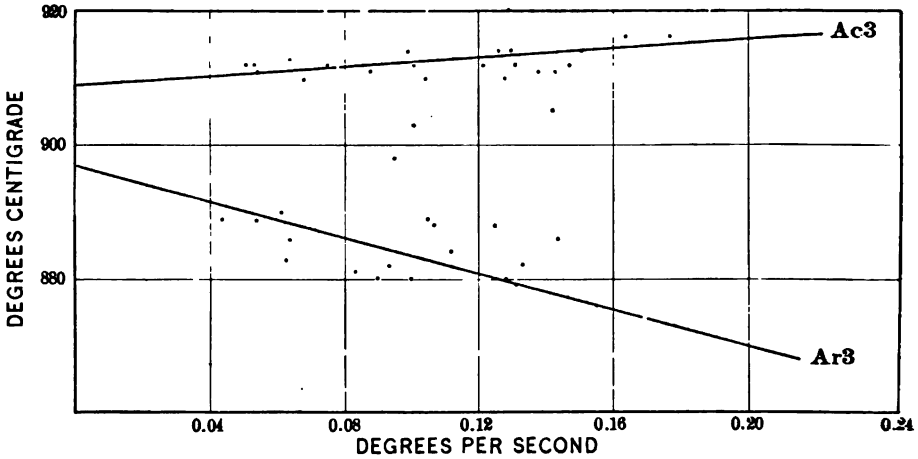

Fig. 7.—Temperature vs. Rate for A3 Range, Definite Series. Samples F2, F2′, F3, F4, F5, F7, F1, F8, F9.

The Effect of Plotting at Different Intervals is shown in Plate III for the observations of a single heating and cooling curve of sample A1. It would appear that there is no advantage in reducing the interval at which temperatures are taken to as small as 1° in the desire not to miss any very slight transformation; on the contrary, the sensibility is thereby reduced proportionally, since what is measured is only the effect between two successive taps of the key. On the other hand, with wide intervals such as 4° missing a single observation may be sufficient to throw the whole series into doubt. The interval chosen, 2°, appears to avoid satisfactorily the two pitfalls of lack of sensibility and danger of missing the cusp of a transformation.

The Curves of the Definite Series are shown in Plates IV to VIII. The most detailed study was made of sample F2, consisting of 33 g. of premelted electrolytic iron prepared by Mr. Cain. In contrast to the observations by ourselves and others on untreated electrolytic iron, which show irregular curves for the first three or four heatings, with this premelted iron the first curve is about as regular and sharply defined as the succeeding ones. The similar character of the two types of curve is well shown here, the only considerable difference being that for very slow and for rapidly changing rates the inverse-rate curve shows marked departures from the vertical. It is apparent from an inspection of the curves that although the maxima and to a less degree the ends of the critical ranges are sharply defined, the exact location of the beginnings, especially on heating, is attended with considerable uncertainty for both types of curve.

The curves of Plate IVA show the results of an effort to attenuate or eliminate, if possible by various operations, the A2 point. As the curves indicate, it is impossible to influence the position or nature of Ac2 or Ar2 by any thermal operations performed upon the iron above or below this temperature. Thus during the sixth heating, the sample was held 71 min. at 825°, heated to above A3 and then cooled; in the seventh it was heated to just below the beginning of A3 and then cooled; in the eighth and ninth it was heated and cooled twice over a short range, including the A2 point; in the tenth it was held 60 min. just below A3 and then carried up and down three times over the A3 range before cooling. In all these operations, Ac2 and Ar2 remain unchanged in any way, and the evidence is abundantly conclusive that the critical range A2 is in no sense related to the A3 transformation, as has been suggested it might be, particularly by Benedicks.

A small piece (1.5 g. = F2′) of the above sample was also examined with the same results (Plate IV).

The observations with several other samples of electrolytic iron, F3, F4, and F7, premelted in the gas furnace, are shown in Plates V, VII, and VIII, corroborating the previous observations. It will be seen that in each case the first heating gives as definite results as the succeeding ones. A sample of 35 g. of remelted ingot iron, F1 (Plate VII), gives the same type of curve as the remelted electrolytic irons.

A sample of particular interest is F5, it being 0.7 g. of Professor Carpenter's electrolytic iron, which is seen (Plate VI) to behave exactly like the others, showing Ac2 and Ar2 both clearly located on the curves at 768°.

The advantage of using small samples for sharply defining the magnitude of the critical ranges is well shown for the curves of F5 (0.7 g.), F8 (1.0 g.), and F9 (2.1 g.), Plates VI and VII, the last two being pieces remelted *in vacuo*. These very small samples were also inclosed within the platinum neutral in the furnace, a proceeding which appears to be advantageous, since the sample F2′ of 1.5 g. not so inclosed (Plate IV) shows the A3 range much less sharply.

The Curves of the Preliminary Series are not all presented here, but a sufficient number are included to indicate the constancy of behavior attained with the variously prepared samples with the unperfected apparatus and at times with an unsteady heating current. All the observations taken of one of the electrolytic samples premelted *in vacuo* (No. 26) are given and this series of curves (Plates X, XA, and XB) gives a fair example of them all. Some of the untreated electrolytic samples give somewhat erratic results, as shown for sample No. 1, Plate XI. The curves for the ingot iron, sample No. 22, Plates IX and IXA, are also representative of the results obtained with the earlier apparatus.

The Existence of Other Critical Ranges.—Several observers have announced the existence of transformation points in addition to A2 and A3, notably by Sir Roberts-Austen, who considered he had found several points below A2; by Carpenter, one at 675° which Rosenhain later attributed to the heating tube; by Arnold, a "fourth recalescence point" between A3 and A2 (or a doubling of A2); by Robin, two critical regions below A2 near 400° and 100° respectively; and by P. Curie, Broniewski, and others, one or more points above 1,000°.

A few of our observations extend to as low as 300° and as high as 1,050° without indicating the presence of any critical regions above A3 or below A2.

Some of the curves, particularly on cooling, give slight evidence of a minute transformation at about 805° (see in particular, several of the cooling curves for sample F2), although with the other samples this point can hardly be said to be present. If a point between A3 and A2 is really present, this would corroborate the magnetic observations both of Curie and of Weiss and Foëx, whose susceptibility curves show an inflection between A2 and A3 (see Fig. 2).

There is more positive evidence of a slight absorption of heat in the heating curves corresponding in temperature to the maximum of Ar3. Although not always present in the heating curves, this doubling of Ac3 is well shown, mainly for the inverse-rate curves, in the following of the definite series: Sample F7, both curves; the curves

of very slow rate of sample F2, especially the fifth; F4, all curves; F3, F1, and F8, slightly evident; while this doubling is apparently absent in F9 and F5 (Carpenter's sample), both of very small mass.

Some of the curves of the preliminary series also show one or another of these secondary points (see Plates IXA and XB) usually more marked in the inverse-rate curves, but, due to the relative irregularity of the preliminary series, as compared with the definite series using the perfected apparatus, we do not place any considerable reliance on the former for this purpose.

In view of the apparent impossibility of finding experimentally a common equilibrium temperature Ac3 for the heating and cooling curves, the presence of an inferior, even if slight, transformation on heating at the temperature of Ar3 would be an aid in explaining this anomaly, although the allotropy of iron would thereby be further complicated,—a step we are by no means anxious to champion, unless the facts are considered sufficiently convincing.

MICROSCOPICAL EXAMINATION.

(By H. S. Rawdon.)

Figs. 1 and 8 of Plates II and IIA. which are micrographs of a sample of electrolytic iron prepared by J. R. Cain, Bureau of Standards, illustrate the appearance of the metal upon deposition. The iron is quite different in its properties from iron under ordinary conditions. It is quite brittle and hard, and the samples for microscopic examination are prepared with much less difficulty than after the iron has been melted. This difference is undoubtedly due to the hydrogen which saturates the metal as a result of its method of preparation.

All of the other specimens examined show some spherical gas bubbles, due to the liberation of the dissolved gases during the processes of melting, and which did not escape from the iron an account of its viscosity. These bubbles have a peculiar appearance under vertical illumination, resembling inclusions of MnS. This is due to the absence of all oxide film from the walls of the bubble. Fig. 5 shows this appearance of the bubbles somewhat. These bubbles occur in all the specimens regardless of their method of melting, and also very small ones in samples which were heated for the cooling-curve determination without previous melting. Fig. 3 of the photomicrographs is interesting in showing the interior veinings in the iron crystals which appear upon deep etching after considerable heating.

All of the photomicrographs, unless otherwise stated, are at a magnification of 100 diameters. Picric acid was used as an etching agent.

Summary and Conclusions.

In the first part of this paper, after a brief statement of the current theories of the allotropy of iron, we have given what we hope will be considered an impartial account of the numerous experimental efforts of others to locate and define the critical ranges A2 and A3 of pure iron in terms of the various physical phenomena which have been found to change with temperature.

These observations are embodied in Tables I and II, from which it is seen that all the physical properties of iron which have been studied, with the single notable exception of crystallographic structure, have shown, in the hands of one or more skillful experimenters, a distinct discontinuity for the A2 range as well as for the A3 range. For several of the phenomena, such as electrical resistance, thermoelectricity, specific heat, and magnetism, it would appear that the discontinuity is at least as great for A2 as for A3, while the thermal effect has of course been long recognized as being much the more pronounced at A3.

The investigation proper consisted in taking *in vacuo* some 130 heating and cooling curves by two methods simultaneously, the inverse-rate and differential, for 15 samples of pure iron prepared by various methods and analyzed by several chemists. Unusual precautions were taken to secure uniformity of heating of the samples, and it was also possible to take observations for samples of widely different mass and over a wide range of rates, each maintained strictly constant. Two furnaces were used, and temperatures were taken with six separately calibrated thermocouples. The observations show that in order to get consistent and reliable results in the thermal analysis of a substance such as iron, the properties of which are so readily susceptible to many minor influences, it is necessary to get rid of all the disturbing influences.

We find essential the following precautions:

1. The iron should be pure; our purest samples were 99.983 and contained 0.003 per cent. carbon or less; and it should be kept pure by heating it only *in vacuo;* a pressure of 0.01 mm. of mercury suffices.

2. Either the occluded gases should be removed by premelting the iron, preferably *in vacuo*, or it will in general be found necessary to take a series of heating and cooling curves at widely different rates, in order to determine correctly the location of the critical ranges A3 and A2.

3. The iron should be in a single piece entirely surrounding and in contact with the thermocouple junction, otherwise the thermocouple

will integrate the irregular progress of the heat through the sample and the curves will lose their sharpness; small samples (1.0 g. or so) give sharper results than large samples.

4. The interval of recording temperatures should be wide enough that sufficient sensibility is attained, and narrow enough that the contour of the curves is not distorted; we have found a 2° interval satisfactory.

5. The sensibility of the apparatus indicating temperatures and differences of temperature should be of the order of 0.01° and time should be measured to better than 0.2 sec.

Among the results of this investigation may be mentioned the demonstration that the inverse-rate and differential methods, the latter plotted as the derived-differential curve, give identical results of the same sensibility for the critical ranges, and the two sets of curves are strictly similar, save for minor particulars.

The plotted curves give what seems to us conclusive evidence of the independent existence of A3 and A2, all of the 130 curves without exception showing both these critical ranges sharply defined and unquestionably distinct. It was found impossible to eliminate or attenuate A2 by thermal treatment. The A2 transformation has not a double cusp, nor do there appear to be other transformations above A3 and below A2 between 300° and 1,050°.

With electrolytic iron, unless the sample has been premelted into a compact mass, erratic results will be obtained for both A3 and A2, the location of the critical points apparently depending in the main upon the rate of heating or cooling. The critical points may even be displaced by over 50°. (See Tables IV and VI.) It is possible, however, to reduce the observations on untreated electrolytic iron to exactly the same temperature basis as the gas-free, compact material by taking curves at several rates and reducing to zero rate. This would appear to indicate that the gases occluded, mainly hydrogen, play no essential chemical rôle in modifying the iron equilibrium.

Even with gas-free iron the A3 point is not entirely independent of the rate of heating or cooling, so that it is necessary to reduce the observations to zero rate in order to obtain correct results for Ac3 and Ar3. The range in the location of Ar3, for example, with premelted samples was found to be 876° C. at 0.155 deg-sec. to 897° C. at zero rate. (See Fig. 7.)

All preparations of pure iron, even those containing gases, when reduced to the common basis of zero rate of heating or cooling, have the same maximum for the A2 critical range, namely, $A2 = Ac2 = Ar2 = 768° + 0.5$. All but one of the 15 samples gave this result to within 2°, the other to 3°.

Similarly for zero rate, the values of the maxima of A3 are found to be Ac3 = 909° ± 1 and Ar3 = 898° ± 2.

It was not possible to infer a single equilibrium temperature Ae3 from these experiments, the Ac3 transformation on heating always being at a higher temperature than Ar3, the transformation on cooling.

It was found, however, that the beginning of Ac3 coincides in temperature with the beginning of Ar3 as closely as could be judged. It is as if the crystallographic change at A3 required, so to speak, a temperature inertia to complete itself both on heating and on cooling, although the effect of rate on the equilibrium (see Fig. 7) appears to be slightly the greater on cooling.

It is possible that the A3 tranformation is somewhat more complex than this, as there are indications from some of the heating curves of a doubling of Ac3, although not of Ar3. This effect may be fortuitous.

The relative amounts of heat accompanying the two transformations, A3 and A2, are approximately 3 to 1 respectively.

An examination of some of the heating curves will perhaps give the incorrect impression that Ac2 is an evolution rather than an absorption of heat. The swing back at the maximum is very abrupt, following what appears to be a gradual building up of this maximum from an indeterminate low temperature. This behavior would be in accordance with the gradual change in certain physical properties, such as magnetism and electrical resistance, as A2 is approached. On the other hand, the operations carried out on sample F2 (Plate V) failed to diminish sensibly the intensity of Ac2, which would imply that this thermal transformation is limited to a narrow temperature interval; also some of the curves of Ac3 show that this long back swing appears to be in part at least a property of the heating conditions. The shading off on cooling through Ar2, if this effect is a real one, might be masked by a similar swing back from this peak, and therefore be indistinguishable.

We hesitate to express an opinion on the nature of the allotropy of iron. These thermal observations appear to show that the transformations A3 and A2 are of the types illustrated in Fig. 8. The fact that A2 appears to be accompanied by no crystallographic change, such as accompanies A3, requiring a violent rearranging of relatively large crystal masses and involving a considerable quantity of heat, may account for the sharpness with which Ac2 equals Ar2, and the A2 transformation may be merely molecular, not involving the crystallographic structure as such. Whether we have any β iron or not

to inhabit the region between A2 and A3 will depend on our definition of allotropy, but we hope that we have proved beyond a reasonable doubt that under standard conditions there is a definite transformation at 768° and a less well defined although more intense one at 898° to 909° in terms of their maxima on cooling and heating.

In conclusion, we take great pleasure in acknowledging the samples of iron and analyses from Prof. C. F. Burgess, Prof. H. C. H. Carpenter, Dr. H. Goldschmidt, J. R. Cain, and the American Rolling Mill Co., and the co-operation of the chemical division of this Bureau in making check analyses. The care and skill with which H. Scott has assisted in taking and reducing the observations has greatly expedited the investigation, and we are indebted to H. S. Rawdon for the photomicrographic examinations.

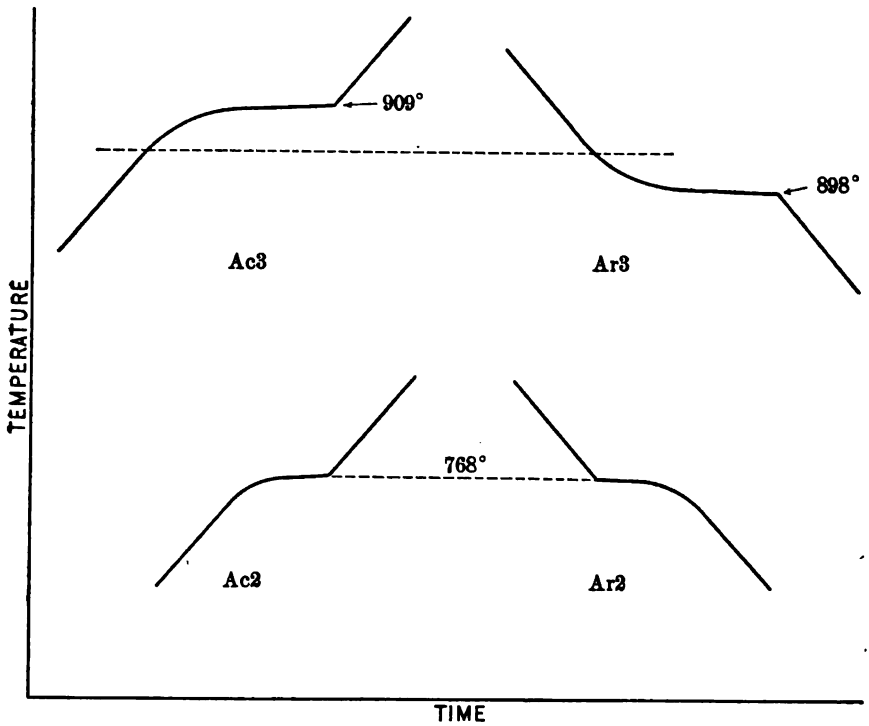

FIG. 8.—TYPES OF TRANSFORMATION IN IRON.

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This list of references to the literature of the critical points of pure iron makes no pretense of being complete. Many of the papers dealing mainly with the theoretical and controversial aspects of the subject are omitted, but it is believed that a sufficient number of titles

concerning the experimental aspects of the subject are included to enable any one readily to get in touch with it. The papers are grouped in terms of the principal phenomenon studied by the authors.

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PLATE I.—PHOTOGRAPH OF APPARATUS.

Fig. 1.—Cain's Electrolytic Iron as Deposited. Section is perpendicular to the electrode.

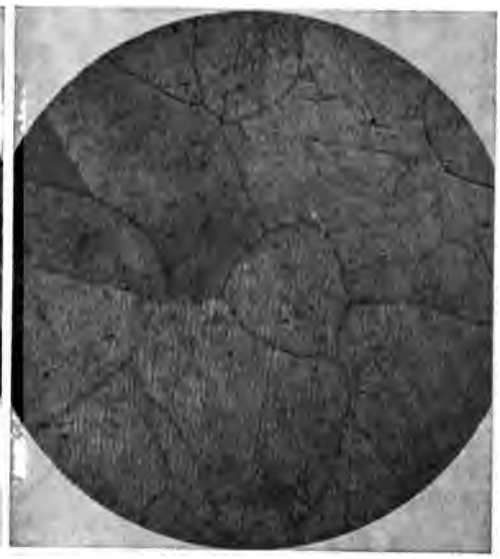


Fig. 2.—C. F. Burgess's Electrolytic Iron heated to 1,050° several times but not remelted.

Fig. 3.—The same at 500 ×. Small bubbles are seen at this magnification.

Fig. 4.—Cylinder A. Remelted from American ingot iron in the gas furnace.

PLATE II.—PHOTOMICROGRAPHS.

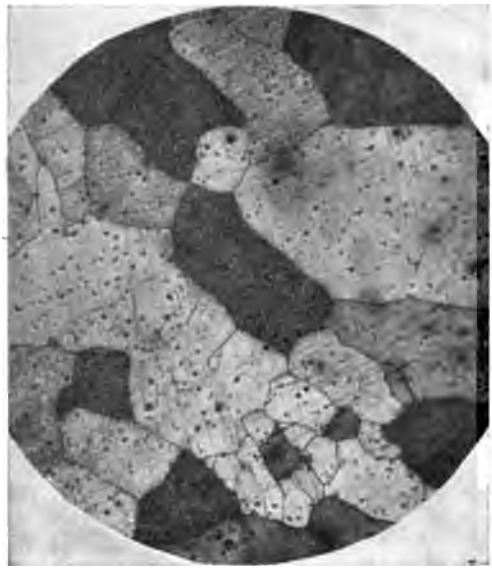


Fig. 5.—Cylinder Ag. Cylinder from Cain's electrolytic iron, remelted in gas furnace.

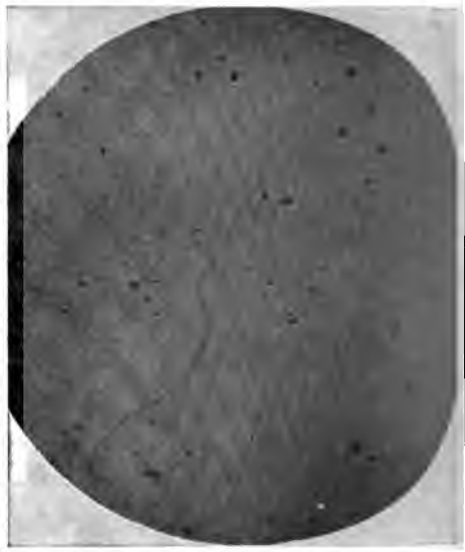


Fig. 6.—(Langheim-Pfanhauser) Cylinder No. 26, remelted in Arsem furnace *in vacuo*.

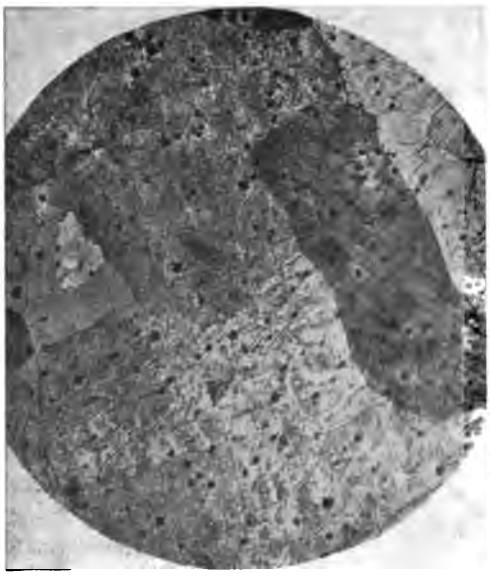


Fig. 7.—Cylinder Ag, Langheim-Pfanhauser, remelted in gas furnace.

Fig. 8.—Same as Fig. 1, but 250 diameters.

PLATE IIA.—PHOTOMICROGRAPHS.

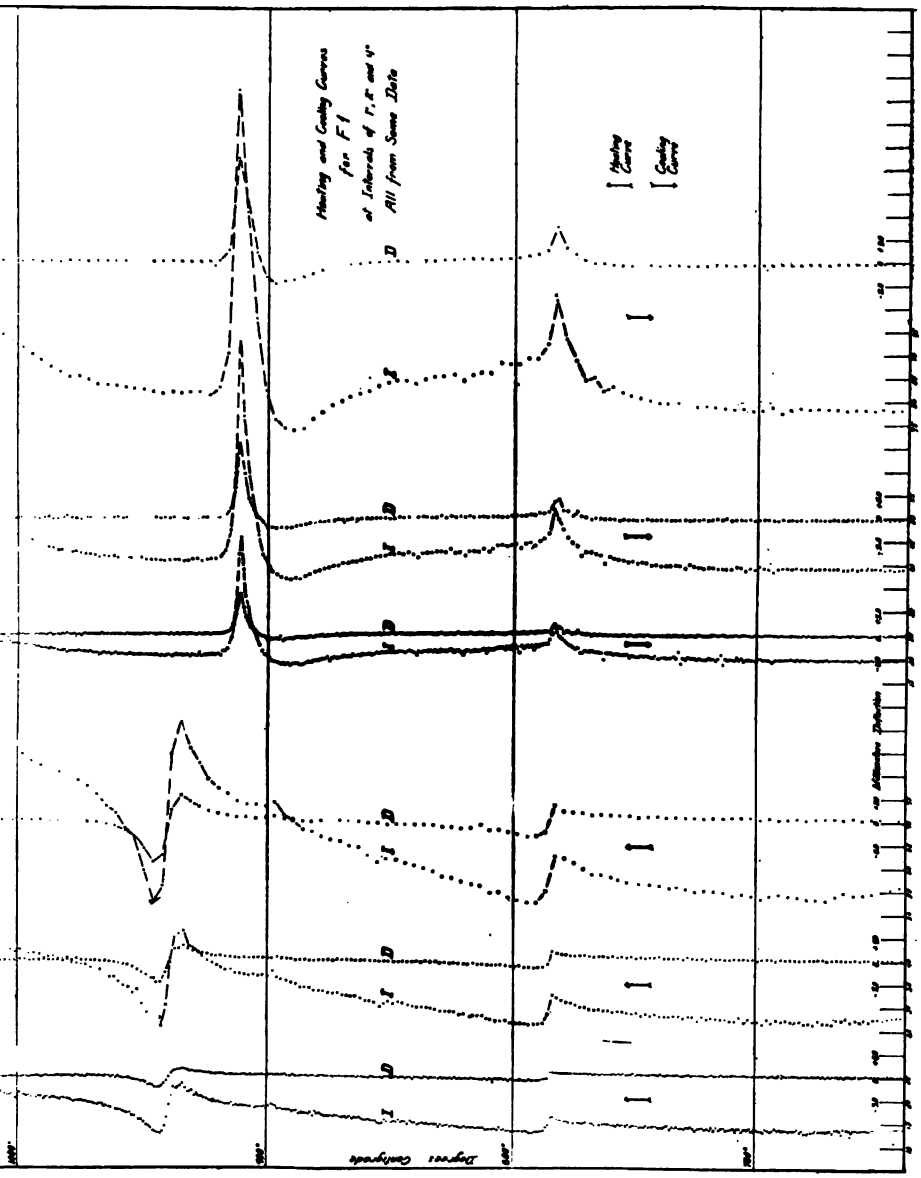
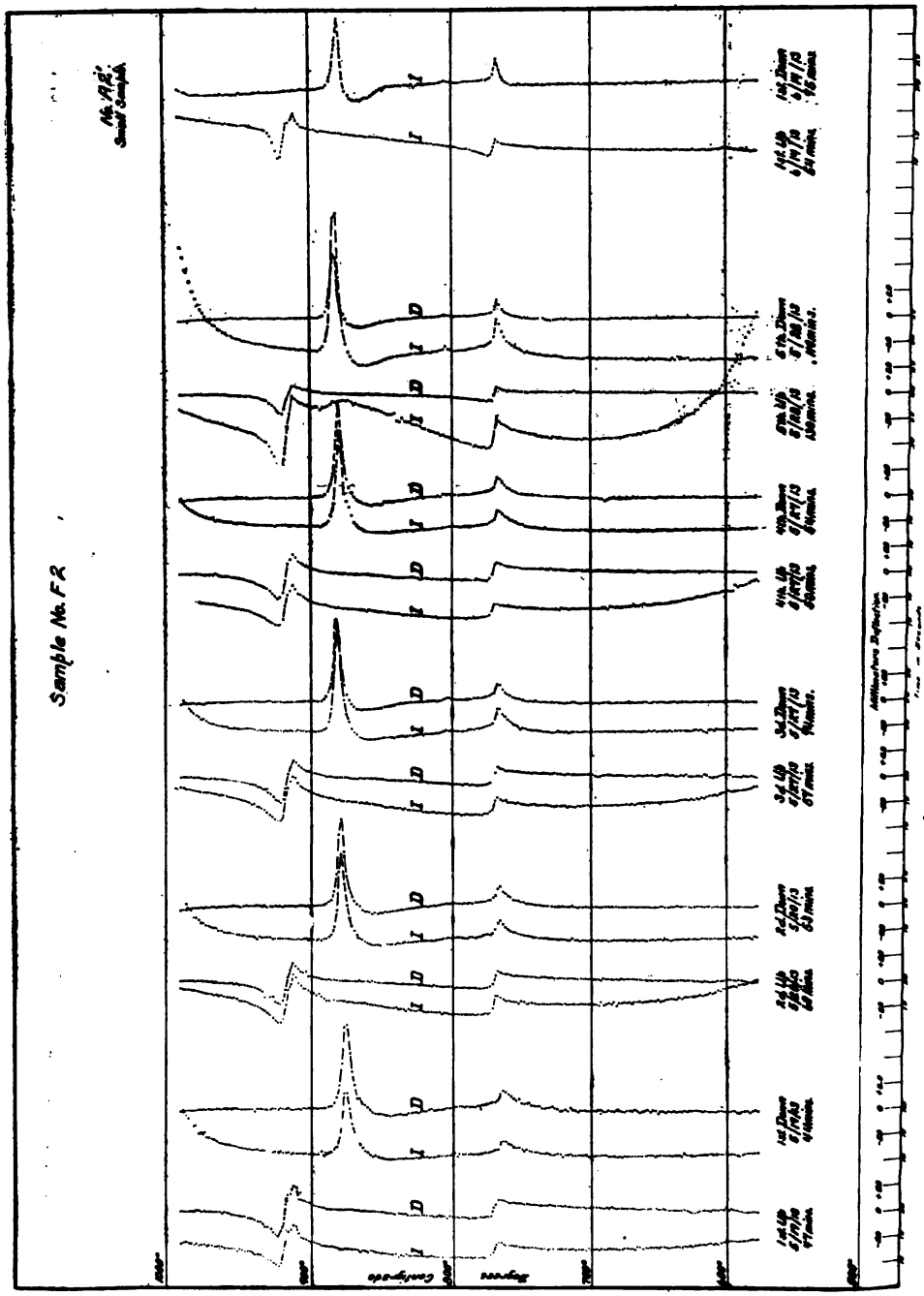


PLATE III.—EFFECT OF DIFFERENT INTERVALS, SAMPLE F1.


Sample No. F2
No. 'A2'
Small Sample
Millimeters Deflection
Degrees Centigrade

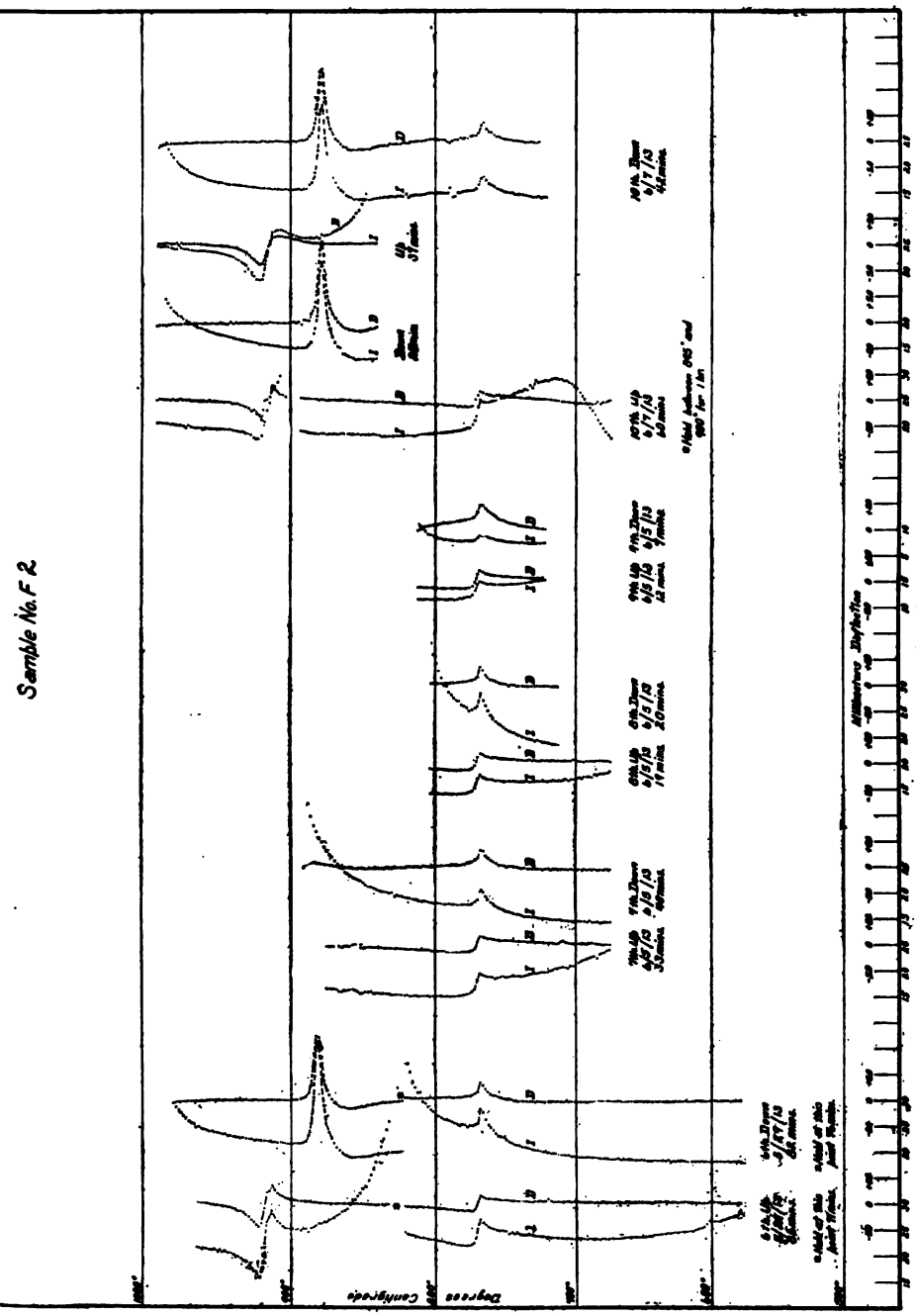
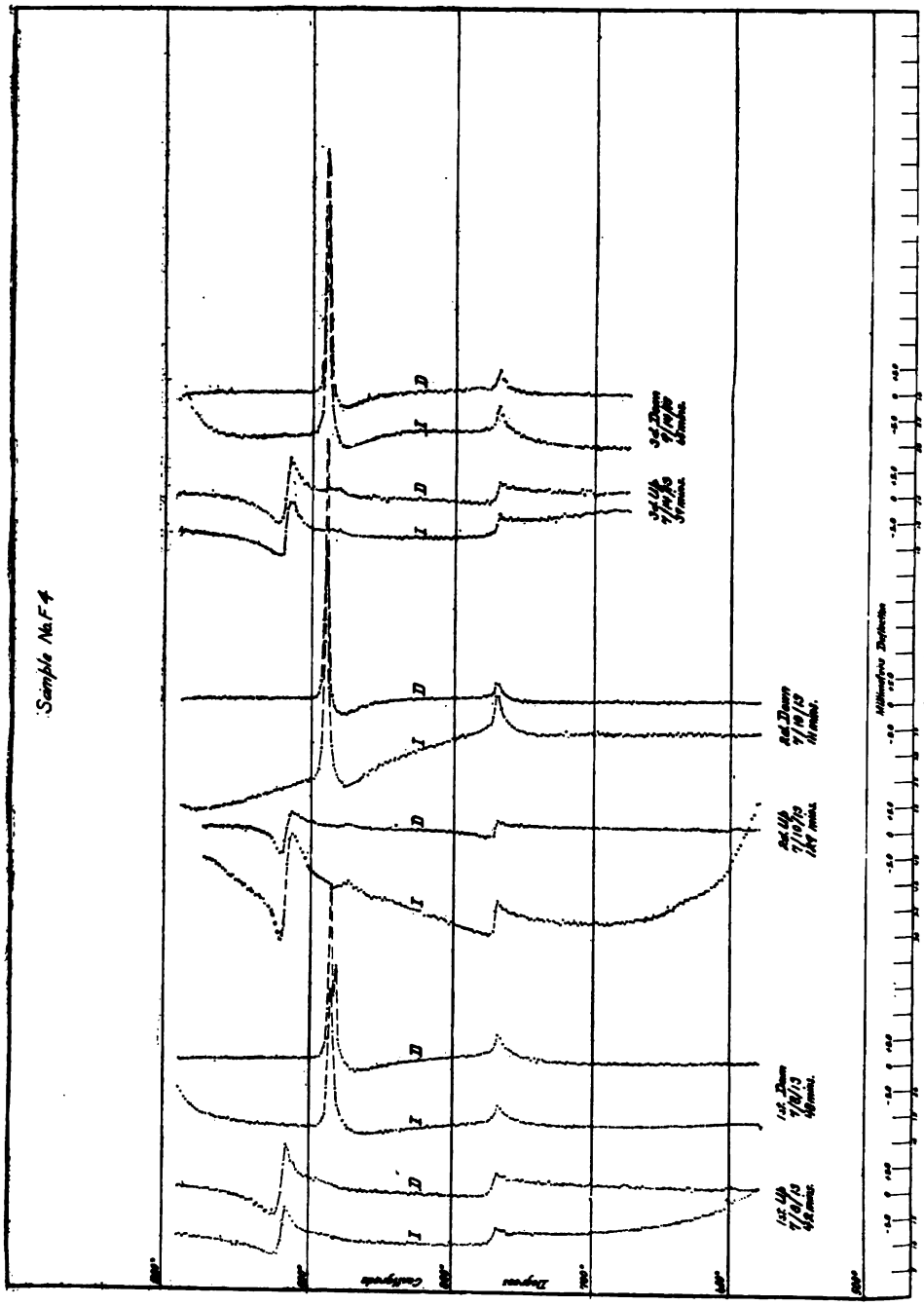


PLATE IVA.—CURVES FOR SAMPLE F2.


Sample No. F 4
Millimeters Deflection
Degrees
Centigrade

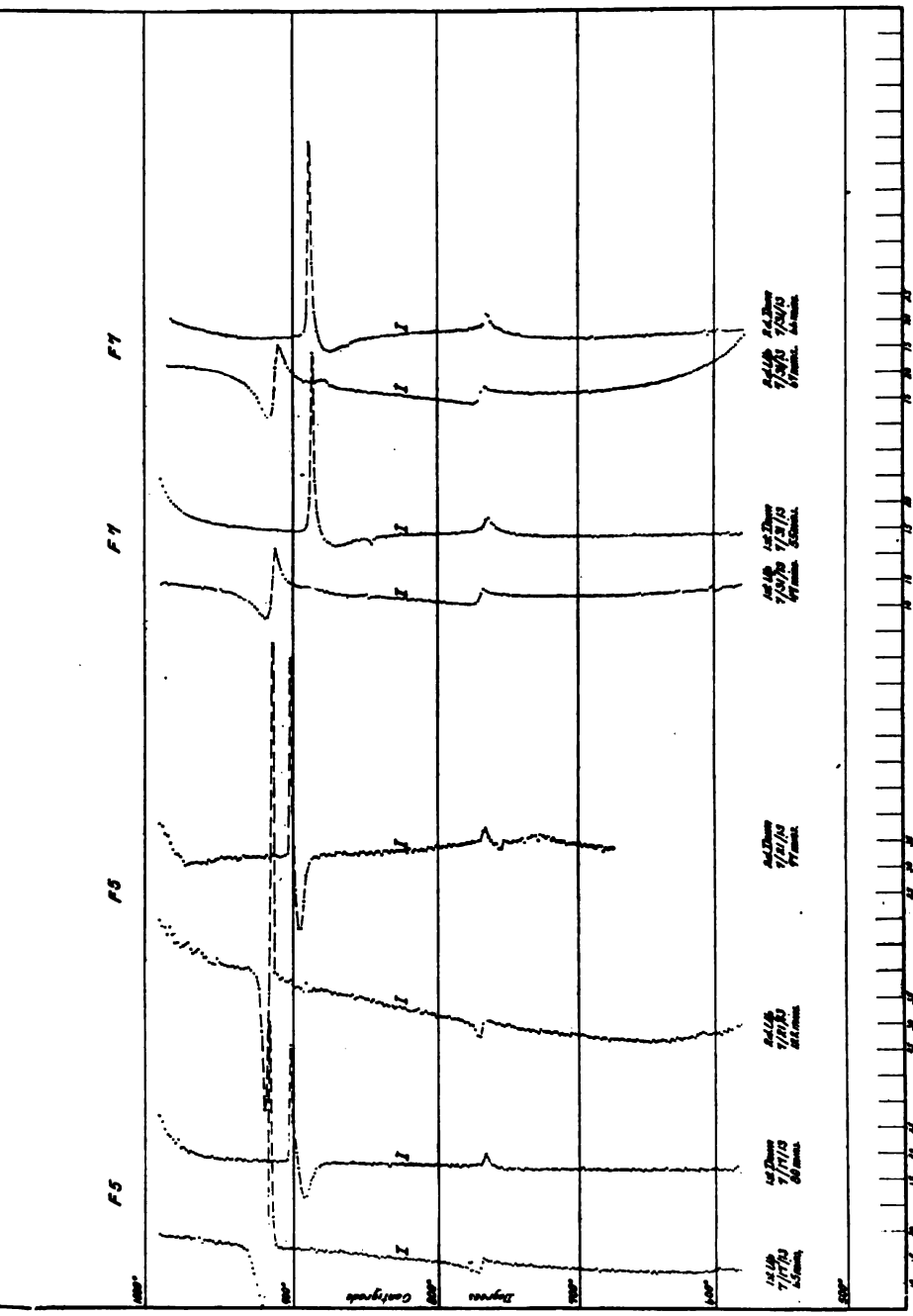


PLATE VI.—CURVES FOR SAMPLES F5 AND F7.

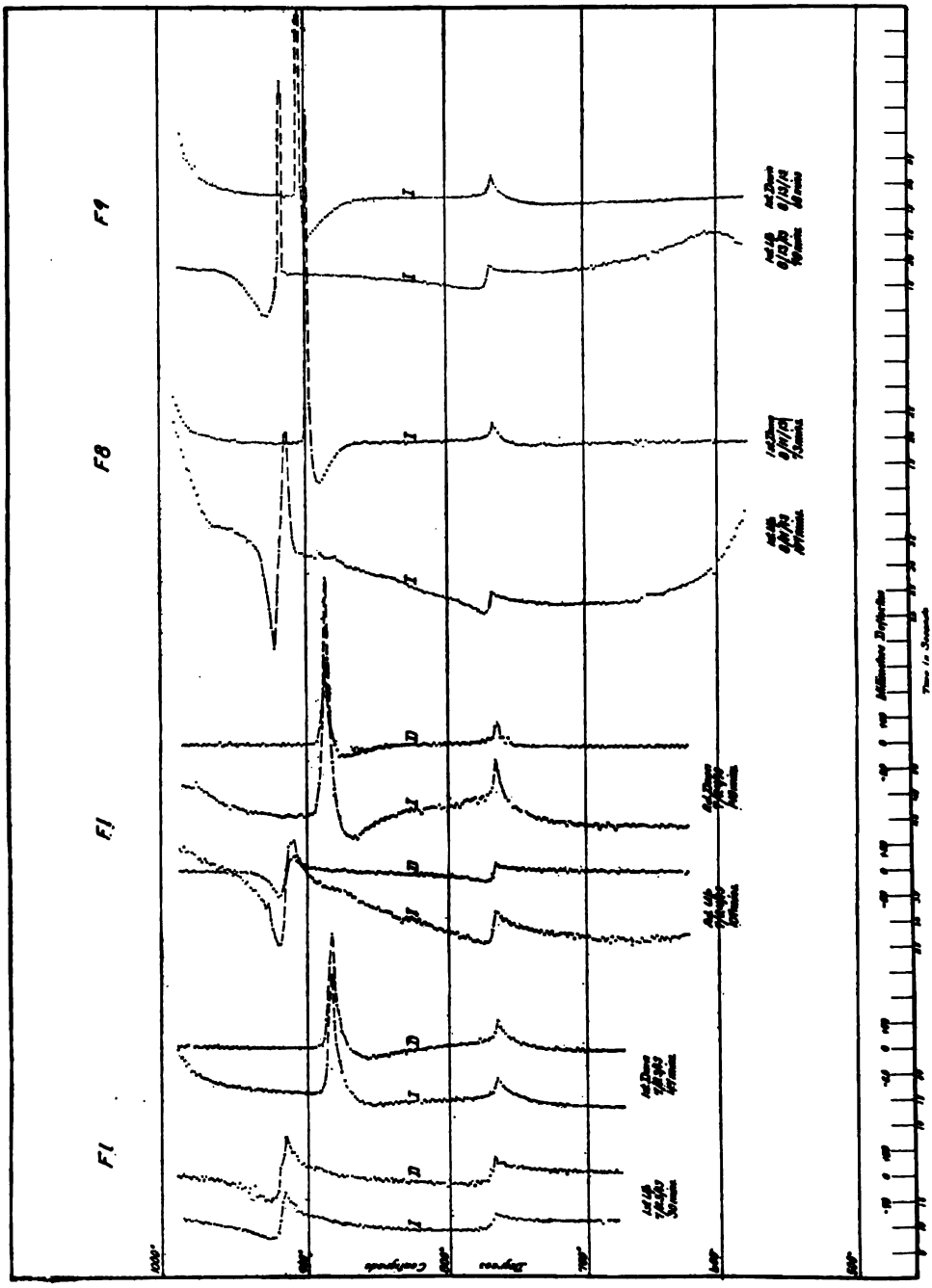


PLATE VII.—CURVES FOR NAMPLES F1, F8, AND F4.

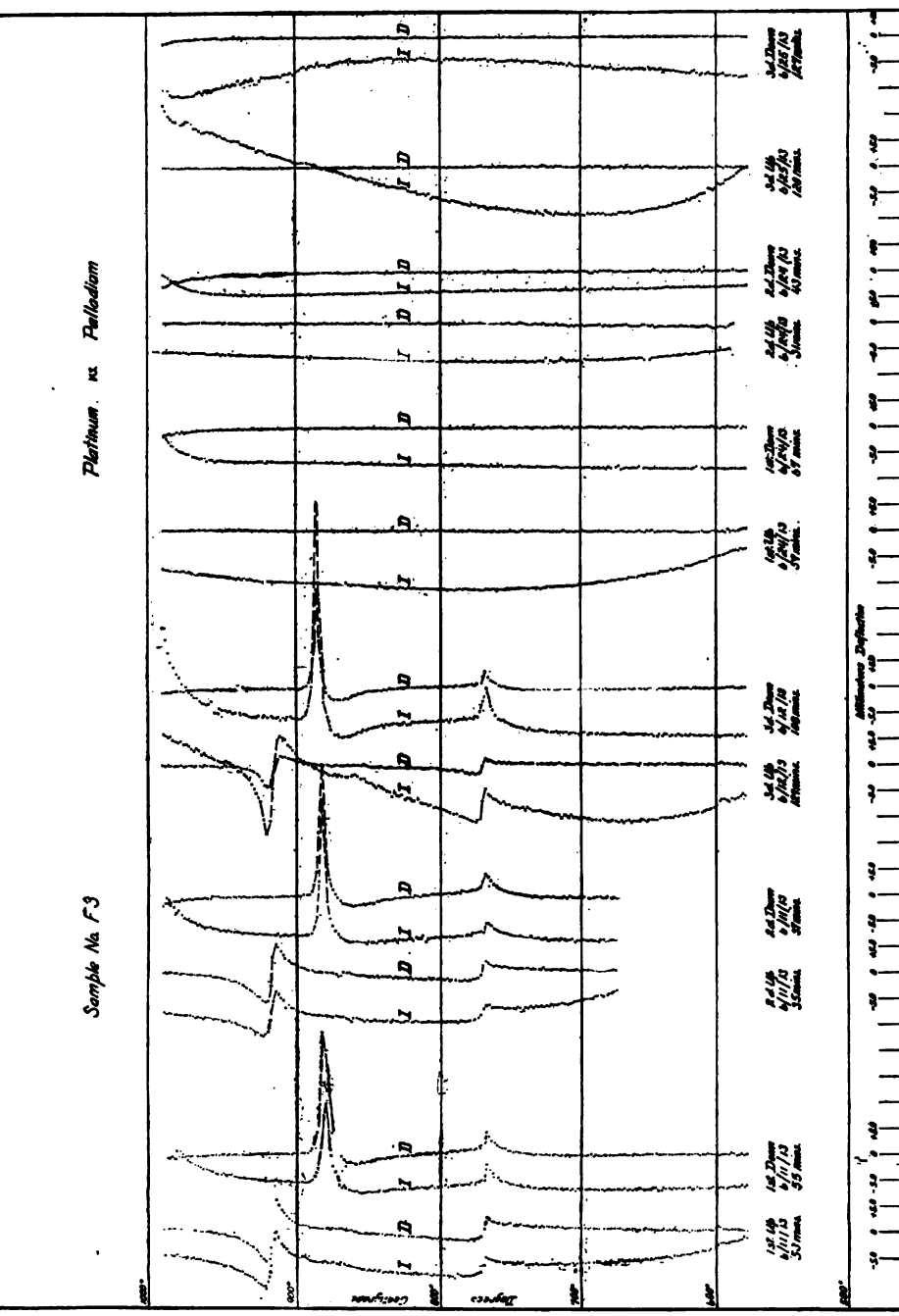
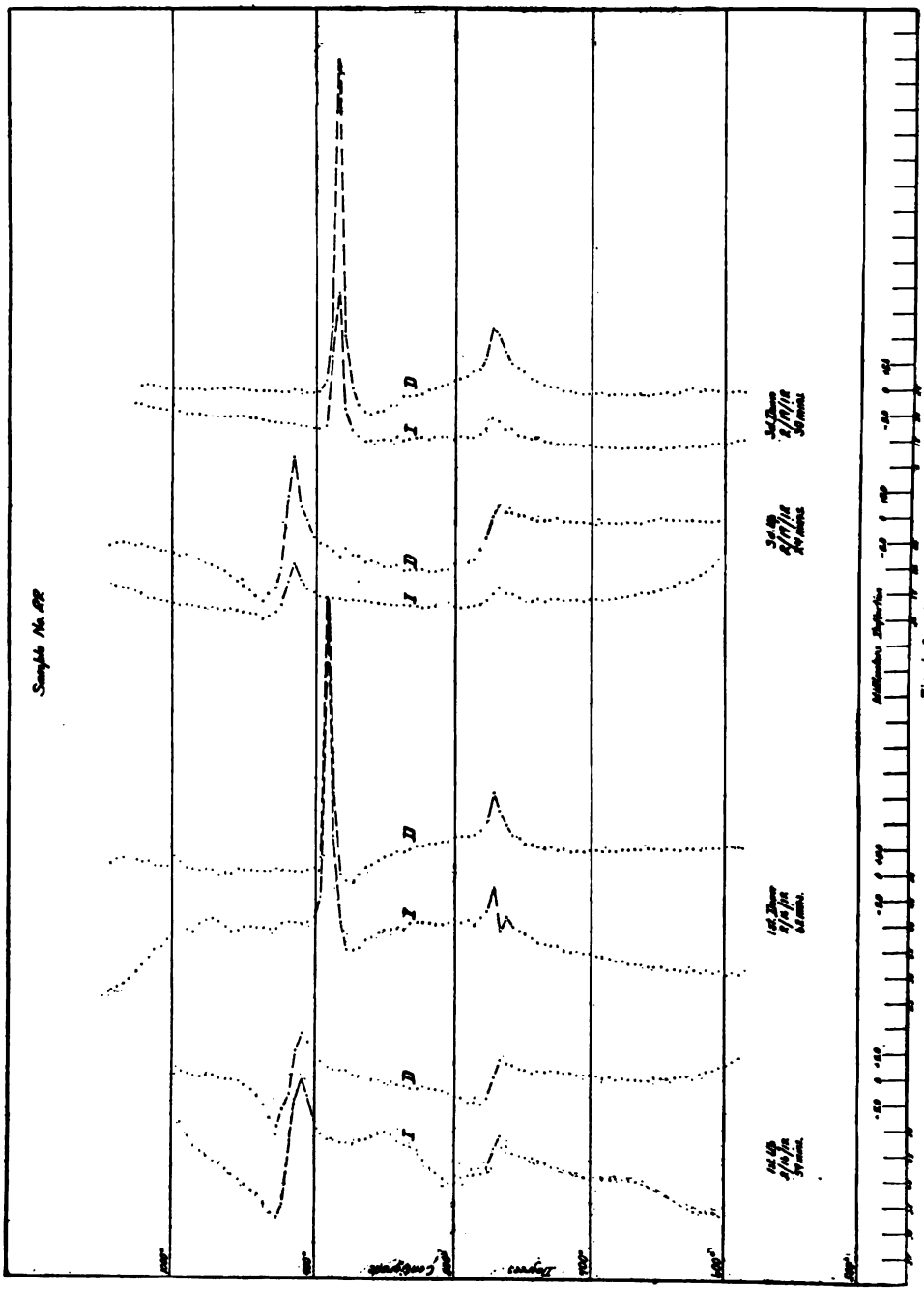


PLATE VIII.—CURVES FOR SAMPLE F3 AND PT-PD BLANK.



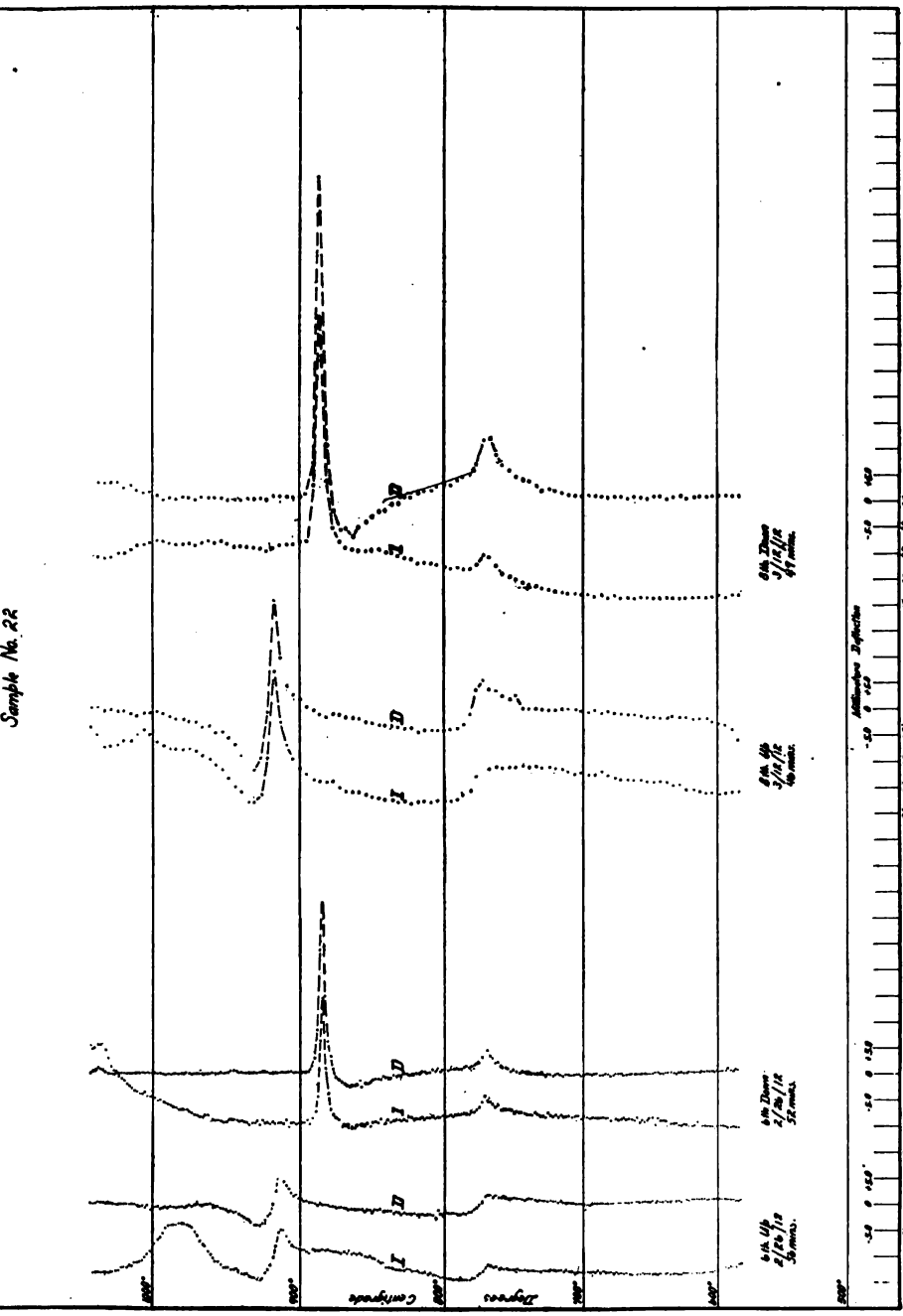
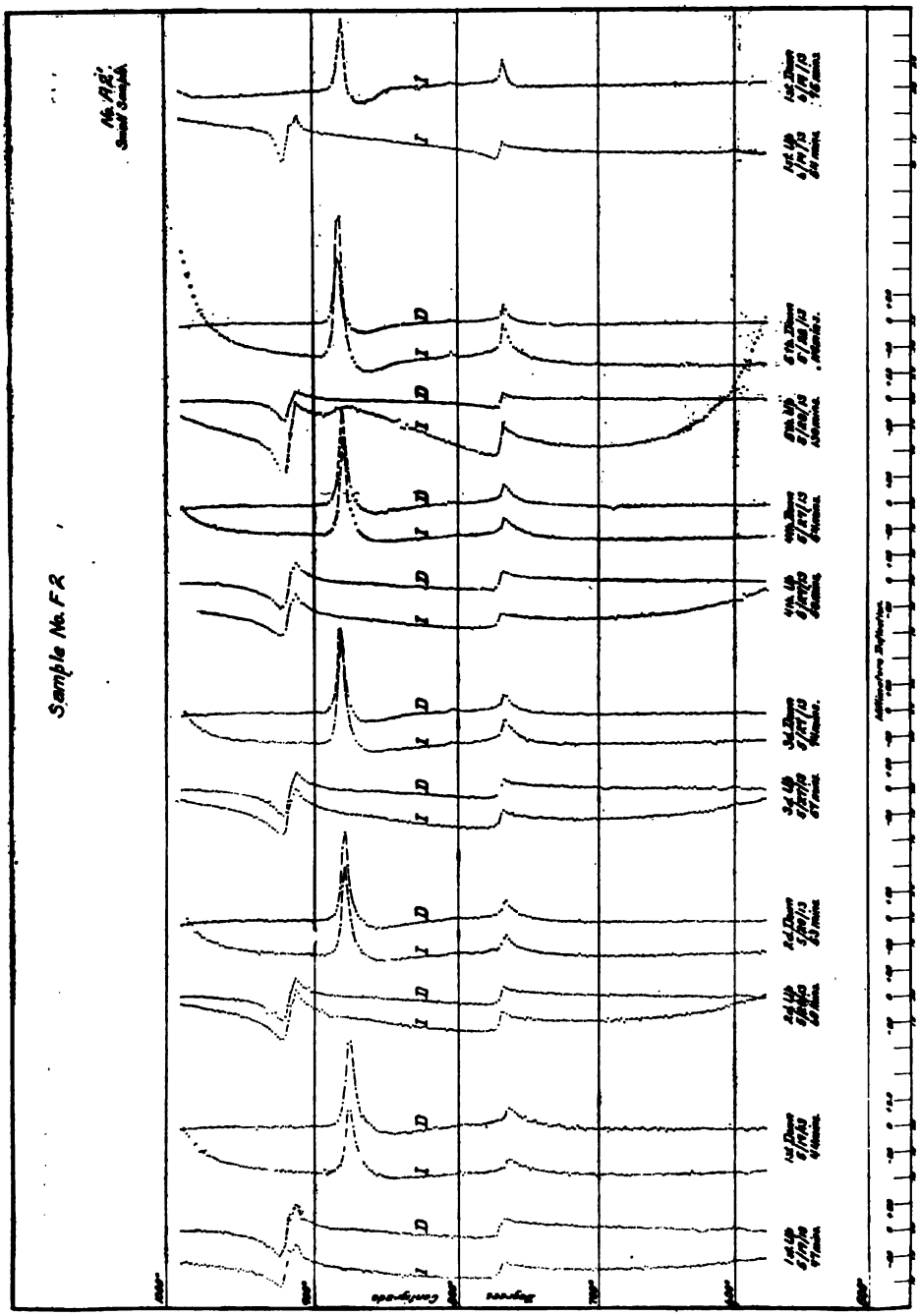


PLATE IXA.—CURVES FOR SAMPLE 22.


Sample No. F.R
No. A.E.
Small Sample.

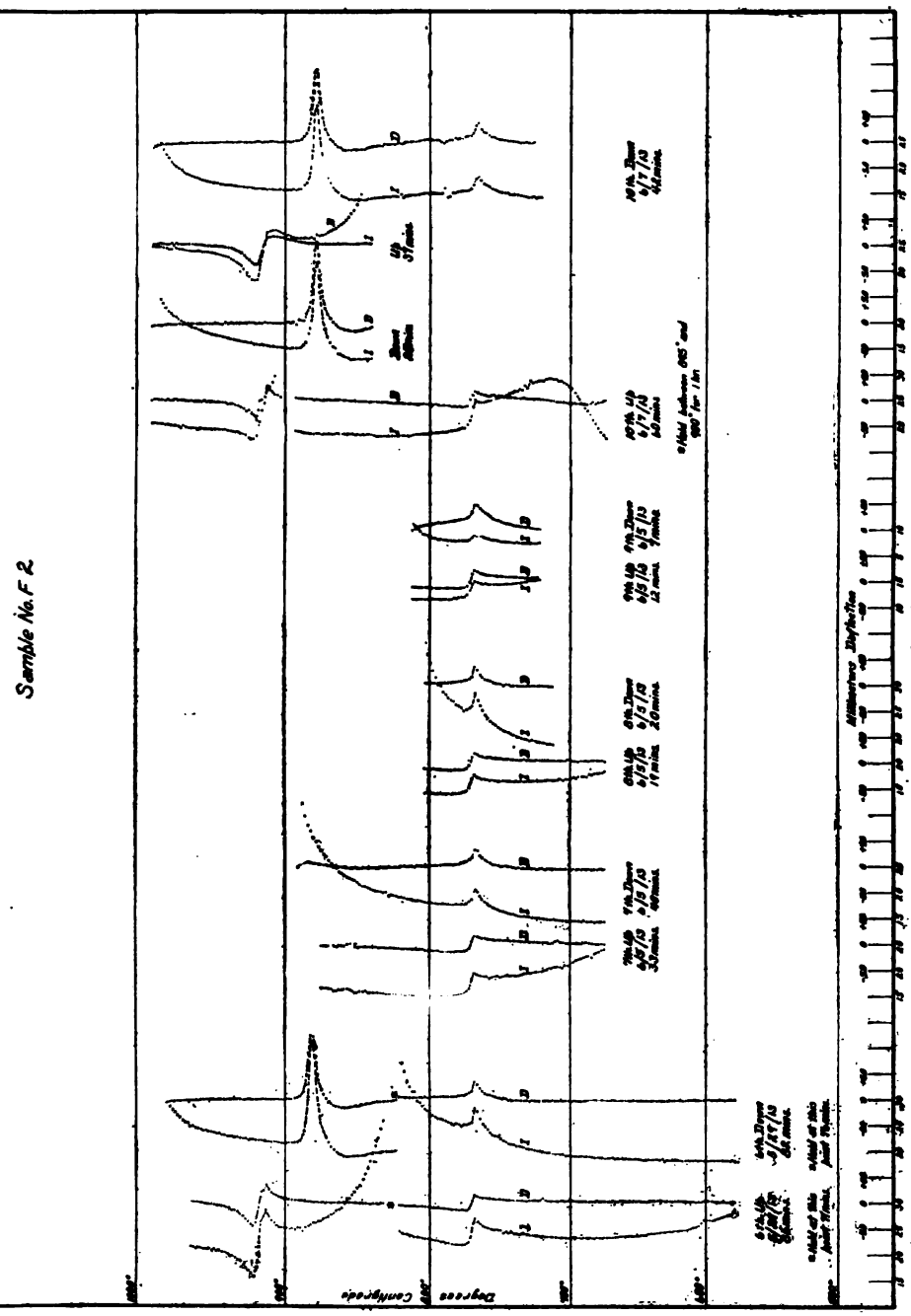
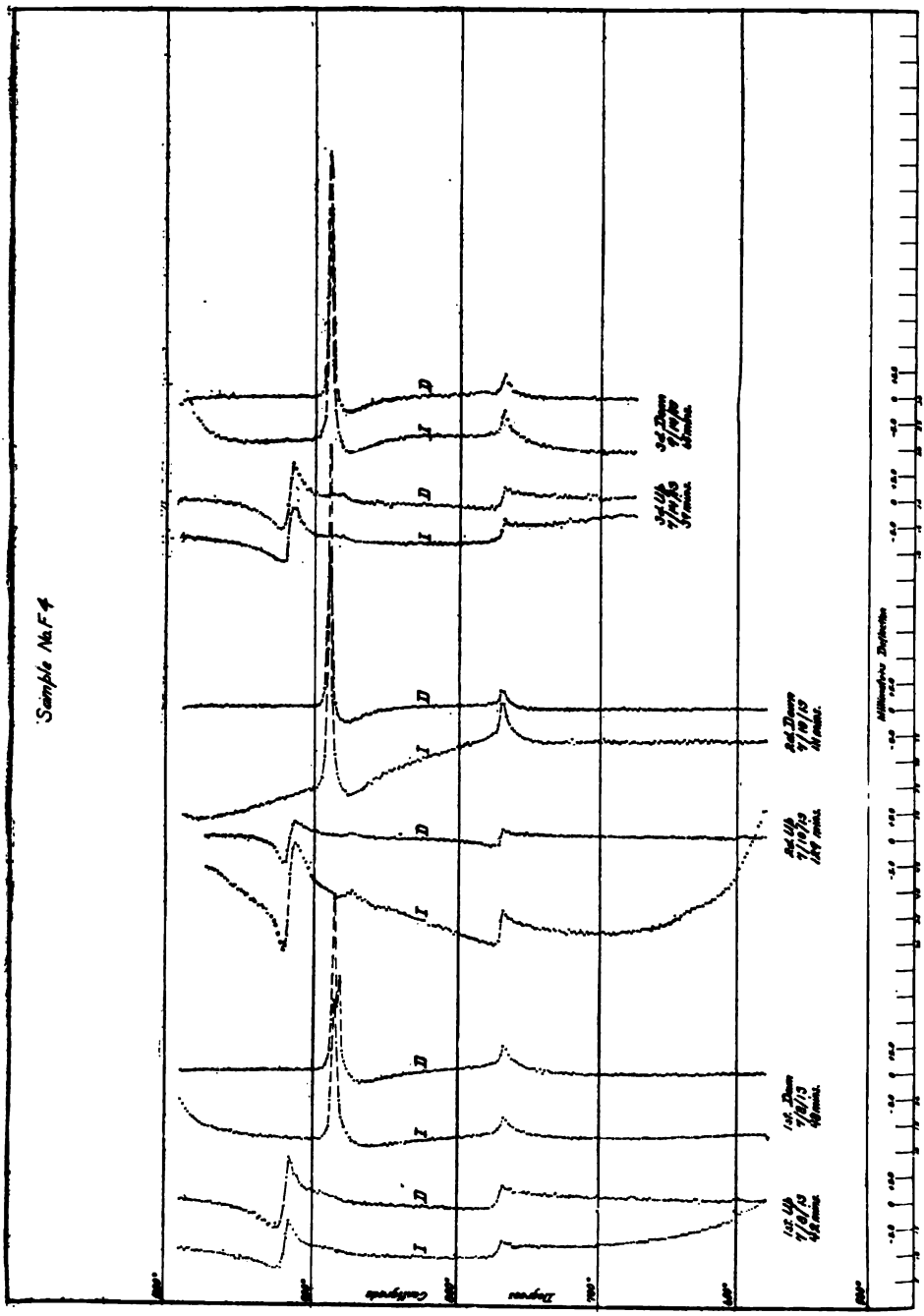


PLATE IVA.—CURVES FOR SAMPLE F2.


Sample No. F 4
Millimetres Deflection

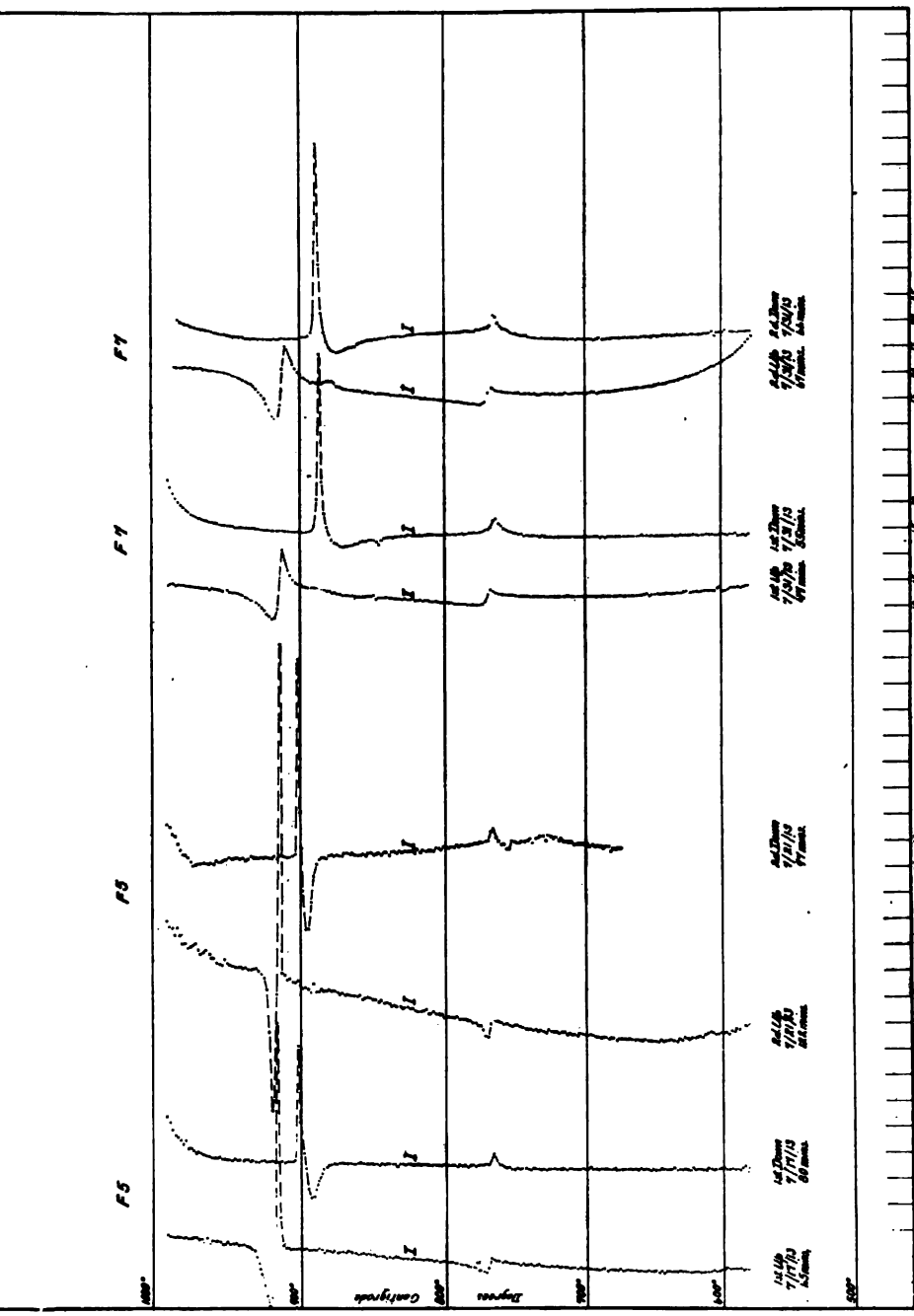


PLATE VI.—CURVES FOR SAMPLES F5 AND F7.

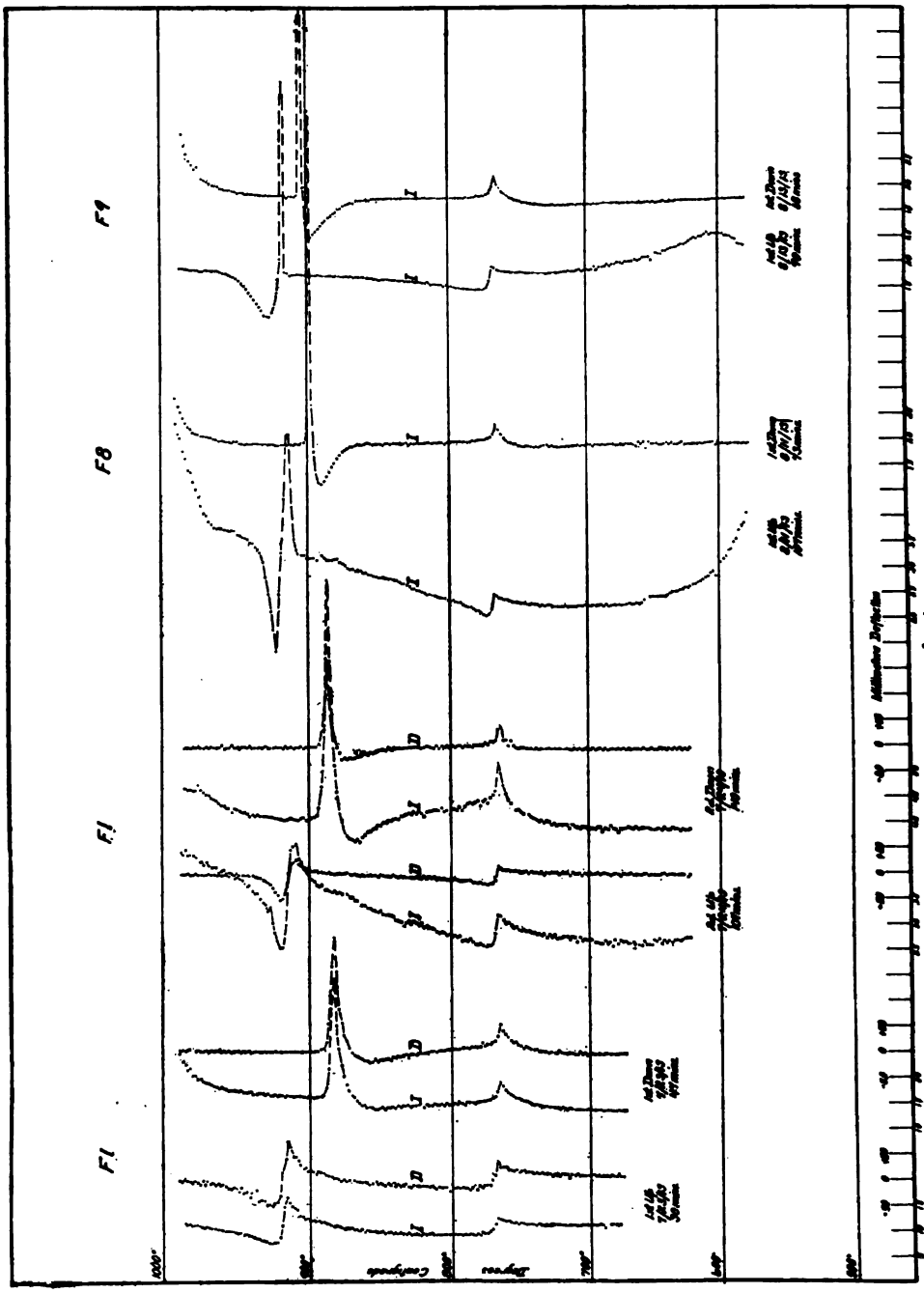


PLATE VII.—CURVES FOR HAMPDENS F1, F7, AND F9.

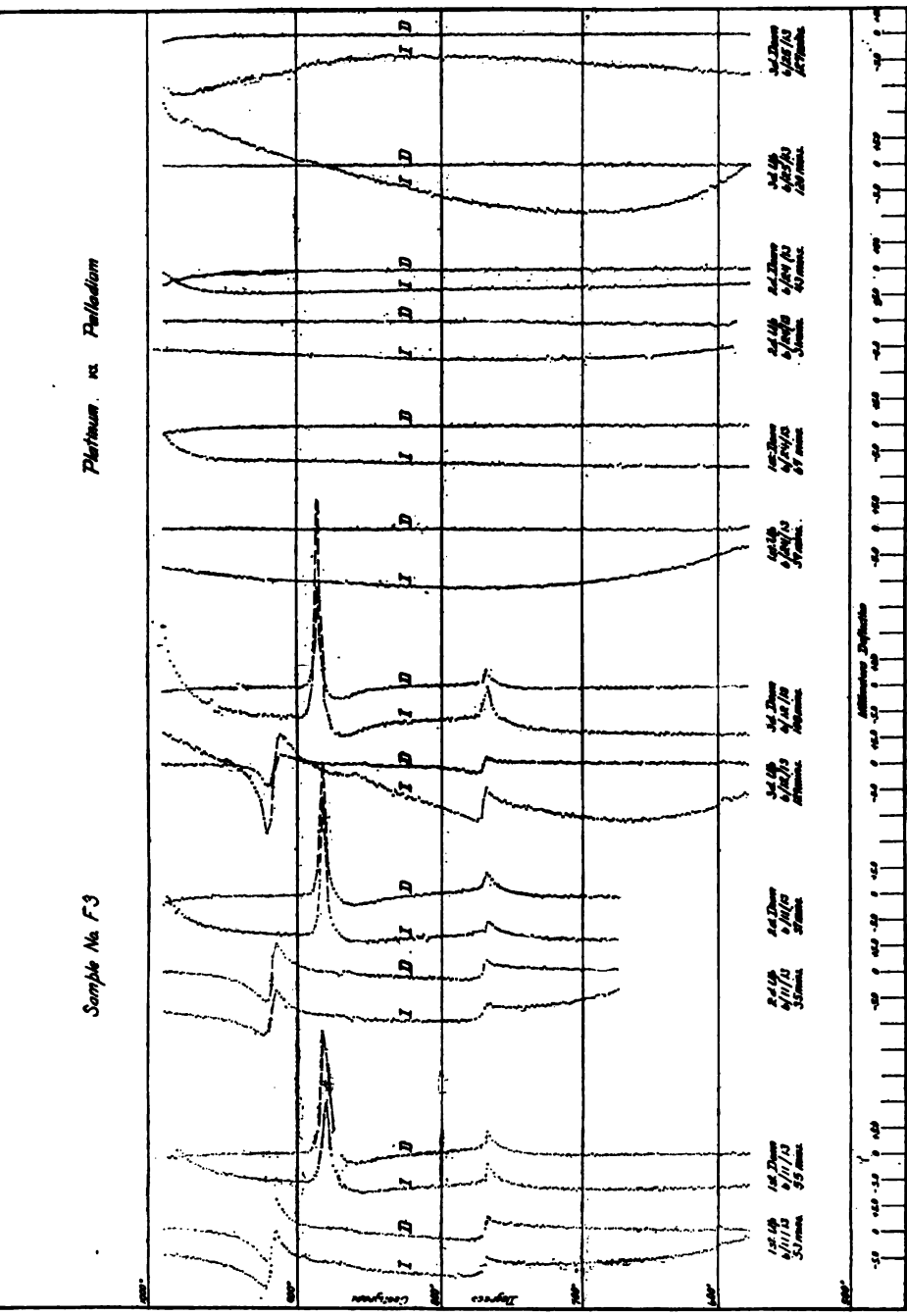


PLATE VIII.—CURVES FOR SAMPLE F3 AND PT-PD BLANK.

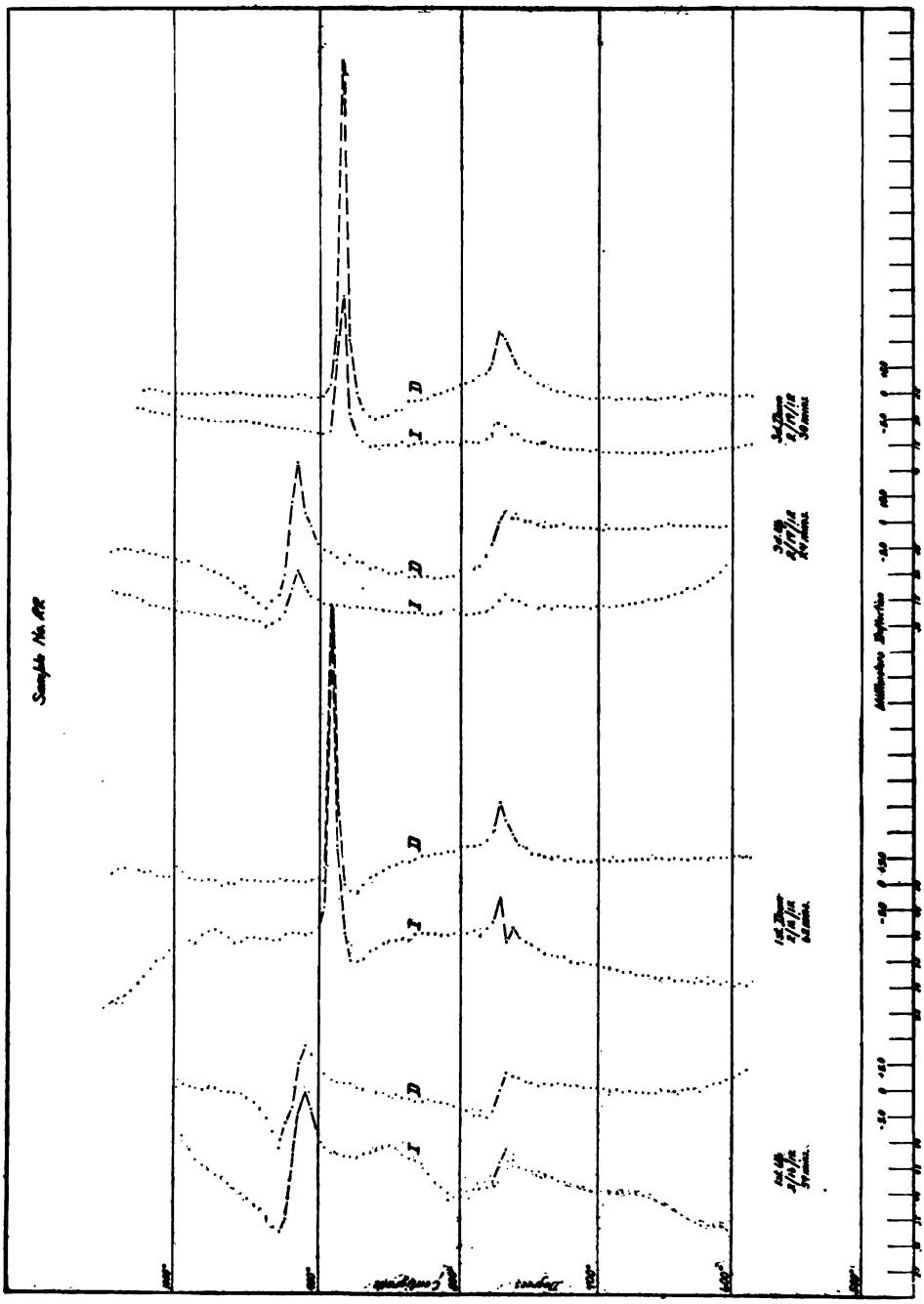


PLATE IX

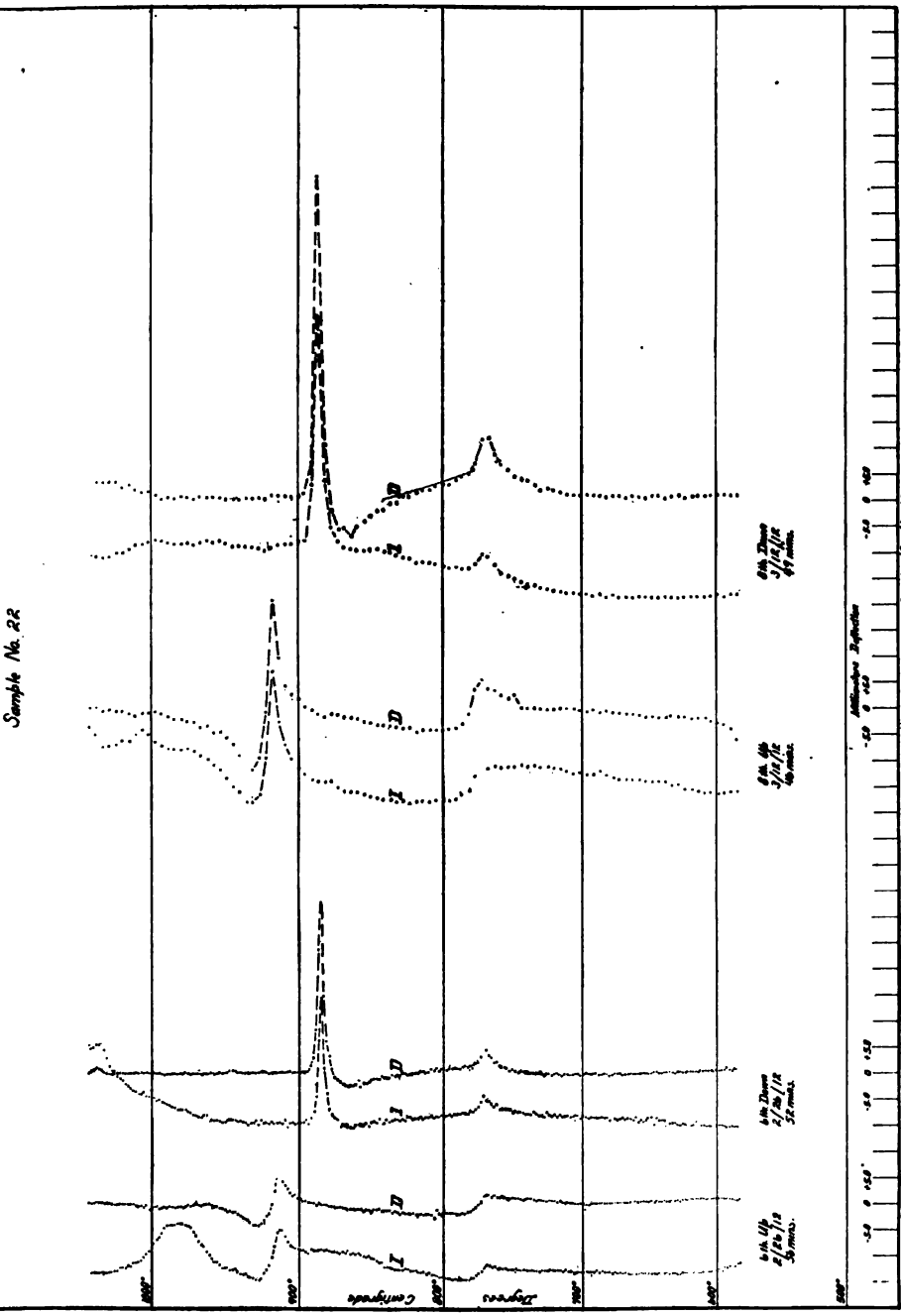


PLATE IXA.—CURVES FOR SAMPLE 22.

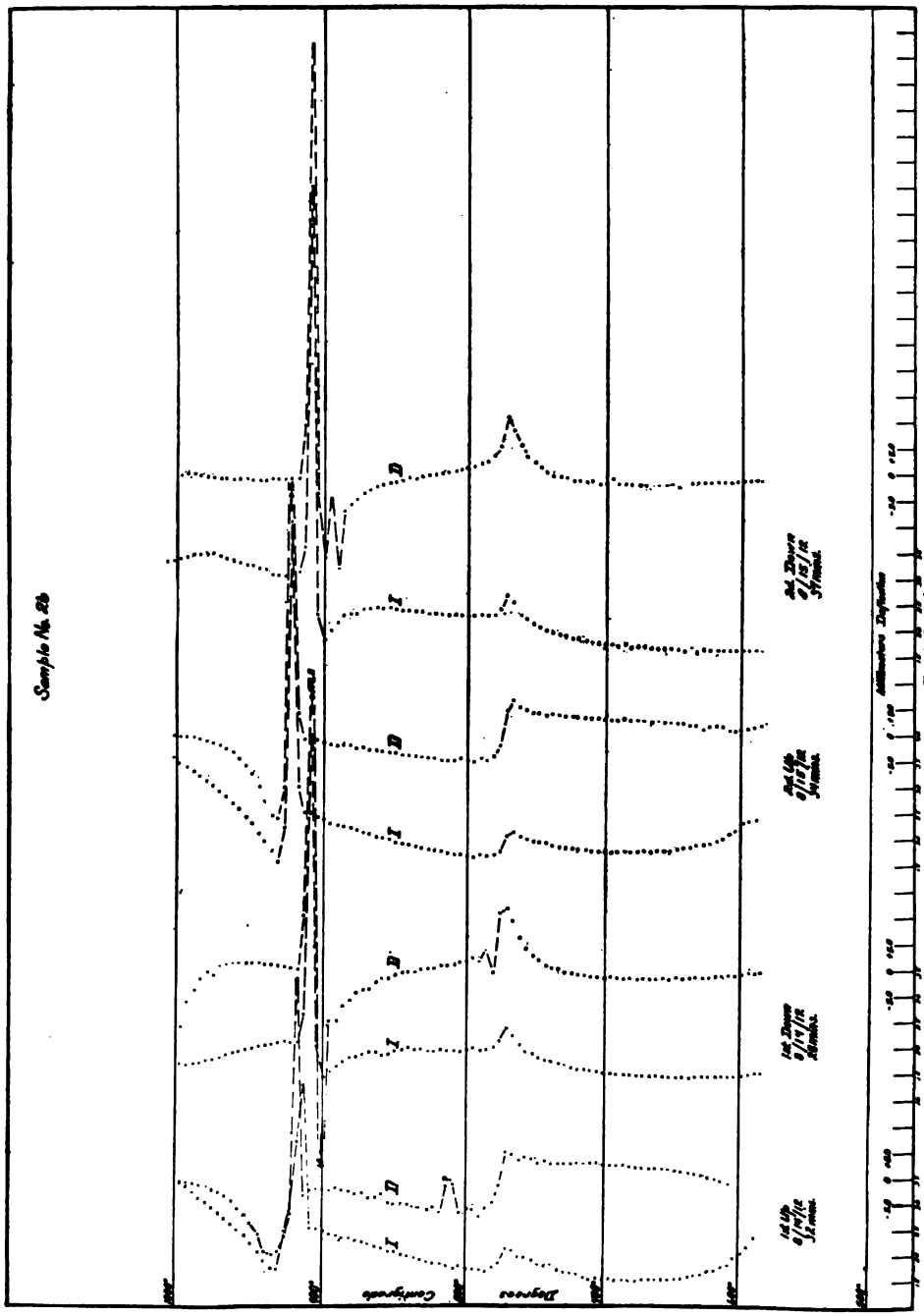


PLATE X. CURVES FOR HAMPER 20

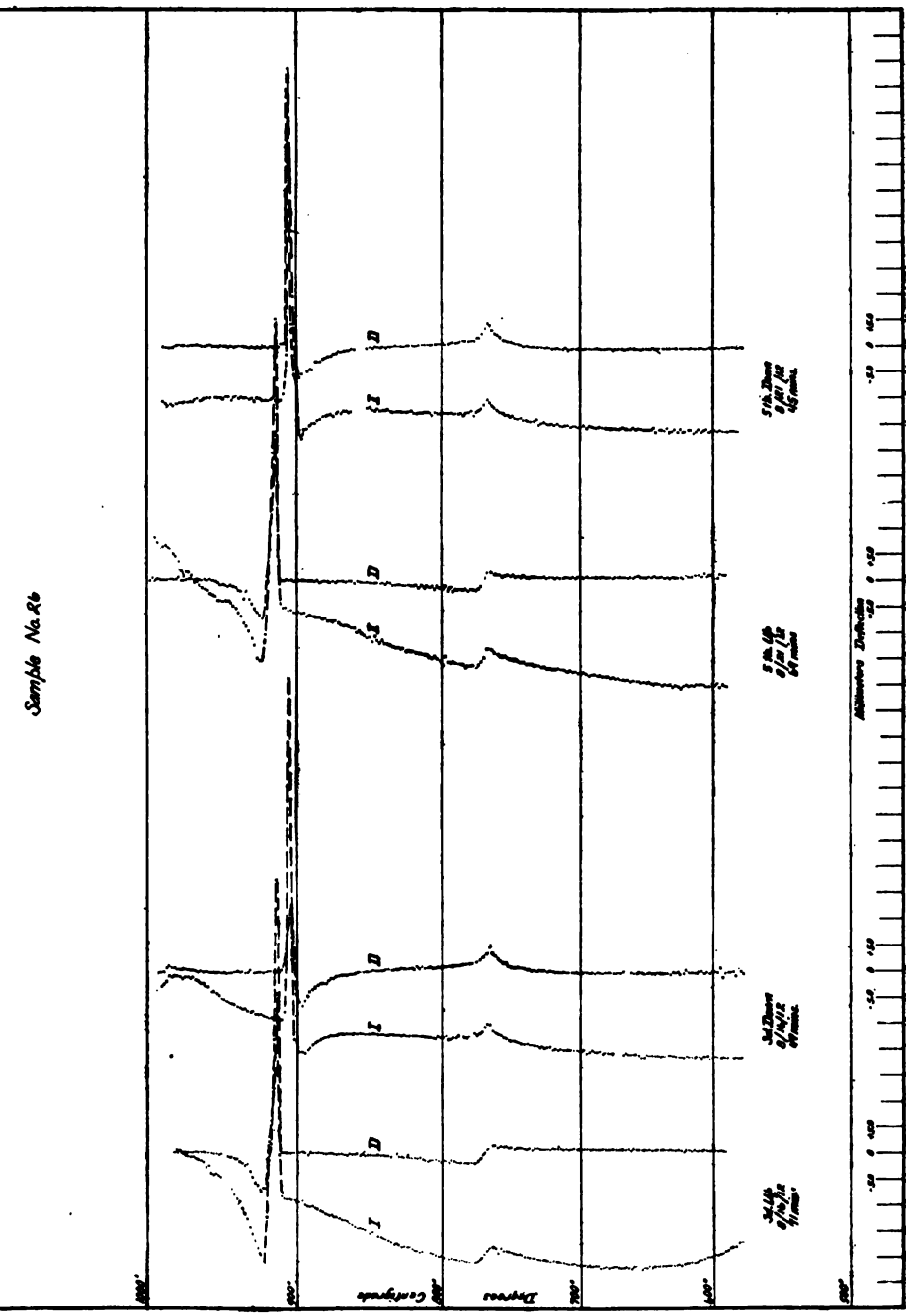
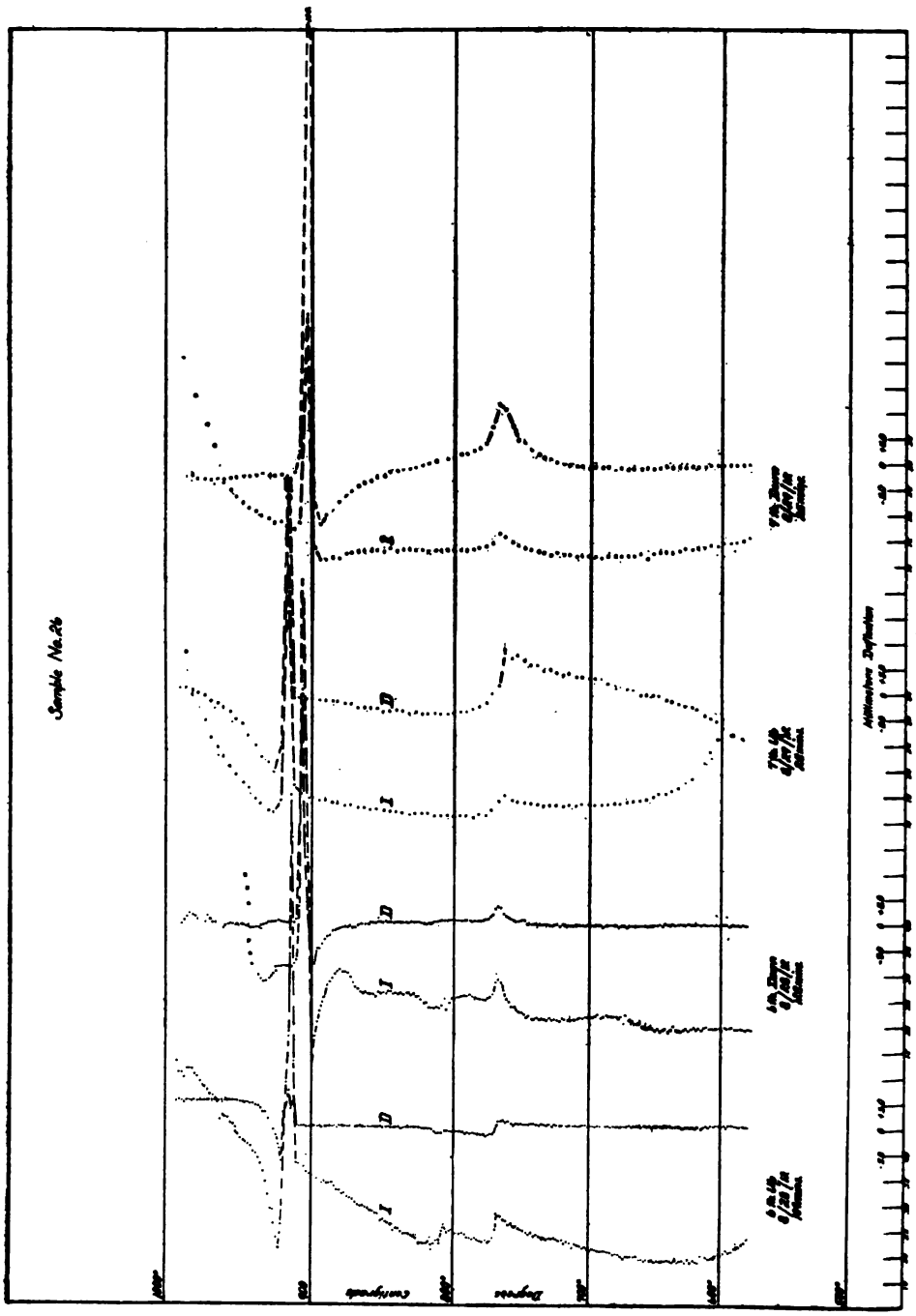


PLATE XA.—CURVES FOR SAMPLE 26.


Sample No. 26
Millimeter Deflection

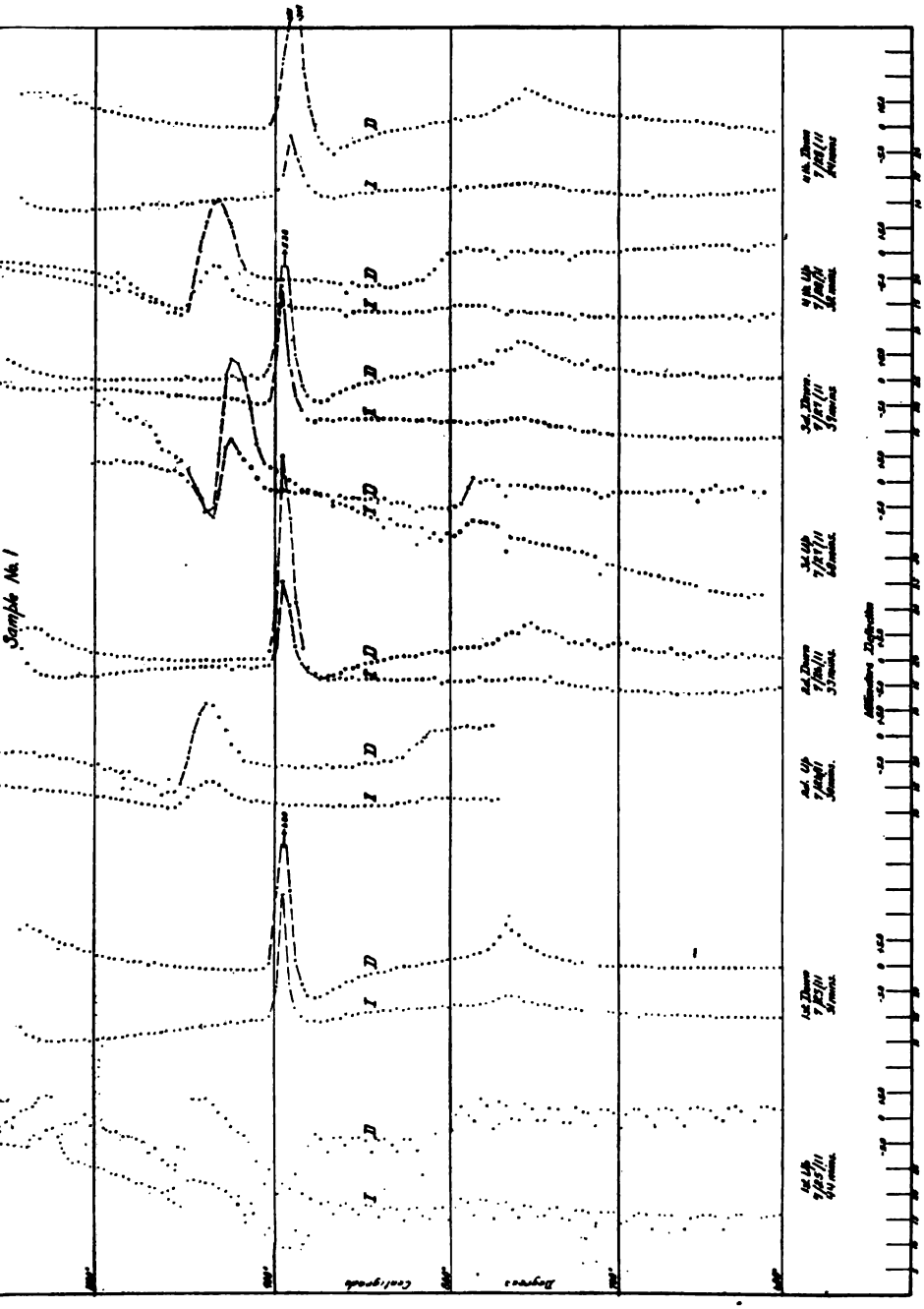


PLATE XI.—CURVES FOR SAMPLE No. 1.

Discussion.

Henry M. Howe, New York, N. Y.:—I think we can hardly overrate the importance of the entry of the Bureau of Standards into our field. There is no body in the world that is better fitted to undertake this work than that Bureau, and it is work which really calls for the aid of some national organization. The skill and care with which this work has been done speaks for itself.

It is very striking that within a very few months the distinguished English investigator, Professor Carpenter, of whom I spoke before, thought that he had disproved the existence of beta iron by proving that the A2 retardation which Professor Burgess has now demonstrated did not occur. Dr. Burgess's demonstration of the existence of this retardation makes the existence of beta iron very probable, in my opinion.

Osmond used to call beta iron "decipuum." You knew of its existence as you know of the existence of certain planets, only by the perturbation which it caused. Certain phenomena could not be explained, except by assuming the existence of a third allotropic form, beta iron. The evidence that you got was always evasive and susceptible of other explanations, except that which we have to-day, which it seems to me is not capable of other explanation.

Professor Carpenter proceeded in the same way that Dr. Burgess has, by taking the heating and cooling curves of exceedingly pure iron, and thought that he had shown that there was no thermal change, no absorption or evolution of heat at the supposed A2 point, and hence that the A2 point did not exist. This he did by a piece of reasoning which I think seems to most of us rather surprising. That is to say, if of ten witnesses, three have seen a thing and seven have not, the evidence of the seven who have not is set aside, and that of the three who have seen stands. In Dr. Carpenter's own experiments under certain conditions he found the A2 change, the heat evolution; under certain others he did not. That is like the three witnesses who saw and the seven who did not see. You can arrange your test so that this change will not become visible or so that it is masked. But though an existent change can be masked a nonexistent one cannot be shown. So that the reasoning seemed to me wholly fallacious.

Professor Benedicks started some months ago, about a year ago, to call our attention to the fact that our ideas of allotropy were very much mistaken in one respect, that we looked at an allotropic change

as something which must needs occur at a definite temperature. Water freezes at zero and boils at 100. That was a very natural conception when we first thought of allotropy, and only had the allotropy of carbon, sulphur, phosphorus, those very striking cases, in mind; but now we find allotropy on every hand.

There is no question, I suppose, now that water above the freezing point consists of water with ice dissolved in it. As the temperature shifts you get that reversal of density above the freezing point. That reversal, which is a progressive and not an instantaneous one, means that the quantities of water and of ice present—not visible ice, of course, but ice dissolved in water—change progressively through a continuous smooth curve. By means of a like case of allotropy Professor Benedicks brought attention to the fact that these allotropic changes may be continuous, they may be spread out through a range of temperature, and may occur through a perfectly smooth curve, giving no sign of their existence to a superficial observer.

What Professor Carpenter did was to cause the A2 or beta allotropic change to take place through so smooth a curve that in many cases no break was detected. He assumed that, because he did not always detect a break, there was no allotropic change, which was not good reasoning. Failure to detect a break might represent either (1) absence of an allotropic change, or (2) the presence of an allotropic change, occurring so progressively and continuously that (*a*) it caused no break, as none is detected in passing the maximum density of water, or finally (*b*) that the break caused could not be detected under the special conditions of the experiment. The fact that Dr. Burgess now detects this break with absolute certainty shows that this last is the reason why Professor Carpenter failed to find a break under certain conditions; his method, though sensitive, was not sensitive enough.

ALFRED STANSFIELD, Montreal, Canada:—I have great pleasure in expressing my admiration for the work of Dr. Burgess, and I am interested to learn something more about the point Ar2.

A number of years ago I showed the existence of this point in electrolytic iron, which was deposited directly on the thermocouple to avoid the possibility of any carbon entering it. At that time we were not beyond reproach in the matter of gases present in the iron, although a good deal of hydrogen was removed by heating *in vacuo;* but as the Ar2 point was observed at the same temperature in low-carbon steel it did not seem possible that hydrogen could cause this evolution of heat. Experiments with mild steel showed that Ar2 was

not due to hydrogen, and experiments with electrolytic iron showed that it was not due to carbon or other impurity, so that we had no doubt that Ar2 was an evidence of some change in the iron itself.

I was therefore surprised to learn that the authenticity of the Ar2 point has been called in question, and I am glad to see that it has been resuscitated and properly authenticated. In my experience I found the Ar2 point not only on the line *MO*, but also following a continuation of this line to the right of *O*. I supposed that this was due to the irregular distribution of carbon within the mass. Perhaps the external portion of the steel was somewhat decarburized, and that would give the Ar2 point when the average carbon content was over 0.45 per cent.

In regard to my discussion of Professor Howe's paper, I should say that the work to which I was referring was of a very unusual and entirely uncommercial character. I was working on the burning of steel. All the steel specimens were carried up into the range *AaB* and deliberately burned. Steel having 1.5 per cent. of carbon showed a very coarse crystalline fracture; great leaf-like crystals could be seen with the naked eye spreading over practically the whole surface of the fracture. In order to break up that structure and restore to some extent the fineness of the grain, I found it necessary to reheat the steel to the line *Sa*. That seemed to be the only way to obtain any reduction of the size of the grain by heat treatment alone, although it was of course impossible to obtain a very fine grain by heating the steel to this temperature.

A. A. STEVENSON, Philadelphia, Pa.:—I would like to call attention to a point brought out by Professor Le Chatelier calling attention to the industrial value of time limit, as determined by Dr. Howe, for holding the piece at a fixed temperature. As I understand it, Dr. Howe's experiments were made with pieces of small mass. The time required to give the best results will depend largely on the mass of the piece. Dr. Burgess tells me he hopes to conduct some experiments at the Bureau of Standards, using samples of dimensions such as we have to deal with in commercial work. The results of such experiments will certainly prove of interest and value.

BRADLEY STOUGHTON, New York, N. Y.:—I can testify to that in confirmation of what Mr. Stevenson says. I know that the makers of cast-steel rolls of very large diameter heat them up for several days, and even after that treatment, when the neck of the roll is broken off

they often find a section in the very center which is not refined. Of course, theoretically that is impossible, because the heat conductivity of the iron should certainly carry the temperature into the center of the roll in a very short time. But unfortunately the people who buy and pay for the rolls are not theorists, and they will not accept the academic ideas on that subject. If a roll is broken and is not refined in the center it is rejected and it is a cause of pretty serious loss to some of the steel foundries. So it would now be a very valuable contribution to our knowledge if we could get some idea of how long it is necessary to heat large masses of steel in order to refine them to the center.

A. A. STEVENSON:—Referring to what Secretary Stoughton has said, in my opinion the question of time required to heat up should not alone be given consideration, but also the effects of various lengths of time in cooling.

PAUL WEILLER, Perth Amboy, N. J.:—One of the most important of processes is just the rolling of heavy steel, and this rolling is going on nearly always between the critical ranges of temperature. It seems to me it would be very important to know how critical temperatures are affected by high pressure occurring in rolling. Everybody knows that in rolling a good many phenomena are observed that were hardly explained till now. I think an investigation in this line would be very useful.

As to the definition of allotropy, this has been done in a very exact way by Willard Gibbs and Bakhuis Roozeboom. Every allotropy is connected with a definite vapor pressure for a definite temperature, and different allotropies, of course, have different vapor pressures for the same temperature, and the crystal form is wholly incidental. It might be that two different allotropies have the same crystal form. It is not the rule, but it may be. From every temperature the allotropy with the lowest vapor pressure is the stable form and all the others are instable.

P. H. GRIFFIN, New York, N. Y.:—It is important to secure, if possible, the interest of manufacturers in the work that has to be done. I think many of the gentlemen present have had some experience with the attitude in the past of some of the larger manufacturing institutions toward work like this. I think this work is the most important that has ever been taken up in this country. That is to

say, it is coming out at a time when there is more of a disposition on the part of all concerned to take it favorably, to get in connection with it. I have had considerable experience in practical manufacturing, and know more or less of the attitude of practical manufacturers toward work of this kind, and the difficulty always has been to get them to realize its value and importance. I think that is due to two facts. The first one is that naturally a good deal of it is Greek to them. They are not chemists, they are not men who understand the terms and other things that they have to understand to keep in touch with the work. And then again a great deal of work is done by practical men, who, generally speaking, have the idea that practice and theory are two such totally different things they never will come together. So far as the work of the Bureau of Standards is concerned, it is really the only place in this country where something of this kind can be done after the work of the different institutions has been brought together. This is by far the largest and most important subject in the whole history of iron and steel in this country—that is, in its possibilities. I want to emphasize the need of getting the co-operation of bodies or associations of manufacturers or others; and particularly of the mechanical men in them, the men that do not very often get into these meetings, because they can throw a great deal of light on this subject if you can only get them to talk. They can tell you their experience in working large amounts of these materials. If you can get those men to take that action it will make a sort of public opinion back of the work that the Bureau of Standards can do. The Bureau of Standards is a branch of the Department of Commerce, and the Department of Commerce has to have some backing to do this thing. It is more or less like what the Agricultural Department is doing for the farmer, and the iron and steel manufacturers are getting more or less into a position like that of the farmer where they are asking the co-operation of bodies like this one and of the government.

CHAIRMAN SAUVEUR:—I am sure that we do desire the co-operation of all iron and steel makers if we want to get results. Meanwhile I think we are extremely fortunate in having the Bureau of Standards ready to take up these important questions, and I for one will look forward to extremely important contributions from them.

The Institute is to be congratulated on having secured for permanent record in its *Transactions* a paper of such importance and timeliness. Dr. Burgess and Mr. Crowe's work in demonstrating, beyond

any possible doubt, the independent existence of the A2 point will be most weighty in settling a controversy which it was highly desirable to settle. It is well known that for many years some metallurgists have doubted the allotropic character of the A2 point, some indeed going so far as to question its very existence. At the Fall, 1912, meeting of the Iron and Steel Institute, Professor Benedicks outlined a theory based on the assumption that the Ar2 point indicated the end of the Ar3 transformation, some gamma iron remaining untransformed (and therefore in a metastable condition) below Ar3, through the influence of impurities. The theory demands the absence of both Ar2 and Ac2 points in absolutely pure iron and the absence of the Ac2 point even in impure iron. At the May, 1913, meeting of the Institute, Professor Carpenter called attention to the above requirements of the theory and reported that he had indeed shown the non-existence of the point Ac2 in impure iron. As to the non-existence of the Ar2 point in strictly pure iron, since such metal cannot be produced, this claim of the theory cannot be tested. In Professor Carpenter's opinion the results of his experiments justified the following statement: "The conception of A2 as an independent allotropic change must be abandoned, and it must now be regarded as proved that Ar2 is simply the termination of Ar3 retarded by impurities present even in the purest forms of iron hitherto prepared." So sweeping a conclusion based on a few experiments, the results of which were opposed by those of all other investigators, was taken exception to by nearly every one discussing Professor Carpenter's paper. This led him to modify his views as follows:

"Summarising the position, therefore, and having regard to the case not only that other investigators have found Ac2 even in the purest samples of electrolytic iron hitherto prepared, but also that facts have been adduced in the discussion which are difficult to explain on the hypothesis that A2 is retarded A3, the author desires to modify his conclusion that 'the conception of A2 as an independent allotropic change must be abandoned,' and to say that 'the present state of knowledge does not justify this conclusion.' At the same time he considers that his own results detailed in the paper appear to him to be most satisfactorily explained on this hypothesis."

At the September, 1913, meeting of the Iron and Steel Institute, I expressed the belief that the burial of beta iron had been premature, giving in support of my contention some results of my own experiments. After reading the paper now under discussion I am more convinced than ever that Benedicks's theory will have to be deeply modified if it is ever to displace Osmond's original theory. The

authors have taken in vacuum and with the greatest refinements some 130 heating and cooling curves of the purest irons obtainable, including a specimen of Professor Carpenter's own metal, and never failed to detect a marked evolution of heat at Ar2 and marked absorption at Ac2. They have proved the independent existence of Ac2 and Ar2 in a way that is absolutely conclusive. To the contention, should it still be made, that in strictly pure iron the point A2 would not occur, it may be replied that the fact that the point Ar2 does not decrease in intensity as purity increases is quite conclusive proof that it would not suddenly disappear with absolute purity. Its disappearance must of necessity be a gradual one.

As the authors well say, whether the A2 point marks or not an allotropic transformation is, to a great exteut, a question of definition. If it be insisted that allotropy necessarily implies a change of crystalline forms, then it may be argued that A2 is not an allotropic point. If, on the contrary, it is believed that a change of internal energy, made evident by spontaneous evolutions of heat, is the criterion by which we should determine the existence of allotropy, then A2 indicates an allotropic transformation. The latter view is strengthened by the discontinuity in some of the properties of iron at the A2 point, especially by the magnetic change occurring at that point.

ROBERT W. HUNT, Chicago, Ill.:—I would like to add just one word on the commercial side, particularly in relation to the Bureau of Standards. It is no idle word that the work should have the emphatic encouragement of our Institute, and of every other scientific and commercial organization in the country. Unfortunately, while they can investigate to a great extent, their voting power is a very limited one, with the result that they are not close to the hearts of the law makers of our country. I have had experience in that line in regard to the effort to establish a national testing laboratory, having appeared before the Congressional Committee on Appropriations. Indifference and want of comprehension of the necessity for such national organizations have generally existed with the American law makers, and they need our sustaining hands. One of the most important steps that have been taken in this country is to recognize the necessity of such a Bureau, and our representatives ought to be made to feel that it is just as much their duty to vote for proper appropriations for such work as for battleships or armies.

G. K. BURGESS:—I would like to express my appreciation of the sentiments that have been stated in the discussion of this paper, and

also I shall take to heart the several suggestions that have been made regarding the work that should be done, which is demanded by the several members of this society; as, for instance, the effect of mass and pressure and the other items which have been mentioned regarding the study, for instance, of the iron-carbon diagram. I also assure you that I will go back to Washington encouraged to go on with this work with as much effort as will be put at our disposal for carrying it on.

CARL BENEDICKS, Stockholm, Sweden (communication to the Secretary*):—1. The great merits of this paper are so obvious that they need not be emphasized; it marks a record in giving accurate figures under definite circumstances, and must be most welcomed by every one. It could possibly be suspected that exception might be made for the present writer, and also for Professor Carpenter, as a glance at the exuberant collection of curves seems to reduce almost to *nil* the value of the beta iron theory advanced by the writer, and as Plate VI shows the most clear heat absorption at Ac2 where Professor Carpenter could not find any heat absorption at all.

To judge from some expressions, the authors had a certain disregard for theorizing. The essence of our science, however, is a well-balanced equilibrium between laboratory work and pure brain work. While I am now going to increase that "amount of theorizing," the fact is that the amount of new experimental work before us makes me feel justified.

2. In every case known as yet, even in the cases of homogeneous equilibrium studied by Professor Smits, an allotropic change is a molecular change—viz., a *chemical* change from one kind of molecules to another—and involves regularly a true recrystallizing. Such a molecular change or recrystallization always proceeds at a comparatively low speed. A consequence of this is that, *without exception, an allotropic transformation point is found at a higher temperature at heating than at cooling.* Now, this is obviously the case for A3; compare especially the excellent Fig. 7, giving evidence not only that A3 is deplaced, but also that it is deplaced all the more, the greater the speed of the temperature change is. (I am not convinced that the lines drawn on Fig. 7 should necessarily be straight lines; there is a possibility that they should be replaced by gently bending curves meeting at the temperature axis; as drawn, the straight lines signify the occurrence of a "false equilibrium" of Duhem, which has not been definitely proved in a single case.)

* Received Nov. 29, 1913.

3. Now, the most important point borne out in the paper is, so far as allotropy is concerned, *that A2 is always found at exactly the same temperature*, and is not at all influenced by heating or cooling speed. This is a proof as good as any that the phenomenon investigated here and designated as *A2 is not a chemical allotropic point* (involving molecular change). Had the authors, on the iron freed from gases, traced the slightest lag in the position of A2, there would be a possibility of admitting a distinct chemical allotropic point. Instead, A2 is determined with the monumentally accurate formula Ac2 = Ar2 = 768° ± 0.5°, allowing of no lag.

The question then arises: if, on this ground, A2 cannot possibly signify a true allotropic change, what might be the secret nature of this A2, immovable and enigmatic as a Sphinx?

4. We then proceed to an inspection of the registered curves. Leaving out of regard the good agreement between the I and D curves, and that astonishing constancy in the position of A2, the cooling curves do not show any new facts, not known by the previous work of Osmond and others. The amount of heat connected with Ar2 seems to be little in comparison with that of Ar3, just as found by nearly all previous workers (the curves below and above Ar2 on all the plates lying tolerably in the same extension).

On the contrary, the heating curves all have a common character which is novel and not met with in any previous research known to the writer. I mean the very abrupt deviation to the left, or back swing, of the heating curves, occurring as soon as Ac2 is reached. The part above Ac2 lies, in fact, very much to the left, signifying an *enormous, continuous heat absorption above Ac2 (or heat development in the specimen below Ac2)*; the heat development at Ar2 is entirely beyond comparison with this. This singular fact is slightly touched upon by the authors on p. 700, just before concluding. If I have correctly grasped this passage, which I do not find quite clear, the authors consider that "this long back swing appears to be in part at least a property of the heating conditions"; they thus seem to attribute the abnormal behavior of the curves to *irregularities in the heating*. On the other hand, on p. 683 special mention is made of the rheostat used, as giving "automatically a constant rate of heating and cooling." The contradiction seems to be an unmistakable one, especially as a constant storage battery was used.

5. Following the suggestion of the authors as to some unknown or not observed influence of the heating, we now proceed to a critical discussion of the energy supply. It is correctly stated that a magnetic field might influence the magnetic transformation. On the other hand,

the assertion is made that an alternating current "eliminates any magnetic field about the iron sample." Now, it is not necessary to refer to the illustrious experiments of Tesla; it might be enough to refer to the much more prosaic experience of the unwelcome heating of iron transformer plates. At each magnetizing cycle, as is well known, a certain quantity of heat is deliberated in every ferro-magnetic substance—the hysteresis coefficient may be as low as possible, the iron as pure as conceivable. The energy supply being very high, and the platinum being wound in two layers, the reversing magnetic field by no means can be neglected, without special precautions which are not mentioned. Let us follow the heating up of a specimen. Being strongly ferro-magnetic, it receives constantly a certain amount of hysteresis heat (besides eddy currents), which per minute might be of the same order of size as, say, the heat development at Ar3. But, as is well known, the ferro-magnetism disappears rather quickly in a narrow temperature range. The consequence is, that this interior extra energy supply must disappear rather suddenly. In the heating curve this must necessarily appear as a strong heat absorption,—which has nothing direct to do with allotropy.

In the cooling curves this extraneous effect must be less pronounced, as the current generally is less on cooling than on heating.

Further, the magnetizing velocity of iron being known to be extremely high, it does not seem unreasonable *a priori* to admit that the passage of the iron from the ferro-magnetic state to the para-magnetic one, and *vice versa*, might also occur with a very great velocity, much superior to the velocity of recrystallization at Ac3. Thus, if the heat developments observed as Ac2 and Ar2 by the authors in fact are due to "heat of magnetization," then it is very natural that no trace of lag is observed ($\pm$ 0.5°!).

It is not possible for me here to go more into details. It is quite possible that my view on this question is not correct, but, from the facts and description as given in the paper, no other explanation seems to me to be possible. In order that the conclusions could be correct, it seems quite necessary, until the authors prove the contrary, to have in view that their observations are valid only in a reversing magnetic field.

In view of this it might be reasonable to add that the failure of Professor Carpenter to find any Ac2 in his purest specimens, on which a very clear Ac2 is indicated on Plate VI, needs not to be caused by his method failing in accuracy, but might entirely be due to the absence in his work of the very serious source of error involved by the use of alternating current.

6. Finally, I do not consider it justified to assert that "the only phenomenon studied which has hitherto always given negative results for A2 is crystalline structure" (p. 677; similar statement, p. 698); it is necessary at least to add that this is also the case with the *expansion*, all the more as this property is extremely important, its measuring being much more reliable and less affected by sources of errors than probably any other property.

Summing up: in spite of the very elaborate and careful experimental work now carried out with well-known thermometric mastership, and so ably presented as an apparent support to the old allotropy views—even if the authors have hesitated in expressing an opinion on the nature of the allotropy—there is, at present, no ground for modifying the allotropy theory as advanced by Smits and, particularly for iron, by the writer.

H. C. H. CARPENTER,* Manchester, England (communication to the Secretary †):—I wish to offer my very cordial congratulations to Messrs. Burgess and Crowe on their paper entitled The Critical Ranges A2 and A3 of Pure Iron. There can be no question but that in point of thoroughness and accuracy it is the best investigation of this problem that has ever been carried out. I need hardly say that the research has a special interest for me, inasmuch as I have myself been recently working in the same field and have come to a different conclusion with regard to the character of the A2 inversion.

Let me begin by saying that the evidence that there is a discontinuity at 768° on the heating curves of pure iron appears to me to be conclusive and indeed overwhelming. Further, I am entirely disposed to agree with the reasons that the authors have suggested as to why I was unable to detect with certainty the Ac2 change in the specimens of iron with which I worked. The evidence offered by them that this was due to "the poorer conductivity and the presence of gases in the wound-up sheets of otherwise untreated electrolytic iron producing a flattening out, distortion, and displacement" of the Ac2 change appears to me to be very strong. At the same time, however, my results as to the location of Ac3 and Ar3 are in very fair agreement with the conclusions of Messrs. Burgess and Crowe.

Before discussing the interpretation of the discontinuity found by the authors at 768° I should like to draw attention to several points of importance which they have established.

1. *The Effect of Occluded Gases.*—This research has shown for the first

* Non-member.

† Received Dec. 1, 1913.

time what this effect actually is, and that if accurate results are to be attained the most suitable procedure consists in premelting the iron *in vacuo*, of course under conditions which will as far as possible avoid contamination.

2. The authors are to be specially congratulated on having obtained satisfactory curves with very small samples (whose weight was of the order of 1 g.). It is quite intelligible why sharper indications of the critical range are thus obtained. I have been particularly interested in the curve F5, of my own sample of iron, obtained with 0.7 g.

3. The thanks of all metallographers are due to the authors for having worked out for the first time the effect of different temperature intervals on the form of the curves (Plate III.) and for having shown that a 2° interval is a satisfactory mean. Another very useful conclusion which they have established is that the inverse-rate and derived-differential curves are strictly similar "save for minor particulars" and "give identical results of the same sensibility for the critical ranges."

4. I am very glad to notice the authors' conclusion that "the A2 transformation has not a double cusp." It has always appeared to me as probable that when a double cusp was obtained this was due to a too rapid rate of cooling.

I come now to the question of the meaning of the discontinuity shown on both the heating and the cooling curves at 768°. The authors say: "An examination of some of the heating curves will perhaps give the incorrect impression that Ac2 is an evolution rather than an absorption of heat. The swing back at the maximum is very abrupt, following what appears to be a general building up . . . from an indeterminate low temperature." An examination of both the heating curves of F5, Plate VI. (my own iron), shows that they are reasonably smooth up to 768° and that there is then a sudden acceleration in the rate of heating. This means either the sudden cessation of an absorption of heat (whose beginning it is impossible to detect) or else an actual evolution of heat. I do not think it is possible to say dogmatically which of the two hypotheses is correct, and for the purpose of my argument it does not matter, but I will take the authors' view that this discontinuity is not an evolution of heat, but corresponds to the cessation of some change which has been proceeding "from an indeterminate low temperature." The cooling curves, however, show that there is an actual evolution of heat at 768°—viz., at the same temperature as on the heating curves. How then is it possible to regard the discontinuity at 768° on the heating curve as corresponding to the discontinuity at 768° on the cooling curve,

since, according to the authors' view, the change begins on heating at a temperature *below* that at which it occurs on cooling? This seems to me to be impossible, and I should be glad if the authors would explain how they can reconcile their interpretation of the curves with the evidence of their own paper.

As a matter of fact, although the authors have undoubtedly established a sharp discontinuity on the heating curves at 768°—a result which my own curves do not show—nevertheless their conclusion that there appears to be a gradual building up of this maximum from an indeterminate low temperature is really very much the same as my own (No. 2):[5] "There are traces of a retardation in the rate of heating spread over a considerable temperature interval, which might be expected to include Ac2 within its limits if it existed." In this important respect, therefore, the authors' and my own curves are in undoubted agreement. The question which has yet to be answered and which future research must decide, is, why is it that there is nothing on the cooling curves to correspond to the gradual building up of a maximum over an indeterminate range shown on the heating curves? If the authors can furnish the solution to this question they will render very great service to all those interested in the allotropy of iron.

In conclusion, it is a matter of considerable interest to me to observe the most probable values assigned by the authors to Ac3 and Ar3. The former they regard as taking place at 909° ± 1. My recent research with Dr. Stead on The Crystallising Properties of Electro-deposited Iron showed that at 910° the recrystallization of iron has just begun, and is complete at 915°. Also, my heating curves (Plate LII. of my paper on The Critical Ranges of Pure Iron) represent the thermal inversion as taking place at temperatures between 902° and 916°, giving a mean figure of 909°. This agreement is very satisfactory. Again, the authors place Ar3 at 898° ± 2. The figure I obtained with Mr. Keeling in 1904 was 900°, which agrees rather better than the figures given in my recent paper, which vary between 898° and 886°.

There are other matters in the authors' paper which I should have liked to discuss had time permitted, in particular the thermal lag which undoubtedly exists during the A3 inversion. It is not large, in fact it is only of the order of about 5°, but it is a real lag. I am very much inclined to agree with their suggestion that this is bound up with the remarkable recrystallization associated with this change

[5] The Critical Ranges of Pure Iron, *Journal of the Iron and Steel Institute*, vol. lxxxvii (No. I, 1913), p. 324.

shown by Dr. Stead and myself, but more research is required before the connection can be regarded as scientifically proved.

G. K. BURGESS, Washington, D. C. (communication to the Secretary*):—The authors welcome and appreciate the contributions to the discussion of their paper by Messrs. Carpenter, of Manchester, and Benedicks, of Stockholm. They note with particular satisfaction that Professor Carpenter, who has contributed by experiment to our knowledge of the properties of iron and by close reasoning to the theory of its allotropy, agrees without reserve to the main conclusions of the paper concerning the separate existence, location, and nature of A2 and A3, and accepts the explanation therein given for his failure to locate satisfactorily the Ac2 change in the specimens of iron with which he last worked. They are also greatly obliged to Professor Carpenter for so clearly summarizing the several points of importance which the authors have established.

Professor Carpenter very pertinently raises the question of the meaning of the discontinuity shown in both the heating and the cooling curves at 768°, and asks "if the authors would explain how they can reconcile their interpretation of the curves with the evidence of their own paper."

The authors would like to state at the outset that they are ready to accept what may appear to be the most rational interpretation of these heating and cooling curves for pure iron, whether coming from themselves or some one else. The only point on which they have any particular sensitiveness is the question of the exactness of their measurements, which is not raised by Professor Carpenter.

It does not seem to the authors, however, that there is any greater difficulty in explaining the heating curves in the Ac2 region than in the Ac3 region; for an examination of the heating curves on several of the plates, notably Plates III, IV, IVA, V, etc., shows that the "abrupt swing back at the maximum" is at least as pronounced and as often unaccompanied by an absorption cusp at Ac3 as at Ac2, as the authors endeavored to point out on p. 700. It was also there stated that "this long back swing appears to be in part at least a property of the heating conditions." From this expression, it is by no means to be inferred, as Professor Benedicks would have it, that "they [the authors] thus seem to attribute the abnormal behavior of the curves to irregularities in the heating."

These curves have no "abnormal behavior." It is to be remem-

* Received Dec. 2, 1913.

bered that the cusps of the I and D curves correspond to the instant at which the temperature of the sample, as measured by the inserted thermocouple, is changing the least. The actual shape of the I and D curves in going through a transformation will depend therefore upon the conditions of transfer of heat between sample and furnace, and sample and thermocouple; which conditions, as is readily seen, are essentially different on heating and cooling, a fact which appears to account for the lack of symmetry in the form of Ac3 compared with Ar3, and Ac2 with Ar2. The authors can see no incongruity in regarding "the discontinuity at 768° on the heating curve as corresponding to the discontinuity at 768° on the cooling curve," even if it be admitted that "the change begins on heating at a temperature *below* that at which it occurs on cooling." A similar thermal condition exists in the melting and freezing curves of innumerable slightly impure substances without exciting particular comment; the maxima (Ac, Ar) are at the same temperature and Ac begins well below its maximum. It is the view held by the authors, and satisfied by the curves in the paper, that both Ac2 and Ac3 are built up gradually from temperatures somewhat below their respective maxima. The reason "why it is that there is nothing on the cooling curves to correspond to the gradual building up of a maximum over an indeterminate range shown in the heating curves?" asked by Professor Carpenter, appears to be simply this, that the "swing back" on cooling below the maximum of Ar2 or Ar3 cuts into this "building up" region, as it must (corresponding to the "swing back" immediately above Ac2 or Ac3), so that this region below the maximum of Ar2 or Ar3 apparently does not exist on cooling, an anomaly which cannot well be avoided on account of the experimental exigencies of the sample "catching up" with the furnace in temperature.

Regarding the location of Ac3 and Ar3, the agreement, noted by Professor Carpenter as obtained both thermally by himself in association with Mr. Keeling and by crystallographic examination with Professor Stead, with the results of this paper, is particularly gratifying and convincing.

It is with regret that with many of Professor Benedicks's remarks the authors feel obliged to disagree most emphatically.

"The exuberant collection of curves" is apparently not sufficient evidence for him to discard the lone half dozen that their author, Professor Carpenter, now renounces, and on which slender scaffolding Professor Benedicks would still support an apparently inconsistent and unsound "theory" of allotropy. He also considers the

authors have "a certain disregard for theorizing." The authors do make a sharp distinction between hypothesis and theory and feel constrained to state that they do not consider the published ideas of Professor Benedicks on the allotropy of iron—in so far as they understand them—better than an untenable hypothesis or several such hypotheses, for it is difficult to reconcile with each other many of the statements made by Professor Benedicks in his several recent written contributions to the allotropy of iron. (See *Journal of the Iron and Steel Institute* for past two years.) The authors thoroughly agree with his statement "that our science, however, is a well-balanced equilibrium between laboratory work and pure brain work."

Professor Benedicks's latest criterion (§ 2 of his communication), based on his interpretation of the very interesting views of Professor Smits on allotropy, is that "*without exception, an allotropic transformation point is found at a higher temperature at heating than at cooling,*" and that therefore (§ 3), since "A2 is always found at exactly the same temperature, ... *A2 is not a chemical allotropic point* (involving molecular change)." As stated by the authors on p. 701, allotropy is a matter of definition. This one of Professor Benedicks would exclude all transformations reversible at constant temperature, including freezing and melting of crystalline substances.

Although their observations do not fully warrant the statement, nevertheless the authors are inclined to the belief, that at zero rate and for a mass approximating zero the temperature of Ar3 would equal that of Ac3, since with the smallest pieces Ac3–Ar3 is a minimum; therefore, granting this, by Benedicks's latest theory, iron would have no allotropic forms whatever. Professor Benedicks, however, does not make the converse statement, namely, when Ac is not equal to Ar, there is allotropy. Assuming this, it would follow that since Ac1 > Ar1 or the Fe–C system, the pearlite point divides two "allotropic" forms.

Again, in § 2, Professor Benedicks would like to redraw the authors' Fig. 7, making in the limiting case, as above noted, Ac3 = Ar3. Therefore, by his own reasoning and analysis there is no allotropy associated with the A3 transformation. Professor Benedicks goes on to state that "as drawn, the straight lines signify the occurrence of a 'false equilibrium' of Duhem, which has not been definitely proved in a single case"; but this separation of the lines in Fig. 7 is the exact fulfillment of Benedicks's new criterion of the existence of "allotropy"; therefore A3 is an allotropic point by Benedicks's definition in the same § 2. This "Milo the Cretan" analysis might be

continued indefinitely in theorizing concerning Professor Benedicks's statements, but the authors will limit themselves to the ingenious hysterisis argument of Professor Benedicks, according to which, since with such "monumental" accuracy Ac2 is identical with Ar2, "immovable and enigmatic as a Sphinx," the point A2 has no existence because observed with samples heated in a furnace supplied with alternating current!

If the only thermal observations on the existence of A2 were those of the authors, this "theorizing" of Benedicks might possibly be considered as throwing doubt on the genuineness of the A2 point as here observed—not on its existence, but on its interpretation in terms of "allotropy." To refute Benedicks's argument here, it is only necessary to quote part of his § 4, "the cooling curves do not show any new facts, not known by the previous work of Osmond and others. The amount of heat connected with Ar2 seems to be little in comparison with that of Ar3, just as found by nearly all previous workers," etc. The presence of both Ac2 and Ar2 has been detected by many observers (see Tables I and II) using all kinds of furnaces, from the chimney of Professor Arnold and the electric heating by direct current of Mueller, Rosenhain, Carpenter, and many others, to the alternating-current heating of Rosenhain and Humfrey and the authors. The statement of Professor Benedicks in § 5 "that the failure of Professor Carpenter to find any Ac2 in his purest specimens, on which a very clear Ac2 is indicated on Plate VI, needs not to be caused by his method failing in accuracy, but might entirely be due to the absence in his work of the very serious source of error involved by the use of alternating current," is unwarranted by the facts. The Ac2 region *is present* in Professor Carpenter's curves, but, as stated on p. 675 and accepted by him, "the poorer conductivity and the presence of gases in the wound-up sheets of otherwise untreated electrolytic iron producing a flattening out, distortion, and displacement of the critical regions, especially of the feebler A2."

Even if present only with alternate-current heating, A2 would still be a transformation in iron, allotropic or not according to the necessarily arbitrary definition adopted.

In order to convince even Professor Benedicks that the iron used by the authors has no Sphinx-like qualities at Ac2, due to an alternating-current heating supply, the authors have taken heating and cooling curves of a sample in an ordinary gas-heated muffle furnace, with the following results for A2 maxima (see also Fig. 9):

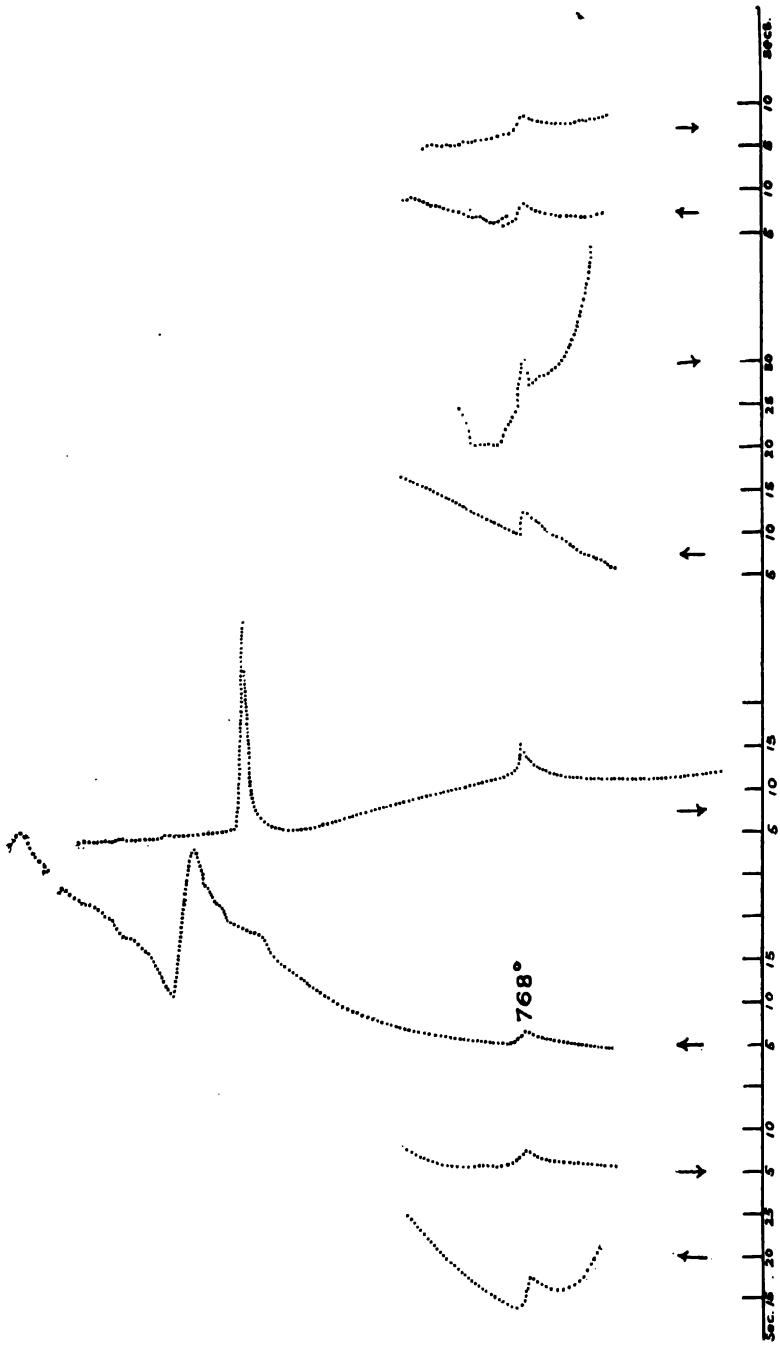


FIG. 9.—PURE IRON. INVERSE-RATE CURVES IN GAS FURNACE.

Location of A2 of Iron in Gas Furnace.

Date.	Sample.	Rate: Sec./Degree.		Ac2.	Ar2.
		Up.	Down.		
12–1–13	F7	0.08	0.33	766	768
12–1–13	F7	0.14	0.24	768	770
12–1–13	F7	0.15	0.05	768	768
12–1–13	F7	0.23	0.23	768	770

The authors object finally to Professor Benedicks unwarrantably classifying them in the ranks of the supporters of the "old allotropy views." They *observed* a reversible, thermal transformation at 768°, but it by no means follows that they necessarily believe in a β state.

In conclusion, the authors would suggest the following concerning the theory of allotropy of iron:

1. Some of the experimental facts that any theory must satisfactorily account for are: a definite thermal effect, independent of rate of heating or cooling, located at $768 \pm 0.5 =$ Ac2 identical with Ar2, *i. e.*, with no measurable lag; a much greater thermal effect, but dependent upon rate, accompanying a marked crystallographic change, and, as extrapolated for zero rate, located at Ac3 = 909 and Ar3 = 898. Both the Ac2 and Ac3 maxima appear to be anticipated by gradual changes in physical properties below these temperatures defining the maxima. A2 and A3 are distinct transformations.

2. Among the experimental uncertainties are, whether with A2 there is associated any crystallographic change, however slight—it being recalled that the thermal change is also very slight; and whether there is any slight volume change accompanying A2. Concerning A3, we are not sure whether, as zero mass and zero rate are approached, Ac3 exactly equals Ar3 or not. For neither A2 nor A3 has the beginning of Ac2 or Ac3 been exactly located.

3. We may define an allotropic transformation in many ways. Confining ourselves to a pure substance in the solid state, let us make the simple definition: *an allotropic transformation is one accompanied by crystallographic change.*

4. There appears to be no doubt that A3 is an allotropic transformation under this definition, whether or not for zero rate and mass Ac3 = Ar3, so long as there is observed a crystallographic change. It is not necessary in the case of iron to add to the definition the complications of Smits's theory, actually observed with the masses hitherto used experimentally; namely, that Ac3 > Ar3, and the faster the rate the greater the interval Ac3–Ar3.

5. Sufficient experimental evidence is yet wanting to decide in favor of A2 being an allotropic transformation according to the above definition.

6. So much for facts and theory. We still have the realms of analogy and hypothesis to work upon to guide us as to the probable nature of A2 in terms of allotropy. One of the greatest experimental difficulties encountered is due to the non-transparency of iron crystals rendering an adequate optical examination of iron passing through A2 well-nigh hopeless.

Turning to the transparent substance quartz, however, we have a material which appears to present a close analogy in certain of its transformations to those in iron. At 575° quartz has a very minute transformation, although a definitely defined one by thermal analysis, corresponding evidently to our A2 in iron. At about 900° there is a violent crystallographic change in quartz accompanied by a correspondingly large thermal effect and also by a very considerable volume change; in iron, exactly our A3. Now it is interesting to note that for the reversible point at 575° in the transparent quartz, there is a very slight crystallographic change accompanied by a minute volume effect, which anticipates the sharp maximum at 575° by many degrees in the same way that the numerous physical properties of iron appear to anticipate the sharp maximum of iron at Ac2 = 768.

7. Reasoning from the quartz analogy, therefore, the following hypotheses may be made regarding the nature of the A2 transformation in iron: namely, that A2 is an allotropic point accompanied by a very minute but as yet undetected crystallographic change and differing from A3 mainly in magnitude; that when sufficiently exact expansion measurements are made on pure iron, a minute but abrupt volume change will probably be found at A2 preceded by a very feeble anticipatory region in the expansion curve below A2.

If experiment eventually proves the first of the above hypotheses incorrect, we have the oft-expressed alternative that the A2 transformation is mainly if not entirely associated with the passage from the ferro-magnetic to the para-magnetic state; and under the above definition, A2 would not be an allotropic transformation, and β iron would be really dead.

Ae 1, the Equilibrium Temperature for A 1 in Carbon Steel.*

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(New York Meeting, October, 1913.)

The Equilibrium Position of A 1.—Some of the most important data on this subject are collected in Table I.

Definition of Ae 1.—Just as we call A 1 of rising temperature Ac 1, and that of falling temperature Ar 1, so we may give the symbol Ae 1 to the equilibrium position of A 1.

Preliminary Assumptions.—In deciding what weight to assign the various data, and indeed what data are to count, three assumptions should be made. These are:

1. The corollary from the phase rule, that the temperature of Ae 1 is constant and independent of the carbon content, though, of course, it may be changed by the presence of other elements.

2. That because neither in heating nor in cooling can the transformation begin before the temperature reaches Ae 1, though it may be delayed by lag till after the temperature has passed beyond Ae 1, therefore Ae 1 should be at or below the observed beginning of Ac 1 and at or above the observed beginning of Ar 1, and, *a fortiori*, it should be at or below the observed maximum of Ac 1, and at or above the observed maximum of Ar 1. It may be necessary to take these maxima into consideration in view of the experimental difficulty of detecting these beginnings.

3. Because of the extraordinary degree of lag in the immediate neighborhood of Ae 1, as proved by Brayshaw, little weight should be attached even to the most conclusive evidence that the transformation has not occurred on reaching, or even staying at, a given temperature, because that failure to occur may always be due to lag. Evidence that the transformation has occurred, or has even begun, at a given temperature deserves incomparably greater weight. In other words, the temperature may pass beyond Ae 1 without inducing the transformation, but the transformation cannot be induced except by

* A Contribution from the Metallurgical Laboratories of Columbia University. MS. received Feb. 10, 1913.

TABLE I.—*Important Determinations of A 1.*

No.	Authority.	Date.	Composition of Specimens.						A 1.			Method.
			C.	Si.	Mn.	P.	S.	Gr.	Ac 1.	Ar 1.	Gap.	
									°C.	°C.	°C.	
1	Carpenter and Keeling	1904	0.34	0.06	Tr.	0.03	0.01	.....	724 (Beginning)	724 (Beginning)	0	Thermal. Ar 1 faint.
2	Carpenter and Keeling	.....	1.85	0.09	.....	.....	.....	Nil	729	718	11	Thermal. Ar 1 well-marked.
3	Carpenter and Keeling	.....	3.98	.....	.....	.....	.....	.....	728	728	0	Thermal. Ar 1 faint.
4	Carpenter and Keeling	.....	4.50	0.12	.....	.....	.....	.....	728	715	13	Thermal. Ar 1 well-marked.
5	Carpenter and Keeling	.....	2.63	.....	.....	.....	.....	.....	.....	723	.....	Thermal. Ar 1 well-marked.
5 A	Carpenter and Keeling	.....	2.85	.....	.....	.....	.....	.....	.....	723	.....	
6	Carpenter and Keeling	.....	2.85	.....	Tr?	.....	.....	.....	.....	716	.....	Thermal. Maximum of Ar 1 well-marked.
7	Heyn	1904	0.39	0.04	0.03	.....	.....	.....	700	.....	.....	Thermal. Beginning of Ac 1 very faintly marked.
8	Heyn	.....	0.95	0.04	0.06	.....	.....	.....	.....	708	.....	Thermal. Beginning of Ar 3-2-1 very sharply marked.
.....	Heyn	.....	.....	.....	.....	.....	.....	.....	.....	695	.....	Thermal. Maximum of Ar 3-2-1 very sharply marked.
9	Rosenhain (on Brayshaw's steel A 2)	1910	1.14	0.09	0.40	0.014	0.018	Tungsten	720	.....	.....	Thermal. Maximum Ac 1, beginning at 718° ±.
10	Rosenhain (on Brayshaw's steel A 2)	.....	.....	.....	.....	.....	.....	.....	715	.....	.....	Thermal. Beginning Ac 1.
11	Benedicks	1908	1.00	.....	0.25	.....	.....	.....	725	695	30	Thermal maxima.
12	Charpy and Grenet	1903	0.64	.....	.....	.....	.....	.....	713	.....	15	Dilatation. Ends of transformation in heating and cooling, with long holding at stationary temperature.
13	Charpy and Grenet	.....	0.64	.....	.....	.....	.....	.....	.....	698		
14	Charpy and Grenet	.....	0.93	.....	.....	.....	.....	.....	728	.....	16	.....
15	Charpy and Grenet	.....	0.93	.....	.....	.....	.....	.....	.....	712		.....
16	Charpy and Grenet	.....	1.50	.....	.....	.....	.....	.....	707	.....	.....	Dilatation. Beginning of contraction in heating up slowly.
17	Brayshaw, No. W 1	1910	1.15	0.21	0.31	.011±	.012±	0.50	.....	731	.....	*Hardening:* After holding at higher temperatures, cooling, holding at 731° and quenching, hardness decreased slowly as the holding time increased, at last approaching that of samples quenched from evidently below A 1.
18	Brayshaw, No. W 2	1910	1.16	0.10	0.37	0.014	0.023	0.48	729.25	.....	.....	*Hardening:* Heated direct to quenching temperature, held there, quenched, and hardness determined. No. 18 hardened slightly at 729.25°, not at 728°. No. 19 hardened slightly at 728° but not at 725.5°.
19	Brayshaw, No. A 2	.....	1.14	0.09	0.40	0.014	0.018	0	728	.....	.....	
20	Levy	1913	0.23	0.039	0.05	0.013	0.010	0	725	.....	.....	*Hardening:* Heated direct to 725°, held there, and quenched. Hardened slightly.
21	Levy	1912	0.92	0.14	0.123	0.009	0.011	0	.....	710	.....	Heated to 900°, cooled to the quenching temperature, quenched. Hardened when quenched from 720°, but not from 710°.

reaching or passing Ae 1. Hence, in determining the position of Ae 1, all data are here rejected which show only that the transformation has not occurred, or has not begun, at any given temperature.

The data collected in Table I., taken at their face, give a range of 31°, from 700° to 731°, because one of Heyn's results (No. 7) indicates that Ae 1 lies at or below 700°, and one of Brayshaw's (No. 17) indicates that it lies at or above 731°. But this uncertainty of 31° is unreasonably great for our present powers of observation. A critical study of the observations allows us to cut this range down to about 8°—viz., from 715° to 723°, and a weighted average of the determinations is 723°, as I will now explain.

Narrowing the Range of Probable Temperatures for Ae 1.—From among the data at hand certain ones may be selected as the most trustworthy. These should not include the beginnings of Ac 1 of Carpenter and Keeling, and of Charpy and Grenet, because their original curves are not available; nor Carpenter and Keeling's beginnings of Ar 1 at 724° and 728°, nor Heyn's beginning of Ac 1, 700°, because they are not well enough supported by the thermal curves in view of their conflict with other and well-supported results. Further, there is reason to think Brayshaw's temperature 731° slightly too high, as explained under No. 19 in the note to Table I. All the remaining data are compatible with the theory that Ae 1 lies between 715° (No. 10 of Table I., Rosenhain) and 723° (Nos. 5 and 5A, Carpenter and Keeling). That is to say, all the remaining Ar 1s lie at or below 723°, and all the remaining Ac 1s at or above 715°; and those Ar 1s which lie below 715° may be assumed to do so because of lag, and those Ac 1s which lie above 723° may be assumed to do so because of lag. 715° to 723°, may then be taken as the most probable range.

A Weighted Average.—Recognizing that such averages are weak because they represent only the arbitrary weightings of the weighter, which are purely matters of judgment and opinion, we may yet strike one for what it is worth.

The procedure ought not to follow the usual course, for an obvious reason. The determinations of most properties suggest on their face that they are neither too high nor too low. But this is not true of the determinations by means of which we try to find Ae 1. These determinations are always either Ac 1 points or Ar 1 points. But because each of these is subject to an indeterminate degree of lag, an Ac 1 determination means on its face only that Ae 1 is at or below that determination, and an Ar 1 result means on its face only that Ae 1 is at or above that result. That is to say, these results carry weight in only one direction, the Ac 1 results helping to prevent our

assigning too high a temperature to Ae 1, and the Ar 1 results helping to prevent our assigning too low a temperature, whereas a density or like determination carries weight in both directions, and should be taken into account in considering both those which are greater and those which are less than itself.

TABLE II.—*Calculation of the Weighted Average Temperature of Ae* 1.

No. in Table I.	Ac 1 Data. Ae 1 is at or below ° C.	Ar 1 Data. Ae 1 is at or above ° C.	Weight.	Product. Ac 1.	Product. Ar 1.	Observer.	Method.
17	.....	731	2	.....	1,462	Brayshaw.	Hardening.
18	729.25	.....	3	2,188	.....	Brayshaw.	Hardening.
3	.....	728	1	.....	728	Carpenter and Keeling.	Thermal, beginning.
11	725	.....	2	1,450	.....	Benedicks.	Thermal, max?
20	725	.....	1	725	.....	Levy.	Hardening.
1	724	.....	1	724	.....	Carpenter and Keeling	Thermal, beginning.
5		723	5		3,615		
9	720	.....	3	2,160	.....	Rosenhain.	Thermal, max.
10	715	.....	1	715	.....	Rosenhain.	Thermal, beginning.
21	.....	710	1	.....	710	Levy.	Hardening.
8	.....	708	5	.....	3,540	Heyn.	Thermal, beginning.
16	707	.....	1	707	.....	Charpy and Grenet.	Dilatation, beginning.
7	700	.....	1	700	.....	Heyn.	Thermal, beginning.
13	.....	698	.....	.....	.....	Charpy and Grenet.	Dilatation.
11	.....	695	.....	.....	.....	Benedicks.	Thermal.

This may be made clearer by arranging the data as in Table II. Here we see that, though most of the results are in conflict with at least some one other, certain of them are not in conflict with any others. For instance, all the temperatures in the Ar 1 column, with the exception of Benedicks's 695° and Charpy and Grenet's 698°, argue that Ae 1 is higher than 700° and thus conflict with No. 7, Heyn's 700° for the beginning of Ac 1, which argues that Ae 1 must be at or below 700°. Again, all the results in the Ac 1 column are in conflict with Brayshaw's 731° for Ar 1, for they all argue that Ae 1 is below 731°, whereas his argues that Ae 1 is at or above 731°.

In striking the average, all these conflicting results should be considered, but those not in conflict should not be taken into account. For instance, Charpy and Grenet's 698° and Benedicks's 695° for Ar 1 are irrelevant, because they convey no suggestion as to how far above these temperatures Ae 1 lies, and they are wholly compatible

with any temperature assigned to Ae 1 within the conflicting limits of 700° below and 731° above. The reason for setting these in Table II. is to show why they and like results outside the conflicting range should not be counted. The highest and lowest heavy black lines in Table II. show the limits of this range.

We might at first believe that we ought to represent each observer by one temperature, the temperature to which his data point. But there is no single temperature to which his data point. His Ac 1s have one teaching and his Ar 1s another and independent teaching. Both ought to count and hence both should be included. The highest position for his Ar 1 and the lowest for his Ac 1 are presumably the most accurate, because the least influenced by lag. These, then, are the ones to count.

Moreover, in case his beginning is so faintly marked that we assign a very small weight to it and yet his maximum tends to support that beginning, then we may well take account of that maximum also, always provided that it falls within the conflicting range, *i. e.*, if an Ar 1 point, that it lies above 700°, and, if an Ac 1 point, that it lies below 731°.

The fact that the beginnings of Ar 1 and of Ac 1 are difficult to detect is provided for, not by ignoring them, but by assigning a light weight to them.

The temperature assigned to Ae 1 should be that which does the least violence to the data as weighted, and this temperature is between 723° and 724°, as indicated by the broad horizontal line midway in Table II. Let us take it as 723°. At this temperature the sum of the weighted conflicts is at the minimum. This is only 3° distant from the most important result with which it conflicts, Rosenhain's 720°. In considering this temperature we should remember that the conflict between it and Rosenhain's 720° is apparent only, because the 0.40 per cent. of manganese of his steel may easily lower its Ae 1 position by more than the 3° of conflict. With this 720° left out of account, only Rosenhain's 715°, Charpy and Grenet's 707° and Heyn's 700° for the beginning of Ac 1 argue that 723° is too high, and only Brayshaw's 731° for Ar 1 and Carpenter and Keeling's 728° for the beginning of Ar 1 argue that it is too low, and of these the former may well be several degrees too high. This weighted sum (700 + 707 + 715 = 2,122 below) + (1,462 + 728 = 2,190 above) = 4,312 is less than if any temperature above 724° or below 723° is selected. 723° is reconciled with all the data other than those just discussed by the reasonable assumption that those above it in the Ac 1 column are above it because of lag, and those below it in the Ar 1 column are

below it because of lag. Indeed, when we have reached this point we see that some temperature, close to 723°, would have to be selected even if no weighting process was followed.

Summary.—While there is evidence which tends to put Ae 1 as high as 731° and other evidence which tends to put it as low as 700°, yet a reasonable exclusion of the less strongly supported evidence gives a range of 715° to 723° as that within which Ae 1 probably lies.

A weighted average of the results discussed, including those thus rejected because outside the 715° to 723° range, is 723°.

Note to Table I.

1 to 6. Carpenter and Keeling, *Journal of the Iron and Steel Institute*, vol. lxv. (1904, No. I.), p. 224, and *Collected Researches of the National Physical Laboratory*, vol. i., p. 227, Plate 4. Cylindrical specimens ⅛ in. long and ⅛ in. in diameter were heated, without complete exclusion of the air, in general to temperatures between 1,100° and 1,140° in 1.5 hr., held for about 0.5 hr., and cooled thence to 500° C. in about 1 hr. 20 min. For these individual specimens the full analyses are not given. For seven other specimens the manganese content is given, and in each of these it is a trace.

The time-temperature curve was obtained in certain cases by direct reading, and in other cases by differential reading.

The temperature, 723°, which I give for the beginning of Ar 1 of their 2.63 and 2.85 per cent. alloys (their Nos. 27 and 30), is not given directly by them, but read off their curves Nos. 27 and 30, Pl. 4, *Collected Researches of the National Physical Laboratory*, vol. i., p. 227.

7 and 8. Heyn, Mikroskopische Untersuchung, etc., *Verhandlungen des Vereins zur Beförderung des Gewerbfleisses* (1904), p. 371, and Plates B and C. The time-temperature curve was taken differentially. Specimens were heated rapidly, without complete exclusion of the air, to 1,100°, whence they were cooled. The cooling to 200° occupied about 42 min. in all cases.

9 and 10. Rosenhain, *Proceedings of the Institution of Mechanical Engineers* (1910), parts 1 and 2, p. 688; *Engineering*, vol. lxxxix. (1910), p. 702, column I, Fig. 2. This is the same steel as No. 19. The heating was done in a very high vacuum and apparently with very slow movements of temperature.

11. Benedicks, *Journal of the Iron and Steel Institute*, vol. lxxvii. (1908, No. II.), pp. 218 to 219. Cylindrical specimens 50 by 6.5 mm. were heated without complete exclusion of air. Ac 1 was at 725°; Ar 1 at 695°. His Ar 1 represents the temperature of complete arrest, *i. e.*, of maximum heat evolution, and his Ac 1 also is probably the maximum.

12 to 16. *Bulletin de la Société d'Encouragement pour l'Industrie Nationale* (1903, No. I.), pp. 480 to 482. The observations were all made by the dilatation method, with extremely slow changes of temperature, about 200° per hour in heating.

For Nos. 12 and 13 the transformation, as determined by dilatation, completed itself, on holding the temperature stationary between 708° and 713° for 1 hr. in heating up. In cooling down the transformation completed itself, on holding the temperature stationary for 0.5 hr., between 708° and 698°. For Nos. 14 and 15 the same conditions applied, the stationary temperature being between 723° and 728° in heating up and between 713° and 712° in cooling down. p. 482.

No. 16. Charpy and Grenet give on p. 480 the temperatures at which the transformation occurs, as indicated by the beginning of the abrupt contraction at Ac 1. The temperatures vary from 707° to 730°. In one case a temperature of 693° is given, but as in this case the contraction was "hardly appreciable," the carbon content being only 0.07, and as in their curve itself no trace of this contraction can be seen, this temperature should in my opinion be left out of consideration.

17, 18, and 19. Brayshaw, *Proceedings of the Institution of Mechanical Engineers* (1910, parts 1 and 2), pp. 525, 537, 656 and 670; *Engineering*, vol. lxxxix. (1910), p. 526, column 1.

No. 17 was heated to a variety of temperatures between 760° and 893°, transferred thence to a salt bath in a furnace at 731°, left there for periods ranging from 30 to 240 min., and drawn and quenched in brine at 16° or 18° C. The hardness of the quenched specimen, whether measured by the Brinell or by the sclerôscope test, progressively decreased as the sojourn in the second furnace increased, till after 240 min. it had fallen nearly to the hardness of a specimen of the same steel held in the second furnace at 731° without previous higher heating, and then quenched. From the progressive decrease of hardness it is inferred that 731° must be below Ae 1, the slowness of the decrease representing the slowness with which the Ar 1 change from austenite (through martensite?) into pearlite occurs at temperatures only very slightly below Ae 1. The fact that the specimen heated to 731° and quenched thence did not harden is corroborative evidence that 731° is below Ae 1, though of little weight. The slowness of the loss of hardness during the holding at 731° is referable in part to a continuing of the fall of temperature during an important part of the holding. When the hot specimen first entered the relatively cool bath at 731° its rate of cooling would be very rapid; but as its temperature approached that of the bath its further cooling would be slower and slower, finally reaching 731° only asymptotically.

As pointed out under No. 19, Brayshaw's temperatures may be a little too high. It might be thought that his 731° was actually above Ae 1, and that the reason why his specimens quenched after progressively longer and longer stays there were progressively softer and softer was that they were progressively decarburized more and more by the salt bath. This presupposes that he did not remove the effects of surface decarburization by grinding off the decarburized layer. But this explanation should be set aside, because, if it were true, then the specimens held at 731° for various periods without previously passing above 731° also should have undergone surface decarburization, and hence should have lost hardness progressively, because the hardness of even unhardened steel decreases progressively with decreasing carbon content. But in fact the hardness of these specimens remained practically constant. Hence the inference that Brayshaw's temperature was actually below Ae 1.

No. 18. On heating to 728° and quenching was not hardened; on heating to 729.25° and quenching was hardened slightly.

No. 19. On heating to 725.5° and quenching was not hardened; on heating to 728° and quenching was hardened slightly.

These temperatures of Brayshaw's may be a little high, for Rosenhain found the maximum of Ac 1 with this same steel at 720°, and its beginning at about 715°. But Brayshaw found this steel apparently wholly unhardened when quenched in brine after a stay of 30 min. at 725.5°. This failure to harden at all after so long a stay at this higher temperature, 725.5°, is very hard to reconcile with Rosenhain's finding the maximum of Ac 1 at the lower temperature, 720°, by the thermal method, because the long sojourn of Brayshaw at stationary temperature ought to lead to a lower observed position of Ac 1 than that observed in the presumably more rapid rise of Rosenhain's thermal method. Rosenhain is in the highest degree accurate and trustworthy. In view of the evident extreme care and accuracy with which Brayshaw worked, the most probable explanation is that, though his results are closely comparable among themselves, yet his temperature scale is a few degrees too high.

20 and 21, cylinders ⅝ in. in diameter and 0.5 in. long, treated by my assistant, A. G. Levy.

No. 20, when heated to the quenching temperature and held 30 min., then quenched, hardened slightly when quenched from 733° but not when quenched from 725°. Brinell drop hardness number 20 and 18 respectively.

No. 21, heated to 900°, cooled slowly to the quenching temperature, held 30 min., and then quenched, hardened when quenched from 720° but not when quenched from 710°, the Brinell hardness being 48 and 27.5, in each case by the Brinell drop test.

Note to Table II.

The beginnings of Ar 1, 728° of Carpenter and Keeling and of Ac 1, 700° of Heyn, are weighted lightly because they are not well supported by thermal curves. A further objection to Carpenter and Keeling's 728° is that it coincides exactly with the beginning of Ar 1 for this same steel. In view of the great lag in the immediate vicinity of Ae 1, as proved very clearly by Brayshaw, it seems in the highest degree improbable that the beginnings of Ac 1 and of Ar 1, if found truly by the thermal method with any usual rate of heating and cooling, can coincide. On the other hand the beginnings 723° of Carpenter and Keeling and 708° of Heyn are not only well marked on the cooling curves, but are followed so soon by a very great heat evolution as to deserve great weight. Moreover, this same temperature, 723°, is the beginning of Ar 3 for two of Carpenter and Keeling's alloys. Under these conditions the fairest way seems to be to retain the extreme temperatures, 728° and 700°, but to assign them a very low weight, and to supplement them with these better supported beginnings, 723° and 708°, and to assign these a correspondingly greater weight.

The Rosenhain case may be met in like manner: Because of the greater difficulty of detecting the beginning of Ac 1, his 715° is assigned a relatively light weight, but his maximum of Ac 1, 720°, is assigned a much greater weight.

It might be thought that Carpenter and Keeling's maximum of Ar 1, 716°, also should be given weight. As to this opinions may differ. I think that its weight is already recognized in that assigned to their beginning, 723°, of Ar 1, a weight given in consideration of the good support which these maxima give it. Be that as it may, it would not influence the result of our weighting calculation, because this Ar 1 temperature is below any which the other data allow us to assign to Ae 1.

In considering the relative weight to be attached to the results of Carpenter and Keeling and of Rosenhain we should remember, on one hand, that they used very pure materials, whereas his steel had 0.40 per cent. of manganese, and that their experiments were very extensive; and on the other hand, that they did not work *in vacuo* as he did.

The value of Brayshaw's temperatures is lessened by the fact that Rosenhain, who is extremely accurate and trustworthy, found temperatures somewhat lower in operating on the same steel. Further, Brayshaw's No. 17 had 0.50 per cent. of tungsten and his No. 18 had 0.48 per cent.

The low weight assigned to the data of Charpy and Grenet is due to our lacking the original curve, and to their method of calibration, which may have lowered their observed temperatures materially.

The reasons for assigning so small a weight to the observations of so competent an observer as Benedicks are that his thermal curves themselves are not given; that his numbers are maxima, which are less good evidence than the beginnings of Ac 1 and Ar 1; and that his manganese content is rather high, 0.25 per cent.

The New International Diamond Carat of 200 Milligrams.

BY GEORGE FREDERICK KUNZ, NEW YORK, N. Y.

(Butte Meeting, August, 1913.)

THE manifold inconveniences resulting from the absence of a uniform standard of mass for determining the weight of precious stones have long been obvious. This lack has been keenly felt in commercial transactions, and those who have devoted time and research to the study of historic diamonds and precious stones have had frequent occasion to deplore the absence of such a standard in the past.

In a paper read in Chicago in 1893, before the International Congress of Weights and Measures, held in connection with the World's Columbian Exposition, the writer suggested dividing the carat into 100 parts, and constituting a standard international carat of 200 mg.; that is, 5 diamond carats or 20 pearl grains to a French gram, making 5,000 carats or 20,000 pearl grains to a kilogram. He also called attention to the fact that while this would depreciate the present diamond carat or pearl grain only about 2.5 per cent., it would abolish the troublesome discrepancies between the various carat-weights now in use, and could be easily explained and understood everywhere.[1]

The subject of the various diamond carats, their incongruity, and the resulting confusion as to the correct weights of historic gems when definite records of them are searched for, has been fully treated by me in an extensive study of this subject in *The Book of the Pearl*.[2]

To the earnest and unremitting efforts of C. E. Guillaume, Director of the Bureau Internationale des Poids et Mesures at Sèvres, is largely due the eventual success of this eminently desirable reform, which he has constantly urged both by articles on the subject and by addresses delivered in Paris before the International Committee for Weights and Measures.[3]

The general adoption of a uniform standard for dealings in pre-

[1] See *The Book of the Pearl*, by George Frederick Kunz and Charles H. Stevenson, p. 325 (New York, 1908).

[2] *Ibid.*, pp. 321 to 329.

[3] See C. E. Guillaume, Les récents progrès du système métrique; reprint from vol. xv. of *Travaux et Mémoires du Bureau Internationale des Poids et Mesures*, pp. 62 to 66 (Paris, 1907). Also, George F. Kunz, *Mineral Industry*, vol. xx., p. 624 (1911).

cious stones, based upon a carat of 200 mg., was early recognized to be a result much to be desired, and great progress has recently been made in this direction. The carat has heretofore varied in weight in the different countries, with more or less resulting confusion and inconvenience to business. The metric carat has now been formally adopted in most of the countries of Europe, and its use made compulsory. Our own country has at last taken action, and Great Britain will do so before long. [See Postscript, p. 768.] Many of our dealers and importers recognize the theoretical advantage and the convenience of the change, but they apparently fail to take any active interest in it, as a practical reform. There can be little question, however, that the change must come ere long. A single standard for all countries, and the substitution of decimal for common fractions in the carat, are advantages so plain that they must surely soon be realized.

A striking illustration of the defectiveness of the system hitherto in use, as compared with the proposed new standard, is given in a recent article by L. J. Spencer, of the British Museum, on The Larger Diamonds of South Africa.[4] In this article an effort is made to clear up certain published errors and misstatements as to the great diamonds obtained in recent years from the African mines. But it proved impossible to ascertain definitely the precise weights of some of these notable stones, especially those of the earlier discoveries, on account of the uncertainty as to which carat-weight had been employed in determining them. The metric equivalent of the present "Board of Trade carat" is 205.304 mg.; while that of the old English carat, in use prior to 1888, was 205.409 mg. Hence, with some of the stones, only an approximate statement of their original weight is now possible.[5]

A first step in the direction of simplifying the carat-weight was taken in 1871, when the syndicate of Paris jewelers, goldsmiths, and others dealing in precious stones proposed the adoption of a carat of 205 mg. (3.1636 grains) to take the place of the older French carat of 205.5 mg. (3.17135 grains); this action was confirmed in 1877. On Oct. 17, 1890, the Association of Diamond Cutters of Antwerp fixed the value of the carat at 1 kg. = 4,875 carats, which gave a carat of 205.183 mg., or 3.16561 grains troy.

The more radical and effective change to the carat of 200 mg. was the subject of a resolution passed in January, 1906, by the Chambre Syndicale de la Bijouterie, Joaillerie et Orfèvrerie de Paris, and the

[4] *Mineralogical Magazine*, vol. xvi., No. 74, pp. 140 to 148 (London, Oct., 1911).

[5] G. F. Kunz, Precious Stones during 1911; reprint from *The Mineral Industry*, vol. xx., pp. 624, 625.

Chambre Syndicale des Négociants en Diamants, Perles, Pierres Précieuses et des Lapidaires de Paris, and the reform was strongly advocated by M. Guillaume, in 1906, before the Commission des Instruments et Travaux. Shortly before, in the early part of 1905, the German Federation of Jewelers had petitioned the Imperial government to legalize the carat then in common use as a standard weight, but this was refused as in violation of the law prescribing the exclusive use of the metric system. In the course of the discussion aroused by this refusal, M. Guillaume advocated the new international carat of 200 mg. as a possible solution of the difficulty, and as early as April, 1905, this proposition was taken into consideration by the International Committee. In August, 1906, on the motion of Ludwig Schröder, the following resolution was adopted by the German Federation: "Considering that it is both necessary and advantageous to replace the old carat by the metric carat of 200 milligrams, the Federation authorizes its president to approach the Imperial government and the foreign associations in order that the metric carat may be introduced as soon as possible in all countries."

Following the action of the International Committee, on Dec. 7, 1906, the Chamber of Commerce of Antwerp promised to rescind the resolution of Apr. 29, 1895, approving the adoption of a carat of 205.3 mg., as soon as the new international carat of 200 mg. should come into universal use in the markets. About the same time the Association of Jewelers and Goldsmiths of Prague formally authorized the German Federation to propose in its name the reform of the old carat as soon as possible by international agreement, and the Association of Goldsmiths of Copenhagen declared its willingness to support such a measure. The Belgian Committee of Weights and Measures, in July, 1907, declared its willingness to petition the government to legalize the new carat on its adoption by the more important countries.

In England and its colonies the proposed change was favorably received. In September, 1907, a resolution indorsing the new carat was passed by the Association of Manufacturing Jewelers of Melbourne, Australia; on Oct. 16, 1907, the Association of Societies for the Protection of Commerce in the United Kingdom passed a resolution urging its adoption in all countries, and on Jan. 23, 1908, the Birmingham Jewelers' and Silversmiths' Association gave expression to the hope that all nations would speedily accept an international carat of 200 mg.

The new carat has now been legalized in a number of countries; in others, laws favoring it are in course of preparation, or of adoption.

Definite information has reached us concerning the legal status of the new international carat in the following lands:

Germany.—In the German Empire the question of the carat has been very simply settled, without any new legislation. The fifth article of the law of May 17, 1856, abrogated the carat in common use, which was not referred to in the laws of Aug. 17, 1868, and of May 30, 1908. Now while in most countries not only is the use of non-metric unities prohibited, but also any nomenclature foreign to that of the metric system, the laws of the German Empire are silent in regard to this latter particular. Hence those interested can give the name "carat" to the unity they commonly use without coming in conflict with the laws, on the sole condition that this unity be represented by a standard figuring in the table of standards of mass subject to verification.

In conformity with the legal stipulation officially communicated by the Secretary of the Interior to the General Federation of German gem-dealers and jewelers, the Secretary has decided to adopt the new carat.

Belgium.—The draft of a law, signed by the King, was submitted to the Chambers at the beginning of July, 1909. This law has not yet been promulgated. [See Postscript, p. 768.]

Bulgaria.—The new carat was included in the provisions of a law published Apr. 10, 1910.

Denmark.—The first article of a law voted Apr. 1, 1910, and which became operative on that day, is couched in the following terms:

"In the application of the metric system to the commerce in precious stones, pearls, etc., the metric carat, equivalent to 200 milligrams, shall henceforth be used."

Spain.—A Royal Order of Mar. 11, 1908, has prescribed the use of the new carat of 200 mg.

France.—The law of June 22, 1909, comprises a single article, as follows:

"In the transactions relating to diamonds, pearls, and precious stones, the designation 'metric carat' may, in violation of the first article of the law of July 4, 1837, be given to the double decigram.

"The use of the word 'carat' to designate any other weight is hereby prohibited."

Thus the new French law formally prohibits the use of the old carat, but admits that of the new international carat of 200 mg., considered to be an authorized violation of the fundamental law on the application of the metric system. The prohibition regarding the old carat was to become effective from Jan. 1, 1911, but owing to the

difficulty of preparing the proper weights at so short notice, the request of the jewelers that a postponement should be granted until Jan. 1, 1912, was accorded; since that time the law has been operative.

This law was elaborated in two decrees, of July 7 and of Dec. 13, 1910. The first of these decrees stipulates that "the form of the carat-weight shall be that of a quadrangular, truncated pyramid, or of a cylinder surmounted by a knob. However, the carat-weights of less than one gram are to be in the form of square-cut metal plates. The dimensions of the cylindrical weight shall differ from those established for such weights by the fifth appendix to the ordinance of June 16, 1889.

"The various weights are to be inscribed in intaglio and in legible characters, the number of grams on the lower face; that of the metric carats, followed by the abbreviation C. M., on the upper face."

The same decree enumerates the carat-weights constituting the minimum complete series with which the dealers interested must provide themselves; this series is in conformity with the metric system, between 2 mg. and 100 g.

Holland.—The law defining and legalizing the carat of 200 mg. was laid before the Second Chamber on June 9, 1910. In the exposition of the motives for its preparation reference is made to the desire expressed by the International Committee of Weights and Measures in its session of 1905, and to the decision of the Fourth General Conference. This law was promulgated Apr. 7, 1911.

Italy.—Parliament has already legislated, in principle, the new international carat (July 7, 1910); a Royal Decree will fix the date on which it shall come into use, after consultation with the National Commission of Weights and Measures. [See Postscript, p. 768.]

Japan.—An ordinance of Nov. 11, 1909, specifies that "when the weights of precious stones are expressed in carats, the word 'carat' should designate the mass of 200 milligrams."

Mexico.—The government, considering that the designation "metric carat" merely constitutes an exception to the fundamental law, does not see any objection to tolerate the authorization of its use; it considers that this permission should be of a temporary character.

Norway.—The law authorizing the new carat bears date of May 27, 1910; the decree putting it in force was promulgated June 17 of the same year. The text of the law is as follows:

"The name 'metric carat' designates a special metric unity of mass, amounting to 200 milligrams, exclusively destined for the estimation of the price of diamonds, pearls, and other precious stones, and for their sale or purchase.

"The decimal multiples and subdivisions of the metric carat shall be authorized in so far as they may be necessary.

"The word 'carat' shall be in the future exclusively reserved for the designation of the mass above defined."

The decree establishes the series (similar to the metric series) of the multiple and subdivided weights of the carat. It also specifies that the carat-weights shall have the form of an equilateral triangle, one side of which shall be turned up.

Portugal.—The "metric carat" (*quilate metrico*) is included in the table annexed to the decree of Apr. 19, 1911.

Roumania.—A Royal Decree of Mar. 3, 1910, prescribes, from Jan. 1, 1911, the use of the new international carat: the verification of the carat-weights is to be effected in conformity with the general rules comprised in the law concerning weights and measures.

Russia.—The reform of the carat is comprised in the general law in course of revision.

Servia.—The same conditions obtain here as in Russia.

Sweden.—The law establishing the new carat was promulgated June 10, 1910; the obligation to observe it was fixed for Jan. 1, 1911.

Switzerland.—The carat of 200 mg. is comprised in the law on weights and measures promulgated June 24, 1909.

As may be seen, the new international carat of 200 mg., adopted or in course of adoption in 17 countries, is prescribed by laws having slightly different texts, according to the sense in which the reform is viewed; in general, the prohibition implicitly contained in the law on weights and measures touching the use of any non-metric unity is expressly affirmed as regards the carat in common use, but the employment of the word "carat" to designate a mass of 2 decigrams is admitted as a toleration necessitated by the state of things, and justified by the special conditions of the commerce in precious stones.

In those countries in which no legislation has fixed the value of the carat, but where the metric system is in use, the toleration which has been enjoyed by the ill-defined unity by which the weight of precious stones has been computed, should *a fortiori* be applied to a unity strictly determined by its simple relation to the gram. We may therefore say that the reform is in reality more general than is indicated by the legal status of the carat.[6]

As a preliminary to the presentment of the law legalizing the use of the new carat in Holland, the measure was submitted by the Minister of Agriculture, Industry, and Commerce to the Amsterdamsche

[6] Comité International des Poids et Mesures; *Precès-Verbaux des Séances*, Paris, 1911; 2d ser., vol. vi., Session of 1911, pp. 202 to 205.

Juwelier's Vereinigung and to the Diamond Exchange of Amsterdam; these bodies concurred in advocating the change.

In conformity with a resolution passed by the Fédération Internationale des Bourses du Commerce de Diamants, Perles et Pierres Précieuses of Antwerp, at the convention of 1912, Adolph Adler presented to the Minister of Industry and to Senator Braun petitions for the legalization of the new carat, and received from the latter the assurance that he would urge the passage of a law to that effect in the Belgian Chamber of Deputies.

Not long since some fifty jewelers met in the rooms of the National Jewelers' Board of Trade, in New York, to consider the advisability of adopting the new standard. Prior to the meeting, M. D. Rothschild, who was elected Chairman, had sent out 4,000 postal cards to leading jewelers in the United States, requesting their opinion on the subject of the introduction of a new international carat of 200 mg. Only about 360 replies were received; however, there was some consolation in the fact that but nine of these were negative.

Now that the new carat is likely to be soon in universal use, it would be well to consider whether jewelers should not substitute the use of the gram for that of the pennyweight. The grain is equivalent to about 64.8 mg. (more exactly 64.798918), and the pennyweight equals 1.555 g., the troy ounce being 31.1 g., and the troy pound 373.24 g. There can be little doubt that when the great convenience in computation resulting from the adoption of the new international carat of 200 mg. shall have been fully realized, the advantage of using the gram instead of the pennyweight will become clearly apparent, as in this way the troublesome multiplications by 24, 20, and 12 will be done away with. As five of the new international carats make exactly 1 g., all calculations regarding the weights of diamonds, pearls, and precious stones will then be in accordance with the metric system.

An important step in the direction of the official recognition of the new carat was the sending of a communication by Secretary Nagel, of the Department of Commerce and Labor, to the Secretary of the Treasury, recommending its adoption as the standard weight in computing the value of precious stones imported into the United States. The draft of this letter was prepared by Director S. W. Stratton, of the Bureau of Standards, Oct. 29, 1912. After drawing attention to the adoption of the new carat by so many European countries, the following excellent reasons are adduced for favorable action in the matter by the Treasury Department:

"The carat is not the most important element in estimating the value of precious stones, particularly of the diamond, but it is never-

theless important that its value be fixed. The change from the carat now used by the Treasury Department to the one proposed would probably be of no significance in so far as the amount of duty on precious stones collected by the Department is concerned, but it would be very important in its effect upon the unification of standards. I therefore have the honor to suggest that the Department adopt and use the international carat of 200 milligrams and thus set an example which I feel sure will be followed by the jewelry trade in the United States."

Official steps have been taken by our State Department, at the suggestion of the Treasury Department, tending to secure common action by the governments of Great Britain, Holland, and Belgium with our own in the employment of the new international carat of 200 mg. to determine the weight of all precious stones exported or imported into these countries, and the favorable action of the Treasury Department now realized was foreshadowed by S. W. Stratton, Director of the Bureau of Standards in Washington, D. C., in a letter of Jan. 28, 1913.

A number of representative dealers in precious stones met in London, on Feb. 7 of this year, under the auspices of the London Wholesale Jewelers and Allied Trades Association, Ltd., to consider the question of the new carat-weight. One of the speakers informed the meeting that the Board of Trade had been approached on the subject, and it appeared that its members thought favorably of the chances of the adoption of the new system in England after a little more propaganda work had been done there. The Board of Trade does not at present exercise any official control over the diamond weights in use, but a legalization of the new carat would result in the proper testing and stamping of the new series of weights.

A strong point made in favor of the adoption of the metric carat in England is that, according to the English Weights and Measures Act of 1878, the old carat is an illegal weight, this act expressly stipulating that gold, silver, and precious stones should only be sold by the ounce troy, or decimal part thereof. Hence it has been held that, strictly speaking, a contract to buy or sell so many carats' weight of diamonds would be an illegal contract and not enforceable. The new metric carat, on the other hand, would be fully covered by the Weights and Measures (Metric System) Act of 1897, and would therefore constitute a perfectly legal unit of mass.[7]

[7] See W. J. Lewis Abbott and Leonard J. Spencer, in *The Watchmaker, Jeweler, Silversmith and Optician*, pp. 1447, 1449, 1451 (Dec. 2, 1912).

The carat-weight equaled essentially the Roman *siliqua*, $\frac{1}{1728}$ of a Roman pound, 2.91 grains troy, or 188.5 mg. It is interesting to note that at the present time this is the weight of the Bologna carat. The name is derived from the Greek *keration*, "little horn," and refers to the shape of the seed pods of *Ceratonia siliqua*, the carob-tree (St. John's Bread), the seeds of this tree having been used to weigh the precious material because their weight is fairly constant. The word carat has come to us through the Arabic *qîrât*, which became in Old Portuguese *quirate*, appearing in modern Portuguese and Spanish as *quilate*. A fourteenth century instance of the use of this word, under the form *garat*, to denote a pearl-weight is given in the *Nuremberg Chronicle*.[8]

The relation of the carat to the *siliqua* does not appear to have been constant, for Isidore of Seville, writing in the seventh century A. D., states that in his time a *cerates* equaled one and a half *siliqua*.[9]

The weight of the seeds of *Ceratonia siliqua*, as averaged from 50 specimens, has been given as 197 mg. (3.04 grains); the orange-red reniform seeds of *Erythrina corallodendron* had the same weight, while the lenticular seeds of *Adenanthara pavonina* gave a much higher average, namely 274 mg.[10]

The variations in the weight of the old carat are shown by the statement of Jeffries in 1751 that in his time there were *about* 150 carats in the ounce troy.[11] This would have given (if exactly 150) a carat value of 3.2 grains, or 207.357 mg.; later, John Mawe (in 1823) reckons 151.25 carats to the ounce, making a carat of 3.174 grains. The present English carat, having a value of 3.1683 grains, was already given by P. Kelly in 1831,[12] although it was not officially accepted by the English Board of Trade until 1888. Even now the English carat does not come within the scope of the Weights and Measures Act of 1878, while there is good reason to believe that the metric carat would have a legal status under the provisions of the Weights and Measures Act of 1897, touching the use of the metric system.

[8] Grimm's *Deutsches Worterbuch*, vol. v., p. 73, art. Karat (Leipzig, 1873).

[9] Du Cange, *Glossarium mediæ et infimæ Latinitatis*, vol. ii., p. 286; s. v. Cerates (Parisiis, 1842).

[10] According to Leonard J. Spencer, a higher average weight for the seeds of *Ceratonia siliqua* has been given, namely, 205.2 mg. or 3.1667 grains, almost exactly that of the English carat; see G. F. Herbert Smith, *Gem Stones*, p. 84 (London, 1912). The average of 3.04 grains approaches very closely to the Florentine carat, 3.03245 grains; this was probably the carat-weight used by Tavernier.

[11] David Jeffries, *A Treatise on Diamonds and Pearls*, pp. 2, 3 (London, 1751).

[12] Patrick Kelly, *Universal Cambist* (London, 1831), vol. i., p. 220, where he writes: "The Ounce troy weighs 151½ Diamond carats, the carat is therefore 3⅛ grains troy or 205¼ French Milligrammes."

The following table of carat-weights heretofore in use in the various countries will give the general reader some idea of the chaotic conditions with which gem-dealers have been forced to contend:

Diamond Carats.[13]

	Milligrams.	Grains Troy.
Turin	213.5	3.29480
Persia	209.5	2.23307
Venice	207.1	3.19603
Austro-Hungary	206.1	3.18060
France (old)	205.9	3.17752
France (later)	205.5	3.17135
France (modern)	205.0	3.16363
Portugual	205.8	3.17597
Frankfort and Hamburg	205.8	3.17597
Germany	205.5	3.17135
East Indies	205.5	3.17135
England and British India	205.3	3.16826
Belgium (Antwerp)	205.3	3.16826
Russia	205.1	3.16517
Holland	205.1	3.16517
Turkey	200.5	3.09418
Spain	199.9	3.08492
Java and Borneo	196.9	3.03862
Florence	196.5	3.03245
Arabia	194.4	3.00004
Brazil	192.2	2.96610
Egypt	191.7	2.95838
Bologna	188.6	2.91054
International carat of the year 1877	205.0	3.16363
New international carat	200.0	3.08647

We must note in this table the very wide discrepancy between the heaviest carat-weight, that of Turin, equivalent to 3.2948 grains troy (213.5 mg.), and the lightest, that of Bologna, representing but 2.91054 grains troy (188.6 mg.). Hence the Turin carat is a little more than 13 per cent. heavier than that of Bologna, an enormous difference when we have to deal with such costly commodities as precious stones.

The impossibility of carrying on a diamond business systematically with such an appalling variation in the weight of the diamond carat, and with no possible means of an effective check to determine the accuracy of the weights employed, must be clear to all. It is a fact that a great number of jewelers use a set of weights for as many as 20 years, and in the meantime these weights will either wear off, or, if they are handled and lifted with the fingers, as is often the case, they may become heavier.

[13] *The Book of the Pearl*, by George F. Kunz and Charles H. Stevenson, p. 323 (New York, 1908).

Hungary, Belgium, Argentine Confederation, Spain, Italy, Mexico, Peru, Portugal, Rumania, Servia, Sweden, Norway, Switzerland, Venezuela, Japan, and Denmark.[14]

There is, in the rooms of the Chambre Syndicale in Paris, a standard balance, which is verified once every month by the Maison Exupère. Access can be had to this balance by any member at any time, and this aids materially in securing standard weights of absolute accuracy. There are two great sources of error: one is the accretion of dust and perspiration; the other is the wearing away of the weights by friction on the pan of the balance, in the weight box, and by the tweezers in lifting them up.

The writer had a set of the new international weights made in 1910. These new weights, which will be required as soon as the new international carat of 200 mg. shall be generally recognized and employed by American gem-dealers as the standard weight for precious stones, have already been made by a number of balance-makers. The price charged for a set of these weights, 18 in number, ranging from 100 carats to $\frac{1}{100}$ carat, is $3.80; of course, a fuller set would cost a proportionately higher price. It has been stated that there is really no such weight as a "pearl grain;" yet while this is, strictly speaking, a fact, the so-called "pearl grain" being merely a quarter-carat, the term has so generally been used and is so well understood by all familiar with precious stones and pearls, that it may be said to be "consecrated by usage." In any case, however, the statement that, with the introduction of the new carat, pearls can be weighed with greater accuracy, is a perfectly just one, for the advantage of having a fractional weight as small as $\frac{1}{25}$ of a "pearl grain," instead of being confined as at present to the larger fraction of $\frac{1}{16}$ ($\frac{1}{64}$ carat), is clearly manifest.

The astute diplomatist, Talleyrand, is credited with the basic idea of the metric system, namely, the acceptance of the length of a pendulum beating seconds on latitude 45°, as a standard unit of length. This proposition, made by him in 1790, was accepted by the French National Assembly and the sovereign, Louis XVI. The details of the system were worked out by the great French mathematicians Borda, Lagrange, Laplace, Condorcet, and Monge, who constituted a committee appointed by the French Academy of Sciences for this purpose.

Decimal notation, without which the metric system would have been an impossibility, is said to be of Hindu origin, although it reached

[14] *The International Metric System of Weights and Measures*, pp. 3, 4, Department of Commerce and Labor, Bureau of Standards, S. W. Stratton, Director (Washington, 1906).

Europe through the Arabs. Abu Ja'far Mohammed ben Musa, named Al-Khowârazmi from his birthplace, Khwârazm (Khiva), and who flourished in the ninth century A. D., is considered to be the originator, or introducer of decimal notation among the Arabs, and the introduction of the system into Europe has been traced to a translation of one of his works made in 1202 by Leonardo of Pisa.

The nine numerals with the zero were introduced among the Arabs about 773 A. D. and are by some believed to have been brought by an Indian ambassador to Bagdad at that time. Of their use in India at an earlier period we have monumental evidence in rock inscriptions, and, while the earliest date expressed in these Hindu numerals is 738 A. D., there is sufficient proof of their employment as early as the sixth century.

Introduced to Europeans at the beginning of the thirteenth century, the so-called Arabic numerals very gradually came into general use. For example, in the calculation of the tides in London, in the thirteenth century. A work of Petrarch, printed in Cologne in 1471, has the pages numbered in this way.

The earliest example of the decimal point is said to be found in the Arithmetic of Frances Pellos, written in the dialect of Nice, and published at Turin in 1492.[15]

The first appearance of the symbols + and — in a printed book is in an arithmetic of Johann Widmann printed in Leipzig in 1489,[16] and the sign denoting equality, =, is first found in print in Robert Recorde's (1510?–1558) *The Whetstone of Witte, or the second part of Arithmetike*, London, 1557. He is believed to have adapted it from a similar sign used in medieval manuscripts as an abbreviation of the Latin *est*, "is."[17] Rahn is credited with the first use of the symbol of division, ÷, and Thomas Harriot is said to have been the first to print the signs > and <, denoting respectively excess and deficit, in his *Artis analyticae praxis*, London, 1631.

A simple method of making a rough mental calculation of certain metric weights and measures into equivalents of the English standards is given by Albert A. Cary.[18] This may be in many cases accomplished

[15] *Sen segue de la art de arithmeticha, et semblatment de ieumetrica dich ho nommatus copendio de lo abaco (compilada es ea opera per Fr. Pellos), Impresso in Thaurino lo presente copendio de abaco per meistro Nicolo benedeti he meistro Jacobino luigo de sancto germano-Nel anno* 1492 *ad Di* 28 *de Septembrio.*

[16] Widmann, J., *Behede und hubsche Rechenung allen Kauffmanschafft*, Leipzick, Chr. Kacheloffen, 1489. 8°, 236 pp.

[17] The title "Whetstone of Witte" is the translation of *cos ingenii*, a punning allusion to *cosa* (thing), early used to signify our algebraic *x*, or unknown quantity.

[18] Simple Approximate Metric Conversions, *Power*, vol. xxxvii, No. 7, pp. 217 to 219 (Feb. 18, 1913).

by merely adding 10 per cent. to the figures expressing the metric quantity. For instance, 1 m. equals 1.094 yd. nearly; the error in turning meters into yards in this way amounts to a trifle less than $\frac{6}{1000}$ yd. in each meter. Thus 67 m. would equal 67 + 6.7 yd., or 73.7 yd., or, multiplying this by three, 221.1 ft.; the more exact equivalent being 73.27 and 219.82, respectively. With the kilogram the metric figures must be doubled and then 10 per cent. added to the product to get an equivalent number of avoirdupois pounds, as the kilogram equals 2.204 lb. avoirdupois. Hence 76 kg. can be thus roughly turned into pounds: $76 \times 2 = 152 + 15.2 = 167.2$ lb., the exact equivalent being 167.504 lb., a comparatively slight error when only an approximation is sought. An addition of 5 per cent. to the figures expressing a given number of liters would also offer a fairly close approximation to the number of quarts. If we take, for example, 64 liters, we have: $64 + 3.2 = 67.2$; here the exact equivalent is 67.629 quarts.

In a communication to the Société Française de Physique in Paris, Edouard Guillaume calls attention to a slight difference which exists between the liter and the kilogram or cubic decimeter of water. Accurate modern measurements have shown that the difference amounts to $\frac{27}{1,000,000}$ or a little over $\frac{1}{40,000}$. This very slight, but still appreciable, error is due to the fact that the liter represents the volume of the kilogram of water at its maximum of density under atmospheric pressure.

The metric system was legalized in the United States by the act of July 28, 1866, and although its progress towards popular and general recognition and employment has been unfortunately slow, at intervals since that date certain special official enactments have prescribed its use in particular cases, as in the postal service, where the post offices exchanging mails with foreign countries are provided with balances denominated in metric grams, under the terms of Section 3,880 of the Revised Statutes. A much more important case, however, because it concerns an exclusively national use of the metric system, was the enactment that the weight of the half-dollar, the quarter-dollar, and the dime should be computed in grams, the half-dollar to weigh 12.5 g. and the quarter-dollar and dime respectively one-half and one-fifth of this weight. In 1894 a further step was taken by the enactment that the international units based on the metric system should be "the legal units of electrical measure in the United States."

Already, by an order approved Apr. 15, 1878, the Secretary of the Navy had directed that the metric system should be used in the

Medical Department of the Navy, and later, Apr. 13, 1894, it was ordained that all requisitions and accounting for medical supplies in the War Department should be made in conformity with the metric standard. The scope of these regulations was broadened by Executive orders of Nov. 21, 1902, providing that "for all official, medical, and pharmacal purposes, officers shall make use of the metric system of weights and measures."

In our colonies, Porto Rico and the Philippines, the metric system had been in use long before the date of our occupation, and its continued use, for Porto Rico, was made obligatory by a proclamation of the military governor, dated Mar. 18, 1899, while in the case of the Philippine Islands the continuance of its employment was legalized under Sections 3,569 and 3,570 of the Revised Statutes.

It is not only in the form of the new international carat that the metric system has found application in determining the dimensions of precious stones. Certain classes of these stones are distinguished and sorted by passing them through sieves graded according to the diameters of their apertures expressed in millimeters. The apparatus used consists of a brass ring grooved on the inner side so that there may be set within it a series of sieves. A sieve of a certain grade having been duly adjusted, the stones to be measured are placed upon it, and shaken to and fro until all smaller than the apertures of this particular sieve have passed through it. These stones are then gathered up and placed upon a sieve with smaller apertures, and those which do not pass through this latter sieve are classed by its serial number. Pearls are also sometimes classified in the same way.

Here, however, as in the case of the carat, a fixed standard has been lacking, there being Paris stone-sieves with a difference of about $\frac{1}{5}$ mm., Paris and Idar "pearl-sieves" with a progressive difference amounting to from $\frac{1}{25}$ to $\frac{1}{5}$ mm., and Bohemian garnet-sieves with approximately the same progressive difference as the Paris sieves. But this theoretical uniformity does not exist practically, for the sieves are not standardized, and those used by one dealer may and do usually differ from those used by another. Moreover, the ratio of difference between sieve and sieve is not constant. In the case of some numbers, or grades, it amounts to $\frac{1}{3}$ mm., while in the case of others it is but $\frac{1}{10}$ mm., so that a great quantity of stones will be placed in certain grades and very few in certain other grades. To obviate all these difficulties an international series of sieves has been proposed. The No. 1 sieve is to have apertures of $\frac{1}{10}$ mm., each succeeding number to have apertures $\frac{1}{10}$ mm. greater in diameter, so that in the case of the No. 10 sieve this would measure 1 mm.; No.

20, 2 mm., and so on. By this means the number of the sieve would immediately and invariably denote the diameter of the holes; in the case of No. 17, for instance, the diameter would be $1\frac{7}{10}$ mm.; for No. 36, $3\frac{6}{10}$ mm. As another improvement, it is suggested that the sieves be made as thin as possible, and that thin steel plates be substituted for brass as a material.

Countries Which Have Definitely Accepted and Legalized the Metric Carat.

Spain	1908
Japan	1909
Switzerland	1909
Italy	1910
Bulgaria	1910
Denmark	1910
Norway	1910
Holland, law promulgated Apr. 7	1911
Portugal	1911
Roumania	1911
Sweden	1911
France, adopted June 22, 1909; legalized Jan. 1	1912
Germany	1912
Belgium	1913

The following resolution was adopted at the Eighth Annual Conference on the Weights and Measures of the United States, held at the Bureau of Standards, May 14 to 17, 1913:

"*Resolved*, that this Conference is in favor of the metric carat weight of two hundred (200) milligrams being adopted as the standard of weight for precious stones."

It has been suggested that as every State having a special department of weights and measures should be properly equipped for the task of testing the accuracy of weights and balances used for precious stones, provision should be made for officially verifying such weights and balances, the owners merely needing to defray the costs of transportation, which would be inconsiderable.[19]

In a letter sent in the latter part of May, 1913, to G. E. M. Johnson, Secretary of the Decimal Association of London, some important information is conveyed by P. A. MacMahon, deputy warden of the standards of the English Board of Trade, promising the general adoption of the new metric carat in England in the near future. He writes:[20]

[19] G. A. Dazney, The Application of the Weights and Measures Question to the Jewelry Trade, *Jewelers' Circular Weekly* (Apr. 24, 1912).

[20] *Jewelers' Circular Weekly* (June 11, 1913).

"In reply to your letter of the 21st of May respecting the adoption of the metric carat in the sale of diamonds and other precious stones, I have to acquaint you that the Department has now decided to take steps to make the metric carat and its necessary multiples and submultiples standard weights in the United Kingdom, and that an Order in Council giving effect to this decision will probably be issued this year."

On July 1, 1913, the provision goes into effect in Belgium making the metric carat the standard of weight for precious stones in that country, and it is confidently expected that in Holland, where the extensive diamond interests are so nearly allied to those of Belgium, a similar provision will be very shortly enacted. It is thus probable that by 1914 the six countries chiefly interested in the diamond trade, the United States, Great Britain, Germany, France, Holland, and Belgium, will all be freed from the use of the antiquated and complicated carat-weights of the past.

In an interesting and instructive address delivered by Dr. Louis A. Fischer, of the United States Bureau of Standards, before the Retail Jewelers' Association of the District of Columbia, the speaker took occasion to assure his hearers that the jewelers of this country could count upon the assistance and support of the Bureau of Standards if they followed the example set by their European confrères.[21]

For jewelers and gem-dealers doing business in New York, a fact of considerable interest is that the Mayor's Bureau of Weights and Measures is ready, after July 1, 1913, to place its services at the disposal of the general public in correctly determining the weights of precious stones according to the carat of 200 mg. Commissioner John L. Walsh states that the mechanical department of the Bureau, at 224 West 49th Street, will then be supplied with facilities for weighing gems from $\frac{1}{100}$ carat to 500 carats, in accordance with the new standard. While no other city department is as yet able to furnish this service, the New York State Bureau of Weights and Measures, at Albany, and the National Bureau of Standards, at Washington, D. C., can verify sets of the new weights by means of the standard weights deposited there.

In conclusion, it is a pleasure to chronicle the definite official acceptance of the new carat by the United States Treasury Department. On June 17 instructions were issued by the Department to collectors of customs prescribing the metric carat of 200 mg. as the standard unit of weight for imported diamonds and other precious stones, and for pearls; these instructions to take effect July 1, 1913.

[21] In a personal letter, Mar., 1913.

The use of the new carat-weights has received the approval of the National Jewelers' Board of Trade, A. Henius, President; the National Jobbers' Association, and the American National Jewelers' Association; and each of these bodies has passed a resolution commending the metric carat and recommending that members should employ it as a standard weight after July 1, 1913.

The resolutions of the National Jewelers' Board of Trade, passed at a meeting of the Board of Directors, held Nov. 14, 1912, read as follows:

After discussion it was moved, seconded, and carried:

"That the National Jewelers' Board of Trade most heartily endorse the movement of the Committee that was formed for the purpose of the adoption of the decimal metric carat, and recommend to its full membership the adoption thereof, in accordance with the Resolution passed at the meeting held October 29th, and that the President be authorized to appoint ten members of this Board to co-operate with the Committee to be appointed by Mr. Rothschild, and that the Secretary be instructed to notify every member of the Board of the action taken here to-day; that the President or the Secretary communicate the action of the Board to the proper officials of the National Retail Jewelers' Association and National Jobbers' Association, requesting them also to use their influence for the decimal metric carat system."

The following letters and telegrams register the final success of the international metric carat in our country:

(*Copy.*)
DEPARTMENT OF COMMERCE AND LABOR,
BUREAU OF STANDARDS.
Washington.

June 13, 1913.

Dr. George F. Kunz,
405 Fifth Avenue,
New York City.

MY DEAR DR. KUNZ:

I beg to acknowledge receipt of your letter of June 12, regarding the adoption of the metric carat. In reply, I would state that the order the Treasury Department intends to issue regarding the metric carat was submitted to the Department of Commerce for comment and criticism and has been passed upon by this Bureau. It is our

understanding that the Treasury Department will issue this order to take effect on and after July 1st, 1913.

With kindest regards, I remain,

Sincerely yours,

(Signed) S. W. STRATTON,

Director.

(*Copy of Telegram.*)

WASHINGTON, D. C., June 18th, 1913.

Dr. George F. Kunz,

405 Fifth Avenue, New York.

Notice of the Treasury Department concerning adoption of Metric Tariff will be promulgated July first, according to word received from Treasury Department to-day. You are therefore at liberty to publish Bureau letter.

S. W. STRATTON.

(*Copy.*)

TREASURY DEPARTMENT,
Washington.

June 16, 1913.

Mr. George F. Kunz,
405 Fifth Avenue,
New York.

DEAR SIR:

Replying to your letter of June 12, asking if the Treasury Department will adopt or reject the new international carat of 200 metric milligrams, on July 1, I am very glad to inform you that the Department has adopted the new international carat, beginning July 1, and collectors of customs have been so informed.

Respectfully,

JAMES F. CURTIS,
Assistant Secretary.

(*Copy.*)

French Telegraph Cable.

PARIS, June 20, 1913.

George F. Kunz,
409 Fifth Avenue, New York.

Merci telegram communique a confreres accueillez felicitations cordiales pour consecration oeuvre a laquelle avez donne tout votre devouement.

LÉON RHEIMS.

The value of pearls is computed by squaring the number of "pearl grains" (quarter-carats) and multiplying the product by a figure determined upon as the base value of the grain. To ascertain the exact value of a pearl having a certain weight in metric carats, we should first multiply the figures by four, which will give us the number of "metric pearl grains," and then multiply this product by itself, the result being in turn multiplied by the figure representing the value in dollars of a "base-grain." For example, in the case of a pearl weighing 3.64 metric carats and having a base-grain value of $6, we proceed as follows:

$$3.64 \times 4 = 14.56 \text{ (number of "pearl grains").}$$
$$14.56 \times 14.56 = 211.9936.$$
$$211.9936 \times 6 = 1271.9616 \text{ (value of pearl in dollars).}$$

That is to say, such a pearl would be worth $1,271.96. Several books have been published containing the squares and cubes, and square-roots and cube-roots, of a long series of numbers, and any one of them would prove useful in these operations.[22]

In addition to the jewelers already noted, no one has been more indefatigable in the introduction of the international carat of 200 mg. than Mons. Ch. Ed. Guillaume, Director of the Bureau Internationale des Poids et Mesures of Sèvres; Mons. Léon Rheims, President Chambre Syndicale des Négociants en Diamants, Perles, Pierres Précieuses et des Lapidaires of Paris; and in this country, Dr. S. W. Stratton, Director of the Bureau of Standards, Department of Commerce and Labor, and Hon. James F. Curtis, Assistant Secretary of the United States Treasury.

POSTSCRIPT.

Since the presentation of the foregoing paper additional steps in the adoption of the new metric carat have been recorded, as follows:

The adoption of the metric carat will form part of the new regulations on the metric system to be introduced shortly in the Argentine Republic, this new legislation having been delayed to await the results arrived at in France, where the Minister of Commerce is preparing a systematization of the French law.

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[22] *Barlow's Tables of Squares, Cubes, Square-Roots, Cube-Roots, Reciprocals of all integral numbers up to 10,000* (London, 1882).

Dr. A. L. Crelle's *Rechentafeln* (Berlin, 1889).

W. Palin Elderton, *Tables of Powers of Natural Numbers; in Biometrika*, vol. ii. and iv., pp. 474–480 (Cambridge, Nov., 1903).

In Austria a law has been introduced in the Chambers providing for the adoption of the new carat.

The law determining the value of the new carat was promulgated in Belgium Mar. 10, 1913. A royal decree of Oct. 31, 1913, made it compulsory for all computations of diamond weights to be expressed in decimals of the new metric carat.

The Italian law adopting the carat, promulgated Jan. 7, 1910, left the date of its definite establishment to a Royal Decree; this was issued Aug. 9, 1911, and sets the date of Jan. 1, 1912, for the official introduction of the new carat in commercial transactions.

The definite legalization of the metric carat in England was brought about by an Order in Council of Oct. 14, 1913, in part as follows:

"Now, therefore, His Majesty, by virtue of the power vested in him by the said Acts, by and with the advice of the Privy Council is pleased to approve of the new denominations of standard weights specified in the schedule hereto, and doth direct that the same, on and after the 1st day of April, 1914, shall be Board of Trade standards in like manner as they were mentioned in the Second Schedule of the Weights and Measures Act, 1878."

The U. S. Bureau of Standards issued on Nov. 1, 1913, a special circular (No. 43) devoted to the metric carat. The circular gives a number of tables showing the equivalents of fractions of the old carat (2.053 mg.) in decimals of the metric carat of 200 mg., and *vice versa*. A section is devoted to the details of weighing and weights, offering some valuable suggestions in relation to the treatment of balance and scales and the proper conformation and handling of the new carat weights.

DISCUSSION.

Discussions of papers contained in this volume have been placed immediately after the respective papers with the exception of the one here printed.

Rock-Drilling Economics.

Discussion of the paper of W. L. Saunders, p. 147.

LUCIEN EATON, Ishpeming, Mich. (communication to the Secretary *):—I began to use water Leyner drills at the Cliffs Shaft mine at Ishpeming, Mich., over four years ago, and now have over 20 in use. The ore in this mine is hard specular hematite, and is mined by the room-and-pillar system, the rooms being driven as breast stopes. For many years only 3¼-in. piston drills were used in the mine, each machine ordinarily being able to drill from 15 to 25 ft. of holes in a shift. In the hardest ground, however, it was often impossible to drill more than 2 or 3 ft. in a shift. When water Leyner drills were used the footage drilled was materially increased, especially in the hardest ground, but it was found that in very hard ground the breakage of machine parts, and especially of drill steel, was very high. At this time round steel with lugs forged on the shanks was used, and most of the breakage occurred at or near the lugs. Many experiments were made in an endeavor to overcome this breakage, and it was finally decided to do away with the lugs altogether, and use hexagonal steel. Accordingly I designed a chuck to use hexagonal steel without lugs, a tappet being interposed between the hammer and the shank of the drill steel.

The design of this chuck of the proper size for the No. 18 Ingersoll-Leyner drill is shown in Fig. 1. The different parts are also shown photographed in Fig. 2. *A* is the barrel of the chuck, which revolves inside the front head. It is made of machinery steel. *B* is the bushing which screws into the front end of the barrel *A*, and in which the drill steel fits loosely. This is turned down from a steel casting, the hexagonal hole being cast and then broached out to the proper size. It is case hardened inside. *C* is the tappet, which fits smoothly inside *A* just behind the bushing *B*. It is made of "N" temper Crescent tool steel and is carefully tempered. *D* is the tappet ring, which prevents the tappet from being pushed back too far by the

* Received Dec. 12, 1913.

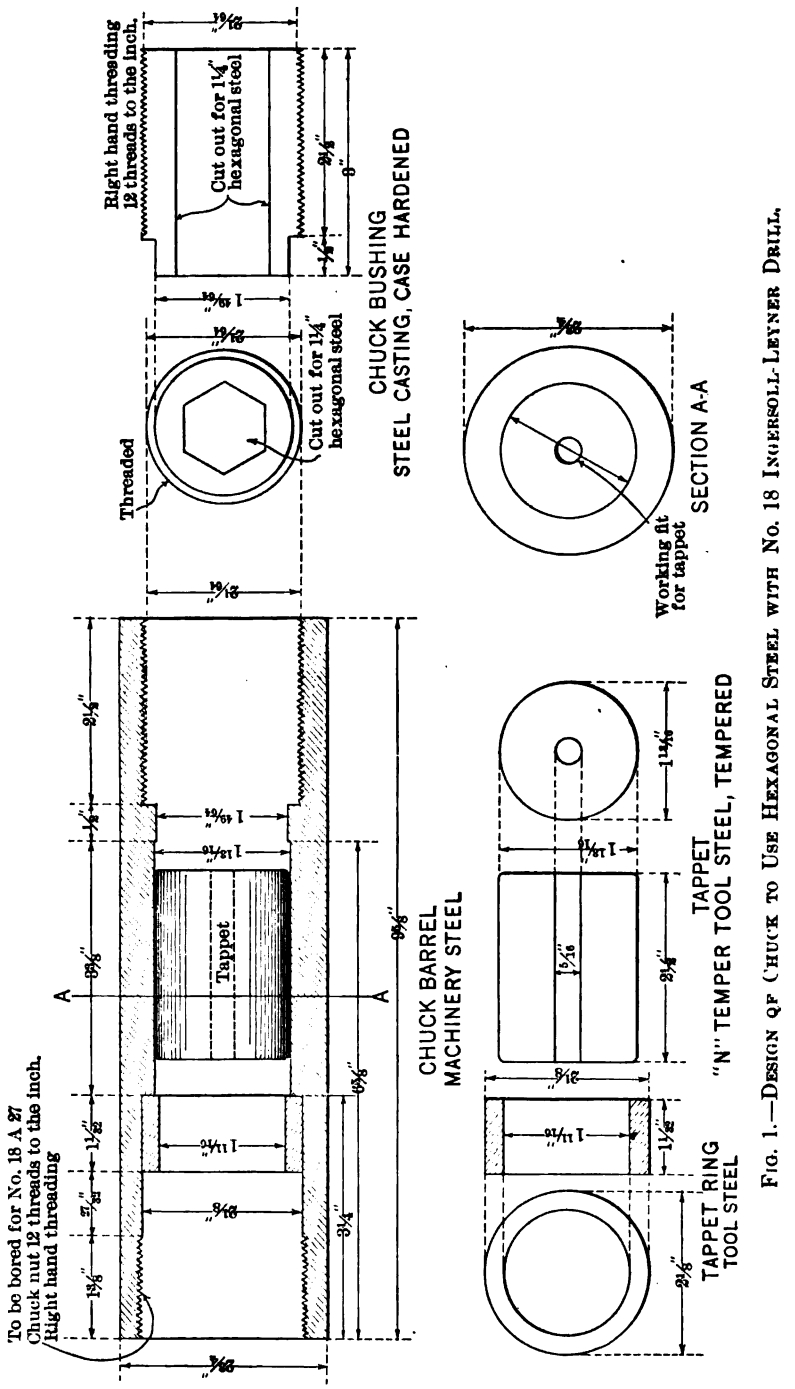

FIG. 1.—DESIGN OF CHUCK TO USE HEXAGONAL STEEL WITH No. 18 INGERSOLL-LEYNER DRILL.

drill steel. It fits snugly in the barrel, and is held firmly against a shoulder by the rotating nut *E*. This is the regular stock rotating nut, furnished by the Ingersoll-Rand Co. with all No. 18 machines. The only change in the other parts of the machine made necessary by the substitution of this chuck for that used with round steel is in the water tube, which is a little longer.

By the use of this chuck unexpected results were obtained. A few tests, made with the same machine using chucks for round and hexagonal steel alternately, showed little difference in efficiency, the chuck for hexagonal steel making a little the better showing. The principal improvement was in breakage of steel. Whereas with the

FIG. 2.—SEPARATE PARTS OF CHUCK SHOWN IN FIG. 1.

round steel the number of pieces broken per month had averaged nearly 30 per cent. of those in use, with the hexagonal steel the number of pieces broken per month, as shown by a test involving all the steel used by 14 machines for a period of eight months, was only 1 per cent. of those in use. In addition, repair charges on the drilling machines were cut almost in half, because with the new chucks the miners did not let the hammer pound on the front head in "backing off" the machine, as is the almost universal custom where lug steel is used. It was found, also, that by using a slightly larger amount of water before changing steel no trouble was experienced

with drills getting stuck in the hole. In fact, only one piece of steel has been so stuck during the last 18 months.

This chuck has been in general and successful use at the Cliffs Shaft mine for over 18 months, and the design is offered freely to any who wish to make use of it.

W. L. SAUNDERS (communication to the Secretary *):—The advantages claimed by Mr. Eaton for his chuck are:

1. It eliminates the necessity of shanking the steel with either a collar or the lugs, as used with the standard Leyner chuck.

2. Steel breakage was reduced from approximately 30 per cent. of steel in service to about 1 per cent.

3. Repair cost on drills was cut about in half.

There is no doubt that with the mining and rock conditions in the Cleveland Cliffs mine, in which Mr. Eaton is Superintendent, this type of chuck has advantages over the standard Leyner chuck. However, for certain mining conditions it has the disadvantage of inability to withdraw the steel from the hole while in the chuck of the drill.

There are certain classes of rock where this feature is absolutely essential in driving flat or down holes to any depth. For this reason it can hardly be considered that Mr. Eaton's chuck could be universally adopted as a standard, unless some form of efficient steel extractor is added to it. In all medium or soft ground, especially where it is interposed with seams of hard material, causing the hole to rifle or run, the extracting principle of the standard Leyner chuck is imperative to the success of the machine.

With Mr. Eaton's type of chuck, the reduction in steel breakage and the saving in cost of rock-drill repairs are probably due to the fact that no steel-extracting device is provided for—this apparently not being necessary in the rock encountered in his mines.

* Received Mar. 19, 1914.

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